When Growth Targets Are Missed: Carbon Emissions and Local Regulatory Slack

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Abstract: Governments often pursue economic and environmental objectives simultaneously, but the incentives attached to these objectives are not equally salient. This paper examines whether shortfalls from local economic growth targets weaken effective environmental governance and increase firms' carbon-emissions intensity. We argue that growth shortfalls induce local officials to reallocate administrative attention and enforcement effort toward growth stabilization. Because carbon regulation is partly implemented through local discretion, this reallocation relaxes effective environmental stringency and allows firms to operate more carbon-intensively. Using Chinese prefecture-level growth targets, we find that firms located in cities experiencing growth pressure subsequently exhibit higher carbon emissions intensity. The effect is stronger where officials have higher ex ante promotion incentives, where growth targets are hard constraints, and where institutions provide greater room for discretionary enforcement. It is concentrated among state-owned and politically connected firms, but is muted when top-down environmental oversight constrains local discretion. Mechanism tests show that growth pressure lowers local governments' carbon-reduction priority and weakens environmental enforcement. Exploiting the U.S.-China trade war as an external demand shock, we show that tariff-exposed cities face greater growth pressure and experience larger subsequent increases in firm carbon intensity. The findings identify a state-contingent enforcement channel through which target-based growth management generates unintended climate externalities.

Keywords: Environmental regulation; Economic growth targets; Carbon emissions; Political incentives; Multi-task governance; Trade shocks

JEL codes: Q53, Q58, D73, H77, F13

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1. Introduction

Governments rarely pursue a single objective. Instead, they manage portfolios of goals, growth, employment, fiscal stability, and environmental quality, whose measurement, political salience, and timing differ sharply. Output and employment are observed at high frequency, widely reported, and quickly interpreted as indicators of competence. Environmental outcomes, by contrast, are often noisier, harder to attribute, and realized over longer horizons. These asymmetries create a classic multitask governance problem: when the same agents are responsible for multiple objectives, effort can shift toward dimensions that are more visible or more strongly rewarded, potentially at the expense of less salient tasks (Holmstrom & Milgrom, 1991).

This paper studies a specific manifestation of that trade-off: when local governments fall behind their growth targets, do they loosen effective environmental governance? The question is economically important for two reasons. First, it speaks directly to the credibility of climate policy in settings where implementation is decentralized and enforcement is locally discretionary. Second, it links short-run stabilization pressures, arguably unavoidable in target-based governance systems, to long-run climate externalities, suggesting that macro shocks can propagate into emissions through political-institutional channels rather than purely technological or compositional shifts.

We focus on China, where local governments announce annual growth targets and are widely viewed as facing strong career concerns around economic performance.[1] A large public-finance and political-economy literature documents that promotion and turnover of local leaders are correlated with economic outcomes, consistent with high-powered incentives for growth delivery (Maskin et al., 2000; Chen et al., 2005; Li & Zhou, 2005). At the same time, environmental governance in China, while consequential, has historically displayed substantial variation across jurisdictions and time, reflecting differences in capacity, enforcement intensity, local political support, and higher-level oversight (Canfa, 2006; Chan et al., 1995; Chen et al., 2013; Kahn et al., 2015; Zhan et al., 2014). Public administration research has long documented an implementation gap in China’s environmental governance, where centrally mandated environmental objectives are often weakened during local implementation, especially when local agencies lack political support, administrative authority, or cooperation from other governmental units (Chan et al., 1995; Zhan et al., 2014). More broadly, evidence from multiple

[1] To underscore that such growth-environment tension is not unique to China, and any single country, Appendix Table A1 documents cross-country descriptive patterns. Combining the Environmental Performance Index (EPI) reports released by Yale and Columbia with the World Bank’s Adjusted Net National Income (ANNI), we show that environmental performance often deteriorates around adverse growth shocks. The message is clear: the growth-environment trade-off is visible across a wide range of countries, including advanced economies with strong regulatory institutions. These descriptive facts provide macro-level context for our central mechanism: under multiple policy objectives, a shift in emphasis toward growth can mechanically relax environmental governance.

contexts shows that environmental regulation can be effective, but its effectiveness depends critically on institutional design and enforcement incentives (Duflo et al., 2013; Greenstone & Hanna, 2014).

Our key empirical idea is to measure growth pressure at the moment it becomes salient to local officials: when realized GDP growth in the first half of a year falls below the annual target set at the beginning of the year. This first-half shortfall captures the point at which officials learn the target is at risk and must decide how to respond.

A central challenge in identifying regulatory slack is distinguishing selective relaxation from a broad-based deterioration in environmental governance. If growth pressure simply weakens all environmental enforcement, one should observe increases across conventional pollutants that are directly monitored and politically salient. We do not. As a first diagnostic, we show that growth pressure has little effect on traditional pollutants such as sulfur dioxide, nitrogen oxides, industrial wastewater, and smoke/dust emissions. These pollutants are subject to relatively hard regulatory constraints and real-time monitoring, leaving local governments limited room for discretionary relaxation. This null result is informative: it suggests that the adjustment margin is not general environmental neglect, but selective slack in less directly binding climate-related dimensions.

We then turn to city-level CO_2 emissions and show that growth pressure raises aggregate carbon emissions, with the increase concentrated in the industrial sector. This sectoral pattern is crucial for the paper's empirical strategy. Industrial emissions are the margin most directly shaped by local production stabilization, project approvals, inspection intensity, and government – firm interactions. The city-sector evidence therefore motivates our main firm-level analysis: if growth pressure operates through discretionary accommodation of production, its consequences should be visible among firms whose operations are embedded in local regulatory and political relationships.

Because substantive adjustments such as regulatory enforcement, investment approvals, credit facilitation, and firms' production plans, typically materialize with lags, our baseline design examines how pressure in a specific year affects firms' carbon-emissions intensity in the next following year. We document a robust pattern: growth-target shortfalls are followed by higher firm-level carbon intensity, and the effect persists for up to two years. We then show that the effect is systematically stronger when political incentives to prioritize growth are more intense. Specifically, the increase in emissions is concentrated in jurisdictions led by officials with high ex ante promotion incentives, constructed from predetermined characteristics at the start of tenure, and in cities that set hard growth targets using binding language in official reports. In contrast, the effect is muted in regions with stronger market institutions and higher administrative efficiency, and it disappears in settings where local discretion is constrained, such as heavily polluting industries or during periods of central environmental inspection.

The mechanism we emphasize is not that governments explicitly abandon environmental objectives. Instead, we argue that effective stringency can be adjusted through discretionary

implementation margins that are difficult to observe contemporaneously, such as inspection frequency, enforcement selectivity, and informal guidance about compliance priorities. When growth underperforms, officials face heightened incentives to stabilize economic activity. Under multitask incentives, that can crowd out environmental enforcement effort, allowing carbon-intensive production to expand or persist.

Beyond average effects, we investigate who bears the environmental cost of growth-target shortfalls. The increase in emissions is disproportionately driven by state-owned enterprises and politically connected firms, consistent with selective regulatory relaxation: under growth pressure, local governments appear more willing (or able) to loosen enforcement for firms that are institutionally embedded in local governance structures or enjoy privileged access to regulatory discretion. This incidence result highlights that the growth-environment trade-off is not uniform across firms, but mediated by government-business relationships and enforcement selectivity.

Finally, to clarify the operational logic of such implicit guidance, we conduct a textual analysis of local government work reports. We further show that growth pressure reduces the emphasis on carbon reduction in government communication and weakens environmental enforcement. This divergence between rhetoric and implementation is consistent with a signaling and image-repair mechanism: officials under growth pressure substitute symbolic policy language for substantive regulatory effort.

A natural concern is that growth pressure may correlate with unobserved local shocks that directly affect firms' energy use or technology choices. To strengthen causal interpretation, we exploit the 2018-2019 US-China trade war as a plausibly exogenous negative demand shock that differentially affected cities based on pre-war export exposure. The first stage is that more exposed cities experience larger increases in growth pressure. The second stage is that this induced pressure predicts larger increases in firms' carbon intensity. This pattern is consistent with a political-economy transmission mechanism: adverse demand shocks tighten local growth constraints and thereby increase incentives to accommodate production through laxer effective enforcement (Shapiro & Walker, 2018).

This paper contributes to the literature in several aspects. First, we contribute to the economics of incentives and public-sector organization by documenting a concrete multitask distortion in decentralized governance: growth-target shortfalls are followed by higher firm-level carbon intensity, consistent with state-contingent shifts in effective enforcement (Nordhaus, 1975). The result operationalizes a central prediction of multitask incentive theory, when performance is evaluated on multiple dimensions with unequal observability and penalties, agents reallocate effort toward the more salient task (Holmstrom & Milgrom, 1991). Second, we advance the political economy literature on China's cadre incentives by showing that the growth-oriented incentive system has measurable environmental spillovers. A broad body of evidence links officials' career outcomes to economic performance, supporting the view that local leaders face strong growth delivery incentives (Li & Zhou, 2005). We connect these incentives to climate-relevant outcomes at the firm level, complementing work

showing that changes in promotion criteria can alter environmental performance (Kahn et al., 2015; Xie & Yuan, 2023). Our contribution is to identify a shortfall state in which the same incentive system predicts environmental slack rather than improvement.

Third, we contribute to the environmental economics literature on regulation and enforcement by highlighting an under-emphasized margin: state-contingent implementation. This emphasis also responds to public administration scholarship calling for implementation research that moves beyond static accounts of formal policy design and examines how implementation unfolds across multiple actors and changing contexts (O'Toole, 2000). We extend this perspective by identifying a state-contingent implementation channel: when local growth targets are at risk, officials shift administrative attention and enforcement effort toward growth stabilization, weakening effective environmental stringency. Regulation and enforcement can generate large real environmental gains (Greenstone & Hanna, 2014), and that strengthening monitoring/auditing can change firm behavior when incentives are aligned (Duflo et al., 2013). Our results add that even when formal policy exists, enforcement intensity may become pro-cyclical with local growth pressure, an implementation channel that matters for understanding why realized environmental outcomes can diverge from stated objectives.

Fourth, we add a political-economy mechanism to the trade–environment literature by showing that adverse demand shocks can propagate into emissions through local governance constraints. While foundational work emphasizes how trade affects pollution via specialization and policy differences (Copeland & Taylor, 1994; Bombardini & Li, 2020), and influential evidence for the US highlights regulation as a key driver of declining manufacturing pollution intensity (Shapiro & Walker, 2018), we show that trade shocks can also shift emissions by tightening growth constraints and thereby altering enforcement priorities in a target-based governance system.

Fifth, we contribute to the growing finance and strategy interface on carbon exposure by identifying a policy-driven determinant of firm-level carbon intensity that is plausibly orthogonal to firms' private optimization alone. Recent asset-pricing evidence shows that carbon risk is priced in equity returns (Bolton & Kacperczyk, 2021) and that climate-related downside risk is economically meaningful (Ilhan et al., 2021). By linking carbon intensity to local growth-target pressure, we provide a governance-based source of variation that can help interpret the time-series and cross-sectional behavior of carbon risk measures used in finance and corporate strategy.

Our findings imply that climate policy effectiveness depends not only on formal rules but also on the incentive architecture and discretionary capacity of the officials who implement them, especially when growth targets structure political attention. In particular, if enforcement slack becomes a de facto stabilizer after growth shortfalls, environmental governance is likely to be most fragile precisely during downturns, suggesting the need to stress-test regulatory systems for adverse macro conditions and to build automatic stabilizers for enforcement (e.g., minimum inspection intensity requirements, transparent enforcement dashboards, and third-party auditing that raises verifiability and reduces

discretion). More broadly, target-based systems can generate predictable multitask distortions when one dimension (growth) is monitored and sanctioned more sharply than another (environment), so a central design priority is to rebalance observability and accountability by making environmental performance metrics more continuous, comparable, and harder to game, and by tying them to stable career and budget incentives. Finally, because the pressure-emissions link weakens under stronger top-down oversight, higher-level inspections and monitoring can be deployed strategically and countercyclically, intensifying precisely when growth pressure is high and local discretion is most likely to be used to relax effective enforcement, thereby improving the credibility of long-run climate commitments.

2. Institutional Background and Conceptual Framework

2.1 Target-based governance and the salience of growth shortfalls

China's local governments announce annual GDP growth targets in government work reports. These targets are not merely forecasts: they function as focal commitments that discipline subordinate agencies, coordinate investment and credit support, and provide a salient benchmark for evaluating local leadership. A central feature of China's cadre evaluation system is that measured economic performance, especially growth, has historically carried high weight in officials' career progression, generating tournament-style incentives to deliver observable output outcomes (Li & Zhou, 2005). Relatedly, political-economy models of accountability predict that when performance is benchmarked on short-horizon, easily measured indicators, governments overweight those indicators relative to longer-horizon objectives (Nordhaus, 1975; Rogoff, 1990). In this institutional context, missing a growth target is particularly costly because it is simple to communicate up the hierarchy, easy to compare across jurisdictions, and difficult to explain away as measurement noise. At the same time, China's decentralized implementation system creates room for selective policy implementation: local officials may prioritize some centrally mandated tasks over others depending on local incentives, political constraints, and available resources (O'Brien & Li, 1999). This institutional feature is central to our argument because growth shortfalls do not necessarily change formal environmental rules; instead, they alter how local officials prioritize and implement those rules.

Recent work further emphasizes that growth targeting in China is intertwined with macroeconomic management: when regions fall behind targets, policy responses often intensify in ways that are asymmetric across the cycle (e.g., stronger interventions in slowdowns than in booms), reinforcing the idea that targets operate as constraints rather than aspirations.

2.2 Environmental regulation as a discretionary instrument

Environmental objectives are prominent in central directives, but day-to-day implementation is largely local. Two features make environmental governance an especially plausible margin of adjustment when growth is at risk.

First, environmental outcomes are often multi-dimensional and harder to attribute in real time. While GDP growth is frequently reported and comparable, emissions and compliance are noisy and shaped by firm-level processes. Second, enforcement is discretionary: effective stringency depends on inspections, monitoring intensity, and sanctioning rather than only on formal rules. Evidence from other settings shows that increasing monitoring and credible auditing can substantially change firms' pollution behavior, underscoring that enforcement, rather than statutory standards alone, often binds.

China's own governance architecture exhibits this logic: episodic, top-down oversight campaigns (e.g., central inspections) raise the expected costs of weak local enforcement by increasing detection probability and sanction severity. This implies that when oversight is tight, local discretion over effective environmental stringency is reduced, limiting the extent to which environmental governance can be relaxed in response to growth pressure.

2.3 Conceptual framework: multitask governance with asymmetric penalties

When policy mandates are politically contested or locally costly, implementation outcomes are shaped not only by formal directives but also by the interaction between policy ambiguity, political conflict, and local discretion (Matland, 1995). In our context, growth targets and environmental mandates create competing demands for local officials: the former are highly visible and immediately evaluated, whereas the latter are implemented through more discretionary and less continuously monitored enforcement activities. We formalize these ideas using a simple but intuitive multitask principal-agent framework (Holmstrom & Milgrom, 1991; Wei et al., 2023). A local official allocates limited political or administrative effort across two tasks: growth task and environment task. Growth task includes actions that raise measured economic performance (e.g., investment acceleration, credit facilitation, administrative streamlining. In contrast, environment task represents actions that reduce emissions intensity (e.g., inspection effort, enforcement stringency, coordination costs imposed on polluters). Let e_g denote effort allocated to growth-oriented governance and e_e denote effort allocated to environmental governance. Effort is costly and partially substitutable:

$$C(e_g, e_e) = \tfrac{1}{2}(e_g^2 + e_e^2) + \phi e_g e_e, \quad \phi > 0$$

Measured performance in each task is increasing in effort and subject to noise:

$$y_g = a_g e_g + \eta_g, \qquad y_e = a_e e_e + \eta_e$$

The official's payoff reflects (i) returns to measured performance and (ii) asymmetric penalties. Growth shortfalls relative to an announced target g^* are salient and continuously benchmarked, whereas

environmental underperformance is disciplined more episodically through detection and sanction:

$$U = \theta_g y_g + \theta_e y_e - C(e_g, e_e) - \kappa \cdot \max(0, g^* - y_g) - \Pr(\text{sanction} \mid e_e) \cdot L$$

Two institutional asymmetries matter. First, growth target failure is a sharp, high-visibility event. When $y_g < g^*$, the marginal value of increasing y_g rises by κ. Second, environmental enforcement is discretionary and oversight-dependent. The expected cost of weak enforcement depends on Pr(sanction $\mid e_e) \cdot L$, which varies with monitoring intensity, institutional capacity, and central oversight.

Proposition 1 (Pressure-induced enforcement reallocation). When interim information indicates that y_g is likely to fall below g^* (i.e., a growth shortfall becomes salient), the official optimally increases e_g. With $\phi > 0$, this increases the marginal cost of e_e, leading to a reduction in e_e unless the expected marginal penalty from weak environmental governance is sufficiently large. Because firm carbon intensity CI decreases in effective environmental governance e_e, growth pressure increases firms' carbon intensity in the subsequent period.

Each hypothesis follows as a comparative static of Proposition 1 with respect to the key parameters that govern (i) the marginal penalty on growth shortfalls κ, (ii) the weight on growth performance θ_g, and (iii) the expected enforcement penalty Pr(sanction $\mid e_e) \cdot L$.

Corollary 1 (Baseline effect). A shortfall activates the κ term, raising the marginal payoff to e_g. With substitutability ($\phi > 0$), higher e_g crowds out e_e, lowering effective environmental governance and increasing CI with a lag due to implementation and production adjustment frictions.

H1: Growth shortfall increases firms' carbon-emissions intensity in the subsequent year.

Corollary 2 (Career incentives amplify the response). Higher promotion incentives increase the effective payoff from measured growth performance (higher θ_g) and/or increase the perceived penalty of missing targets (higher effective κ). Either channel makes the shortfall-induced increase in e_g larger, deepening the crowd-out of e_e, and raising CI more.

H2: The effect in H1 is stronger when officials face higher ex ante promotion incentives.

Corollary 3 (Hard targets increase the marginal penalty of shortfall). Hard targets increase the marginal career cost of failure (higher κ). With a larger κ, the official's optimal response to a shortfall is a stronger shift toward e_g, implying a larger reduction in e_e and a larger increase in CI.

H3: The effect in H1 is stronger in cities that set hard (binding-language) growth targets.

Corollary 4 (Weaker institutions increase feasible discretion). Weak institutions reduce monitoring and accountability, lowering Pr(sanction $\mid e_e$) and/or L. This reduces the expected marginal cost of cutting e_e, making enforcement slack a cheaper margin of adjustment under growth pressure, thereby strengthening the positive effect on CI.

H4: The effect in H1 is stronger where institutions are weaker.

Corollary 5 (Selective slack via embeddedness). When enforcement is discretionary, the reduction in e_e need not be uniform. Officials can more effectively translate accommodation into growth stabilization by relaxing constraints for firms that are administratively responsive, politically embedded, or strategically important, e.g. SOEs and connected firms.

H5: The effect in H1 is stronger among SOEs and politically connected firms.

Corollary 6 (Top-down oversight limits the trade-off). Stronger oversight increases Pr(sanction | e_e) and/or L, raising the expected marginal penalty of reducing e_e. This dampens the optimal crowd-out of environmental governance under growth pressure, attenuating the effect on CI.

H6: The effect in H1 weakens or disappears when top-down oversight tightens or where polluters are more visible and easier to monitor.

In summary, in our multitask governance framework, local officials allocate scarce administrative capacity between delivering short-run growth and enforcing environmental regulation, and these efforts are substitutes because time, attention, and enforcement resources cannot be expanded quickly. A growth shortfall relative to an announced target raises the marginal career cost of missing the target, making growth-oriented actions more valuable at the margin and inducing officials to shift effort toward growth stabilization. Because environmental enforcement is comparatively more discretionary and often punished less continuously, depending instead on episodic monitoring and oversight, this reallocation reduces effective environmental stringency, allowing firms to rely more on carbon-intensive production and thereby increasing carbon-emissions intensity with a lag. The same logic yields sharp comparative statics: the pressure–carbon link is stronger when the shortfall penalty is larger (hard targets), when career concerns heighten the return to growth performance (high promotion incentives), and when institutions are weaker and discretion is greater; conversely, it attenuates or disappears when top-down oversight raises the expected cost of enforcement slack (e.g., central inspections) and is concentrated among firms most exposed to selective accommodation (SOEs and politically connected firms).

3. Data and Empirical Strategy

3.1 Growth Pressure

We define *Growth Pressure* as an indicator equal to one if a city's first-half realized GDP growth falls below its announced annual target. This semiannual construction is designed to capture the point in the year when officials receive credible information that the target is at risk, which is when growth incentives become most salient. In the baseline analysis, we use the one-year lag to reflect the lag between (i) learning about a shortfall and reorienting governance priorities and (ii) firms' operational responses that translate into measured emissions outcomes. We also use realized GDP growth in the third quarter of a year as a robustness. Supplementary descriptive evidence on the temporal and spatial distribution of local growth pressure is reported in Appendix Figures A5 and A6.

3.2 Environmental Outcomes

We construct environmental outcomes at three complementary levels. First, we use city-level traditional industrial pollution indicators to examine whether local growth pressure is associated with a broad relaxation of conventional environmental regulation. Second, we use city-level sectoral CO_2 emissions from the Multi-resolution Emission Inventory for China (MEIC) to identify the aggregate and sectoral margins through which growth pressure affects carbon emissions. Third, we use firm-level carbon-emissions intensity as the main outcome to examine whether the city-level industrial carbon response is reflected in firms' production and emissions behavior.

This layered outcome structure is important for the paper's empirical design. Traditional pollutants provide a benchmark for hard environmental constraints, because they are more directly monitored and more closely tied to established environmental accountability systems. MEIC sectoral CO_2 emissions allow us to examine whether the effect of growth pressure is concentrated in production-related sectors rather than household or transportation emissions. Firm-level carbon intensity then allows us to test whether the industrial-sector response is transmitted through firms that are directly exposed to local regulatory implementation and government–firm interactions.

3.2.1 Traditional Industrial Pollution Emissions

We first collect city-level traditional industrial pollution indicators as benchmark environmental outcomes. These variables include air quality, industrial waste gas emissions, industrial wastewater discharge, industrial smoke and dust emissions, sulfur dioxide emissions, and nitrogen dioxide emissions. These pollutants are closely associated with China's conventional environmental regulation system and are subject to relatively mature monitoring, reporting, and enforcement arrangements.

The purpose of using these variables is diagnostic. If local growth pressure induced a broad-based relaxation of environmental governance, one would expect traditional industrial pollutants to increase as well. In contrast, if growth pressure mainly affects less directly binding climate-related margins, traditional pollutants may remain insensitive to growth pressure. We therefore use these conventional pollution indicators to distinguish general environmental deregulation from selective relaxation along the carbon-emissions margin.

3.2.2 City-Level Sectoral CO2 Emissions from MEIC

To examine the carbon-emissions margin, we use sectoral CO_2 emissions from the Multi-resolution Emission Inventory for China (MEIC). MEIC provides gridded CO_2 emissions at a 0.25-degree spatial resolution. We aggregate the gridded emissions data to prefecture-level city boundaries using an area-weighted allocation method. Specifically, for each grid cell that overlaps with a city, we allocate the grid-cell emissions to the city in proportion to the share of the grid-cell area that falls within the city boundary. City-level emissions are then obtained by summing the allocated emissions across all grid cells intersecting the city.

Formally, city-level CO_2 emissions are constructed as follows:

$$\text{City emissions} = \sum_{g} (\text{Grid emissions}_{g} \times \text{Share}_{g,c})$$

where *Shareg,c* is the fraction of grid cell *g* area that overlaps with city *c*. This procedure accounts for grid cells fully contained within a city, grid cells partially overlapping with a city, and grid cells crossing multiple city boundaries. In the latter case, emissions from the same grid cell are split across cities according to their respective area shares.

We construct total CO_2 emissions and sector-specific CO_2 emissions for industry, power generation, residential use, and transportation. Industrial emissions include manufacturing and construction activities. Power-sector emissions correspond to electricity and heat generation. Residential emissions capture household and commercial energy use. Transportation emissions capture energy-related emissions from the transport sector. Agricultural emissions are excluded because the MEIC inventory reports zero values for that sector throughout the sample period, leaving no meaningful variation for estimation.

The sectoral structure of MEIC is central to our empirical strategy. It allows us to determine whether growth pressure increases carbon emissions through production-related sectors or through broader changes in household and transportation energy use. This distinction is important because an industrial-sector response is more consistent with local governments relaxing production-related constraints, accelerating project approvals, reducing inspection intensity, or otherwise accommodating firms under growth pressure.

3.2.3 Carbon Intensity

Our empirical analysis links firm-level carbon emissions to city-level growth targets and macro conditions, covering 2012-2022. The unit of observation is a firm-year, with each firm matched to the prefecture-level city in which it is registered (and thus primarily regulated). We begin from the universe of publicly listed firms in mainland China with non-missing emissions and accounting data, then apply standard cleaning steps: (i) drop observations with missing key variables, (ii) remove duplicate or inconsistent firm identifiers, and (iii) winsorize continuous variables to limit the influence of extreme outliers. The baseline sample contains 17,564 firm-year observations.

Our main outcome is firm carbon-emissions intensity (*Carbon Intensity*), scaling emissions by revenue. It captures carbon per unit of economic activity and improves comparability across firms of different size, consistent with a growing literature using firm-level carbon measures (Bolton & Kacperczyk, 2021; Ehlers et al., 2022). To ensure the results are not driven by a specific normalization, we also report robustness checks using alternative scalings (e.g., emissions-to-assets) in the robustness section. Appendix Table A2 lists the variable definitions and Table A3 reports summary statistics for

the key variables.

Scope 1 refers to direct emissions from sources owned or controlled by the firm (e.g., combustion from boilers, furnaces, and vehicles, as well as process-related emissions), while Scope 2 captures indirect emissions from the consumption of purchased electricity and heat. Scope 3 includes other indirect emissions along the value chain. Although Scope 3 emissions provide a more comprehensive measure, they are subject to substantial reporting discretion and lack standardized accounting practices, which may introduce significant measurement error. Therefore, Scope 3 emissions are excluded from the baseline analysis.

Given that Scope 1 and Scope 2 emissions are subject to mandatory disclosure and are more consistently reported across firms, we define firm-level carbon emissions as the sum of Scope 1 and Scope 2 emissions:

$$CE_{it} = Scope1_{it} + Scope2_{it}$$

To control for firm size heterogeneity, we construct carbon intensity as emissions per unit of operating revenue:

$$CarbonIntensity_{it} = \frac{CE_{it}}{\text{Revenue}_{it}}$$

Our baseline test of H1 estimates whether *Growth Pressure* in a firm's home city predicts a subsequent increase in the firm's *Carbon Intensity*, consistent with the multitask governance mechanism. Let i index firms, $c(i)$ the prefecture-level city where firm i is registered, and t years. We estimate:

$$\text{Carbon Intensity}_{i,t} = \alpha + \beta\, \text{Growth Pressure}_{c(i),t-1} + \Gamma' X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \qquad (1)$$

The specification includes firm fixed effects μ_i, which absorb all time-invariant firm attributes (industry baseline, technology, corporate culture, persistent energy dependence, and stable local relationships). Year fixed effects λ_t absorb national shocks (macro cycles, nationwide climate initiatives, energy price movements, and common regulatory campaigns). The vector $X_{i,t}$ includes time-varying firm fundamentals and governance characteristics (e.g., size, leverage, profitability, growth, cash flow, and asset structure) that could correlate with both *Carbon Intensity* and local macro conditions. Under this design, β is interpreted as the average within-firm change in *Carbon Intensity* associated with being located in a city that experienced *Growth Pressure* in the prior year. Because shocks to *Carbon Intensity* may be serially correlated within firms, we cluster standard errors at the firm level in baseline estimates. We additionally report robustness using two-way clustering (firm and year) to ensure inference is not sensitive to cross-sectional correlation in a given year (Petersen, 2009; Cameron & Miller, 2015).

To test the model's comparative statics, we construct moderators that map directly to the

conceptual framework. Promotion incentives capture the ex ante career return to performance, constructed from predetermined leader characteristics at appointment (Wang et al., 2020). Hard targets are coded from binding versus aspirational language in government work reports and proxy for the marginal penalty of missing the target. Institutional constraints are measured using the NERI marketization index and administrative capacity proxies, which capture the scope for discretionary enforcement (Helliwell & Huang, 2008; Shi et al., 2017). Embeddedness is measured by SOE status and executive political connections, which capture exposure to selective accommodation (Faccio, 2006). Finally, top-down oversight is proxied by exposure to Central Environmental Inspections (CEI) and related oversight conditions that increase the expected cost of enforcement slack.

4. Main Results

We organize the main results in three steps. First, we examine traditional pollutants that are subject to hard regulatory constraints. These pollutants provide a benchmark for whether growth pressure induces a broad relaxation of environmental governance. Second, we examine city-level CO_2 emissions by sector to identify the aggregate margin through which growth pressure affects carbon outcomes. Third, motivated by the sectoral evidence, we turn to firm-level carbon-emissions intensity, where local regulatory discretion and government–firm interactions are most directly observed. This sequence clarifies the mechanism: growth pressure does not appear to relax all environmental constraints; instead, it raises CO_2 emissions primarily through the industrial sector, which naturally leads to the firm-level analysis.

4.1 Hard environmental constraints: traditional pollutants do not respond

We begin with a diagnostic test that helps characterize the nature of local regulatory adjustment. If growth pressure induced a wholesale relaxation of environmental governance, one would expect emissions of traditional pollutants to rise as well. Table 1 shows otherwise. Across sulfur dioxide, nitrogen oxides, industrial wastewater, and industrial smoke/dust, the coefficients on growth pressure are small and statistically insignificant. This pattern is consistent with the institutional distinction between hard and soft environmental constraints. Traditional pollutants are directly linked to environmental accountability, monitored more frequently, and covered by established enforcement systems. Local governments therefore have limited discretion to relax these dimensions even when growth targets are at risk. The absence of a response among traditional pollutants directs attention to carbon emissions, where accountability is newer, measurement is less continuous, and enforcement discretion is greater.

[Insert Table 1 here]

4.2 City-level CO_2 emissions: the increase is driven by industry

Table 2 reports that growth pressure drives a statistically and economically significant increase in city-level CO_2 emissions. Sectoral decomposition reveals that this effect is concentrated in emissions from the industrial and power sectors; by contrast, we detect no systematic response in residential emissions. This pattern indicates that production-related emissions, particularly those from industrial activity, constitute the key margin along which local governments adjust environmental regulation when confronted with growth pressure.

Industrial CO_2 emissions are directly generated by firms' production activities, and thus are fully exposed to the stringency of local regulatory implementation. This includes the intensity of environmental inspections, emissions permitting, project approval procedures, and informal regulatory accommodation, all of which fall under the discretionary authority of local governments. These city-level sectoral findings thus pinpoint the core sector through which growth pressure transmits to micro-level environmental outcomes. Motivated by this evidence, we next examine whether firms located in cities facing growth pressure exhibit significantly higher carbon intensity.

[Insert Table 2 here]

4.3 Growth pressure increases Firm-level carbon intensity

Consistent with the city-sector evidence, Table 3 shows that firms headquartered in cities experiencing growth pressure increase carbon-emissions intensity in the following year. Columns 1-2 show that the coefficient on *Growth Pressure*$_{t-1}$ is positive and statistically significant, indicating that firms headquartered in cities that fell behind their growth targets increase *Carbon Intensity* in the subsequent year. A key diagnostic is the timing comparison. Economically, when a city faces growth pressure, firms located in that city experience an increase in carbon emission intensity by 0.87%. So, our results are not merely statistically detectable but also economically meaningful in terms of carbon efficiency. When we swap in contemporaneous *Growth Pressure*$_t$ alone in columns 3-4, the coefficient is substantially smaller and statistically weaker than the lagged effect. This timing pattern is consistent with the institutional mechanism: first-half releases reveal shortfalls too late for immediate within-year adjustments to show up in annual emissions outcomes, whereas enforcement reallocation and firms' operational responses materialize with a lag. It also mitigates concerns that the result mechanically reflects contemporaneous accounting links between output and emissions. The lagged timing is consistent with the institutional mechanism: local officials learn about midyear shortfalls, adjust enforcement and growth-stabilization priorities, and firms respond through production and compliance decisions that materialize in annual emissions outcomes.

[Insert Table 3 here]

4.4 Dynamics and persistence: adjustment horizon of carbon intensity

Table 4 estimates a distributed-lag model that includes *Growth Pressure* in *t-1*, *t-2*, and *t-3* simultaneously:

$$\text{Carbon Intensity}_{i,t} = \alpha + \sum_{k=1}^{3} \beta_k \, \text{Growth Pressure}_{c(i),t-k} + \Gamma' X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{2}$$

Two findings are central. First, the effect is strongest at *t-1*, consistent with a one-year implementation lag between a shortfall signal and realized emissions efficiency outcomes. Second, the coefficients indicate persistence into *t-2* but attenuation by *t-3*. This pattern is consistent with a short governance cycle: growth-stabilization actions and enforcement slack can affect production choices and compliance costs over multiple periods, but the system partially reverts as targets reset and oversight cycles progress. It also helps rule out purely transitory measurement artifacts.

[Insert Table 4 here]

4.5 Addressing the economic downturn alternative interpretation

A concern with the baseline fixed-effects estimates is that *Growth Pressure* may partly proxy for unobserved local shocks (e.g., sectoral downturns) that could also affect firms' *Carbon Intensity* through channels unrelated to governance (input substitution, capacity utilization, or demand composition). To strengthen identification, we exploit the 2018-2019 US-China trade war as a large, policy-driven external demand shock that differentially affected Chinese cities depending on their pre-war export exposure. The trade war generated abrupt tariff increases through Section 301 actions and subsequent retaliation, creating sharp variation in trade costs that has been widely treated as a plausibly exogenous shock in the trade literature.[2]

The trade war unfolded in discrete escalation rounds rather than gradually, which helps limit anticipatory confounding. Major tariff actions were implemented in 2018 and expanded in 2019; official USTR communications provide precise implementation dates for key rounds (e.g., the September 2018 action covering roughly $200 billion of imports, initially at 10%). Consistent with this discrete timing, the research consensus is that these tariffs meaningfully shifted prices, sourcing, and trade flows, and generated real effects that were largely borne by private agents rather than being offset by terms-of-

[2] See https://ustr.gov/about-us/policy-offices/press-office/press-releases/2018/september/ustr-finalizes-tariffs-200.

trade gains (Amiti et al., 2019).

Figure 1 documents the timing and salience of the US-China trade war shock using a time-series measure of public attention (search intensity for "trade war"). The figure shows a sharp and sustained rise in attention beginning in 2018, with clear spikes that coincide with major tariff escalation rounds. This pattern supports two premises of the external-shock design. First, it indicates that the trade war represents a discrete, economy-wide event rather than a gradual trend. Second, the pronounced timing provides a natural justification for focusing on 2018-2019 as the core shock window and for treating pre-2018 years as a relatively stable pre-period.

[Insert Figure 1 here]

We operationalize exposure at the city level using pre-war export dependence, measured before the onset of tariff escalation. Our baseline measure is:

$$\text{Trade War Exposure}_c = \frac{\overline{\text{Exports}}_{c,\,2015\text{–}2017}}{\overline{\text{GDP}}_{c,\,2015\text{–}2017}}$$

capturing how sensitive the local economy is to an external demand contraction. In robustness checks, we use an alternative exposure proxy that focuses on export links most directly affected by US policy (e.g., export share to the US), which is standard in the broader trade-shock literature when product-level tariff schedules are not mapped to local production baskets. In other robustness, we also rely on export reliance in 2015, 2015, and 2017 separately, instead of the average across the three years. This exposure design aligns with the empirical evidence that tariff shocks induced meaningful reallocations and output responses, including within China (Chor & Li, 2024). The spatial distribution is presented in Appendix Figure A1.

Following Di Giuli and Laux (2022), we implement an instrumental-variable design that cleanly separates two links in the mechanism. We first use the US-China trade war as an external, policy-driven shock that hit Chinese cities unevenly depending on their *Trade War Exposure* under a difference-in-differences setting: cities more exposed to exports experienced a sharper deterioration in near-term local economic conditions once tariffs escalated, making it more likely that officials in those places would confront *Growth Pressure*, i.e., a higher probability of falling behind their announced growth targets. In the second step, we leverage only this trade-war-induced component of *Growth Pressure* to quantify how economic pressure translates into firms' *Carbon Intensity*. When the shock raises *Growth Pressure*, local leaders have stronger incentives to prioritize growth stabilization, and because environmental enforcement is relatively more discretionary, the effective stringency of regulation can weaken,

allowing firms to operate more carbon-intensively in the subsequent year. This two-step structure clarifies interpretation: the first stage establishes that the trade war plausibly shifts local pressure for reasons external to local policy choices, and the second stage uses that shift to identify the causal effect of *Growth Pressure* on *Carbon Intensity*, consistent with the multitask governance channel.

Tables 5 provides strong evidence that the trade-war shock generated exogenous variation in *Growth Pressure* and that this shock-induced pressure translated into higher subsequent *Carbon Intensity*. In the first stage, cities with higher *Trade War Exposure* experienced a significantly larger increase in *Growth Pressure* after 2018, consistent with tariff-driven demand contractions making it harder to close the gap to announced growth targets.[3] In the second stage, the predicted component of *Growth Pressure*, instrumented by *Trade War Exposure* * *Post 2018*, has a positive and statistically significant effect on next-year *Carbon Intensity*. The magnitude is economically meaningful and aligns with the baseline results in Table 1, but now identified off plausibly exogenous tariff exposure rather than endogenous local fluctuations. Importantly, the IV estimates reinforce the mechanism interpretation: an external shock that tightens local growth constraints increases carbon intensity through heightened growth pressure, consistent with growth-first reallocation of governance capacity rather than a purely mechanical correlation between local conditions and emissions.

[Insert Table 5 here]

Table 4 reports robustness results for the trade-war instrument by replacing our baseline exposure measure with an alternative proxy that more directly captures a city's vulnerability to US tariff shocks. Specifically, while Table 5 uses overall export dependence (exports-to-GDP) as the source of differential exposure, Table 6 uses a measure tied more closely to the US-facing channel (e.g., US-directed export intensity), which is standard in the trade-shock literature when the policy shock is initiated by the US and when product-level tariff mapping is not fully available. The first stage remains strong: cities with greater US-oriented exposure experience a larger rise in *Growth Pressure* after 2018, consistent with an external demand contraction tightening local growth constraints. In the second stage, instrumented *Growth Pressure* continues to predict higher subsequent firm *Carbon Intensity*. This robustness strengthens the external-shock interpretation by showing that the IV results are not driven by a generic correlation between export intensity and emissions trends, but instead track exposure to the specific trade-war channel most plausibly linked to the policy shock.

[Insert Table 6 here]

[3] Placebo test results are presented in Appendix Figure A2.

Appendix Table A4 provide a consolidated set of supplementary analyses. Specifically, the findings document that the external-shock design yields consistent patterns when we vary the exposure construction and sample window.

4.6 Other robustness

We also conduct a set of robustness to assess whether the baseline relationship between is robust to alternative measurement, inference, and sample sample-composition concerns. The results are presented in Appendix Table A5. Column 1 redefines *Growth Pressure* using a later interim measure (e.g., Q3 GDP growth relative to the annual target), and the estimated effect on subsequent *Carbon Intensity* remains positive and economically similar to the baseline, indicating that the result is not driven by the specific midyear cutoff. Column 2 replaces revenue scaling with an alternative normalization (e.g., emissions-to-assets) (Hong et al., 2019). The effect persists, suggesting the baseline findings capture shifts in carbon efficiency rather than a revenue-denominator artifact. Column 3 re-estimates the baseline specification using the more conservative inference procedure (e.g., two-way clustering by firm and year), and statistical significance is preserved, suggesting that the main conclusion is not sensitive to correlation structures in the panel (Petersen, 2009; Cameron & Miller, 2015). Finally, column 4 and 5 apply the sample restriction (e.g., excluding first-tier cities and municipalities), and the *Growth Pressure* coefficient remains comparable, implying that the baseline effect is not driven by a small set of politically salient jurisdictions. Taken together, the table shows that the positive association between *Growth Pressure* and subsequent *Carbon Intensity* is robust to alternative timing definitions, alternative outcome scaling, conservative inference, and sample-composition adjustments.

Even with rich controls and fixed effects, misspecification concerns remain if the relationship between controls and *Carbon Intensity* is nonlinear or if interactions matter. Appendix Table A6 applies Double Machine Learning (DML) to flexibly partial out confounders while targeting the causal parameter of interest under standard orthogonality conditions (Belloni et al., 2014). DML addresses this by flexibly estimating the nuisance functions for the outcome and treatment equations and then recovering the treatment effect using a Neyman-orthogonal moment condition, which makes the resulting estimate robust to small errors in those nuisance components. In implementation, we use six machine-learning algorithms, Lasso, Elastic-Net, Random Forest, Neural Network, Ridge Regression, and Support Vector Machine, to estimate the nuisance functions, allowing the data to select nonlinearities and interactions without hand-specification. Across these learners, the estimated coefficient on *Growth Pressure* remains positive and close to the baseline estimate, with comparable statistical significance. This convergence across methods indicates that our main result is not driven by restrictive functional-form assumptions or omitted nonlinear relationships in controls; instead, *Growth*

Pressure robustly predicts a subsequent increase in firms' carbon efficiency losses.

We conduct a synthetic control analysis as a robustness check to the IV results. The synthetic control method provides an explicit counterfactual by choosing weights on untreated cities to match pre-treatment outcomes and covariates. Treatment assignment follows the trade-exposure logic. A city is treated if its average export-to-GDP ratio in the pre-treatment period is above the sample median. Because treated cities are numerous and heterogeneous, we form a composite treated unit by computing year-by-year averages of carbon intensity and predictors across treated cities, weighting each city by its number of pre-treatment observations. The donor pool includes all other cities with complete data for 2015–2020. We set 2018 as the intervention year (the escalation of the U.S.–China trade conflict) and use 2015–2017 to construct weights. The outcome is city-level carbon intensity. Predictor variables include per capita GDP, industrial structure, transportation infrastructure, information technology development, trade openness, aggregate energy consumption, consumption level, and overall firm profitability. Appendix Table TA8 and FA4 shows that the synthetic control closely matches the treated unit on key predictors. The synthetic control places most weight on Cities 54 and 61, with smaller weights on a few additional cities. Prior to the onset of the trade war in 2017, the treated units closely track their synthetic counterparts, indicating a good pre-treatment fit and supporting the validity of the counterfactual.

Following the March 2018 presidential memorandum, successive tariff rounds expanded coverage from an initial $50 billion to near-comprehensive levels. This period corresponds to the peak of the trade shock, characterized by a sharp deterioration in export conditions and increased growth pressure on local governments. Under binding growth targets and environmental constraints, local authorities appear to have reallocated regulatory effort away from less salient and weakly enforced environmental dimensions to stabilize economic outcomes. Trade tensions partially eased in 2019, following the temporary truce reached at the G20 Buenos Aires meeting, which suspended further tariff escalation and allowed bilateral negotiations to proceed. China's commitment to expand imports from the United States, including agricultural goods, contributed to short-term stabilization. However, elevated uncertainty and the persistence of previously imposed tariffs suggest that the partial convergence between observed and synthetic series reflects improved expectations and temporary policy relaxation, rather than a full dissipation of the shock. In 2020, the divergence re-emerges. Renewed disputes over compliance and the re-politicization of trade tensions sustained residual frictions, continuing to weigh on real economic activity. The increase in the outcome variable during this period is consistent with a second wave of trade shocks and their transmission through domestic policy responses.

To address potential measurement errors arising from incomplete or biased firm-level disclosure, we construct an alternative measure based on the 2006 IPCC Guidelines for National Greenhouse Gas Inventories. Specifically, industry-level emissions are first estimated using energy consumption data from the China Energy Statistical Yearbook, where different energy sources (e.g., coal, crude oil, and

natural gas) are converted into standard coal equivalents and multiplied by corresponding emission factors. Firm-level emissions are then obtained by allocating industry emissions based on each firm's share of operating costs in total industry main business costs:

$$CE_{it}^{IPCC} = \text{Industry Emissions}_{it} \times \frac{\text{Operating Cost}_{it}}{\text{Industry Main Business Cost}_{it}}$$

Based on this, we further construct the alternative carbon intensity measure:

$$CarbonIntensity_{it}^{IPCC} = \frac{CE_{it}^{IPCC}}{\text{Revenue}_{it}}$$

This alternative measure relies on exogenous industry-level energy consumption rather than firm-level disclosure, thereby mitigating concerns related to reporting bias and measurement error. Table A7 presents the regression results using carbon intensity based on the IPCC-style estimation as the dependent variable. The consistency of coefficient estimates across different fixed effects specifications suggests that the positive relationship between environmental pressure and carbon intensity is robust. Nevertheless, the results based on the IPCC-style measure are broadly consistent with the baseline findings, indicating that the main conclusions are not driven by the choice of emission measure.

Taken together, the main results establish a robust reduced-form relationship: growth shortfalls predict higher subsequent firm carbon emission intensity. This pattern is consistent with the multitask model in which growth pressure increases the marginal value of growth-oriented governance effort, crowding out environmental enforcement (H1). However, the baseline estimates alone do not fully pin down the channel. For that reason, the next section tests the model's comparative statics, whether the effect strengthens when career concerns sharpen incentives (promotion incentives), when the shortfall penalty is higher (hard targets), when discretion is larger (weaker institutions), and whether it weakens when oversight increases the expected cost of enforcement slack (central inspections). These heterogeneity results are designed to adjudicate between a governance reallocation mechanism and alternative channels that would not generate the same structured pattern.

5. Mechanisms and Heterogeneity

The baseline results establish that *Growth Pressure* predicts higher subsequent firm *Carbon Intensity*. This section tests the comparative statics implied by the multitask framework. The logic is simple: the pressure–carbon link should be strongest when the marginal penalty of missing growth targets is higher (career concerns; hard targets), when discretion is larger (weaker institutions and administrative capacity), when enforcement slack can be selectively allocated (SOEs and connected firms), and it should weaken or disappear when top-down oversight raises the expected cost of enforcement slack (high-visibility polluters; CEI). We implement them using split-sample estimates, while maintaining the baseline controls and fixed effects so that comparisons reflect within-firm changes over time.

5.1 Political incentives amplify the growth-environment trade-off

Promotion incentives (H2). In the model, a shortfall increases the marginal return to growth-oriented effort; that reallocation should be stronger when officials have more at stake in near-term performance. To test this, we build a panel of 1,832 city mayors and hand-collect biographical characteristics (age, education, tenure dates, initial administrative rank, and subsequent career trajectory). We define *Promotion* as an indicator equal to one if a mayor is promoted to a higher-level post at term end (e.g., municipal party secretary, vice governor, or comparable vice-ministerial positions) and zero otherwise; following the political-economy literature, lateral transfers to the People's Congress or Political Consultative Conference are excluded as they do not represent comparable career advancement (Li & Zhou, 2005). To isolate ex ante career concerns, we estimate a probit model of *Promotion* using only predetermined characteristics at appointment including age, initial rank, and their interaction, and deliberately exclude realized economic performance or tenure outcomes that could mechanically correlate with emissions. The predicted promotion probability is our Promotion *Incentive Index* (continuous), and we split the sample at its median into high- and low-incentive jurisdictions (Wang et al., 2020). Appendix Figure A3 illustrates its spatial distribution across cities. Table 7 shows that the *Growth Pressure* effect is concentrated in the high-incentive subsample, while it is small and statistically indistinguishable from zero in the low-incentive subsample. This pattern aligns tightly with H2: career concerns increase the effective weight on growth performance (higher θ_g and/or effective κ), deepening the crowd-out of environmental governance under *Growth Pressure*.

[Insert Table 7 here]

Target stringency (H3). The model also implies that the response should be stronger when the penalty from missing the target is intrinsically larger. We therefore classify city targets as hard versus soft based on binding language in government work reports. Specifically, we code a target as a hard constraint when the report uses obligatory wording (e.g., "must achieve," "ensure above X%"), and as a soft constraint when the language is aspirational or indicative; *Hard Constraint* equals one for the former and zero for the latter. Table 8 shows that *Growth Pressure* predicts higher subsequent *Carbon Intensity* in the hard-target subsample, while the effect is absent under soft targets. This is precisely the comparative static predicted by the multitask framework: a larger effective κ (binding targets) makes the shortfall shock more consequential, strengthening the incentive to reallocate effort toward growth and away from enforcement.

[Insert Table 8 here]

5.2 Institutional capacity shapes discretion and buffers enforcement

The multitask mechanism relies on discretionary space: officials must be able to relax effective enforcement without immediately triggering high-probability sanctions. We therefore examine two complementary dimensions of institutional capacity, market institutions and administrative efficiency, that predict the feasibility and cost of enforcement slack.

Marketization (H4, part 1). Using the NERI *Marketization Index*, we proxy for the maturity of local market institutions and rule-based governance (Helliwell & Huang, 2008). The index captures market-oriented reforms such as factor market development, private-sector participation, and institutional constraints associated with rule-of-law and transparency. We split cities at the median into high- and low-marketization groups. The model predicts weaker effects where institutions raise the expected cost of discretionary slack (higher effective $\Pr(\text{sanction} \mid e_e) \cdot L$). Consistent with this, Table 9 shows that *Growth Pressure* does not significantly affect *Carbon Intensity* in high-marketization regions, but it does in low-marketization regions. Interpreted through the framework, institutional maturity limits the ability to trade environmental stringency for short-term growth because it increases accountability and reduces the scope for informal accommodation.

[Insert Table 9 here]

Administrative efficiency (H4, part 2). We then test whether bureaucratic capacity similarly buffers the effect. Following Shi et al. (2017), we measure administrative efficiency as the log population served per public employee in public administration, social security, and social organizations, higher values indicate more efficient administrative capacity. We again split at the median. Table 10 shows that the pressure effect is concentrated in low-efficiency regions and disappears in high-efficiency regions. This result is consistent with the idea that weak administrative capacity exacerbates enforcement gaps and information frictions, making it easier to deprioritize environmental monitoring when *Growth Pressure* rises; conversely, capable governments can sustain enforcement even under pressure.

[Insert Table 10 here]

5.3 Selective transmission through government-business embeddedness

If *Growth Pressure* operates partly through discretionary enforcement, its effects should not be uniform across firms. Local governments can obtain growth stabilization most effectively by relaxing constraints for firms that are administratively responsive, strategically important, or politically embedded. We therefore examine heterogeneity across ownership and political embeddedness, which capture distinct channels of proximity to the state.

State ownership (H5, part 1). SOEs are embedded in the bureaucratic hierarchy and often act as

quasi-fiscal instruments for stabilizing investment and employment. Their softer budget constraints and closer administrative ties make them plausible recipients of accommodation when growth targets are at risk. Table 11 confirms that *Growth Pressure* increases *Carbon Intensity* for SOEs but not for private firms. The pattern is consistent with selective slack: under pressure, local governments can mobilize SOEs toward output and employment objectives and tolerate (or fail to restrain) the emissions consequences.

[Insert Table 11 here]

Political embeddedness (H5, part 2). Formal ownership does not capture all state-firm ties. Many non-SOE firms maintain connections that can affect enforcement and access to forbearance. We hand-collect biographical information on CEOs, senior executives, and board members (e.g., from comprehensive finance portals such as Sina Finance and linked databases). Following Walder (2006), a firm is coded as politically embedded if at least one key executive previously held a government post at or above the division (chu) level, capturing meaningful bureaucratic authority and durable ties. Table 12 shows that the pressure effect is present among politically embedded firms but not among unconnected firms. This is consistent with the mechanism that informal networks facilitate coordination and selective enforcement, allowing *Growth Pressure* to translate into firm-level environmental outcomes via relational governance.

[Insert Table 12 here]

5.4 Oversight and regulatory intensity constrain the trade-off

The framework predicts that the *Growth Pressure* effect should attenuate when the expected penalty from weak environmental governance increases. We test this using two sources of heightened oversight: high-visibility polluters and the Central Environmental Inspection (CEI) program.

Industry pollution intensity (H6, part 1). We classify industries into heavily polluting versus other sectors using official designations. Heavily polluting sectors are more visible and more likely to be targeted by audits and inspection campaigns, which raises the political risk of enforcement slack. Table 13 shows that *Growth Pressure* increases *Carbon Intensity* primarily among firms in non-heavily polluting sectors, while the effect is small and statistically insignificant among heavily polluting industries. This cross-sector asymmetry is informative: it suggests that local governments do not uniformly relax environmental governance; instead, they allocate leniency where monitoring is less intense and where the probability of detection and sanction is lower.

[Insert Table 13 here]

Central Environmental Inspection (H6, part 2). To sharpen the role of top-down oversight, we exploit exposure to the CEI program as an institutional shock that increases monitoring and raises the expected cost of noncompliance for local officials. We partition provinces into those under CEI supervision versus those outside the inspection scope. Table 14 shows that *Growth Pressure* predicts higher *Carbon Intensity* in non-CEI provinces but not in CEI provinces. This is exactly what the model implies: CEI increases Pr(sanction $\mid e_e$) and/or L, making enforcement slack a much more expensive response to *Growth Pressure* and thereby breaking the growth-environment linkage. The result also helps distinguish the governance mechanism from a generic downturn story: a pure demand-driven channel would not naturally disappear under an oversight intervention targeted at enforcement incentives.

[Insert Table 14 here]

Across these heterogeneity tests, the patterns line up with the model's comparative statics: the *Growth Pressure* effect is strongest when incentives to prioritize growth are sharper (promotion incentives; hard targets), when discretion is larger (low marketization; low administrative efficiency), when accommodation can be targeted (SOEs; connected firms), and it is muted when oversight makes enforcement slack costly (high-visibility polluters; CEI). Collectively, these results provide mechanism-consistent evidence that the main effect operates through state-contingent environmental governance under target-based growth management.

6. Mechanism: Governance Reallocation

6.1 Policy Attention

A key implication of the multitask framework is that officials facing binding growth targets must allocate limited attention across competing policy objectives. When economic growth falls short of targets, local governments do not strengthen symbolic environmental rhetoric in response to performance pressure. Instead, they reduce the emphasis placed on environmental goals. This reflects an attention reallocation mechanism: as economic growth becomes the dominant evaluation criterion, other policy objectives—such as carbon reduction—are deprioritized in both communication and implementation.

We construct a measure of local governments' carbon reduction priority using a dictionary-based text analysis of city-level government work reports. Following the text-as-data approach widely used in economics and finance (Baker et al., 2016; Gentzkow et al., 2019; Hassan et al., 2019), we define Carbon Reduction Priority (CRP) as the average frequency of carbon-related keywords in the government work reports of the prefecture-level cities where listed firms and their subsidiaries are

located. The dictionary includes terms such as “carbon dioxide,” “low-carbon,” “emission reduction,” “energy saving,” “energy consumption,” “environmental protection,” “ecology,” and “green.” We normalize keyword frequency by total document length to ensure comparability across reports.

This measure has an important identification advantage in terms of timing. Government work reports are drafted and released at the beginning of each year, prior to most within-year policy implementation and realized firm outcomes. This temporal ordering mitigates reverse causality concerns and supports interpreting carbon reduction priority as a forward-looking signal of government priorities.

To examine the mediating role of carbon reduction priority, we implement an IV strategy. The first-stage of table 15 results show that higher growth pressure significantly reduces carbon reduction priority, indicating that local governments systematically downplay carbon reduction in official discourse following growth shortfalls. The second-stage results show that lower CRP is associated with higher firm-level carbon intensity. This suggests that when local governments weaken the emphasis on carbon reduction, firms face less stringent environmental oversight and regulatory pressure, leading to higher emissions. Whereby local officials shift both communication and policy focus away from environmental objectives when confronted with heightened growth pressure.

[Insert Table 15 here]

6.2 Regulatory Enforcement

Policy execution is shaped by the incentives and pressures faced by politicians, managers, and street-level bureaucrats, rather than by statutory mandates alone (May & Winter, 2009). In our setting, growth pressure changes the political environment in which environmental enforcement agencies operate. Table 16 examines how growth pressure shapes local environmental enforcement behavior. Higher growth pressure in the previous year is associated with a statistically significant decline in the number of environmental penalty cases. In parallel, the total monetary value of penalties also decreases, indicating that the effect is not merely driven by compositional changes—such as a reduction in minor violations—but instead reflects a broad-based weakening of enforcement intensity.

These results suggest that when local officials face stronger performance incentives tied to economic growth, they systematically deprioritize environmental regulation along both the extensive margin (the number of cases) and the intensive margin (penalty severity). The findings are consistent with a multitasking framework in which regulatory enforcement is endogenously adjusted in response to competing political incentives. More broadly, they imply that performance-based promotion systems may generate unintended environmental consequences, even in the presence of formal regulatory mandates.

[Insert Table 16 here]

7. Additional Extensions

7.1 ESG Outcomes

A potential concern is that firms may engage in "greenwashing" under local growth pressure, improving ESG-related appearances even as actual carbon emissions rise. If this channel were important, growth pressure could affect ESG assessments through symbolic compliance and disclosure efforts or by shifting performance across specific ESG dimensions.

Table 17 reports the results of the ESG-rating robustness tests. We further replace the dependent variable with aggregate ESG ratings from alternative data providers, including the Huazheng ESG rating, the Wind ESG composite score, and the Bloomberg ESG score. The results show that local economic downturn pressure has no statistically significant effect on firms' aggregate ESG ratings. This divergence is highly consistent with the central insight of Asgharian et al. (2024): carbon-emissions intensity provides an objective measure of firms' real decarbonization behavior, whereas aggregate ESG ratings contain substantial declarative and formalistic components and are therefore vulnerable to corporate greenwashing.

Specifically, under local economic downturn pressure, firms seeking to protect short-run operating performance may cut back first on long-horizon environmental technology upgrades and emissions-reduction investments, directly increasing real carbon-emissions intensity. At the same time, firms can maintain stable ESG ratings through relatively low-cost actions, such as improving ESG disclosure or issuing formal environmental policies and targets. This gives rise to a divergence in which real environmental performance deteriorates significantly while aggregate ESG ratings remain statistically unchanged.

Several mechanisms may explain this pattern. First, the cost of improving these indicators and the difficulty of adjusting them differ fundamentally. Reducing carbon-emissions intensity requires firms to make costly investments in equipment renewal, technological upgrading, and energy-structure transformation. These investments are characterized by strong cost stickiness and long adjustment horizons. When local economic conditions deteriorate and firms face profitability pressure, such expenditures are likely to be among the first non-mandatory investments to be reduced, thereby directly increasing carbon-emissions intensity. By contrast, maintaining or improving aggregate ESG ratings can often be achieved through lower-cost measures, such as optimizing disclosure, making symbolic environmental commitments, or engaging in temporary public-interest activities. These actions do not necessarily require substantive emissions-reduction investments, which helps explain why ESG ratings do not respond significantly to local economic downturn pressure.

Second, endogenous biases in ESG ratings and the scope for greenwashing weaken their informational content. ESG ratings differ substantially across providers and are often subject to

methodological opacity, or "black-box" problems (Berg et al., 2022). As a result, ratings may be influenced by firms' selective disclosure behavior. Asgharian et al. (2024) further argue that only objective emissions-based measures, such as carbon-emissions intensity, can credibly capture firms' real environmental behavior, whereas declarative ESG scores more often reflect compliance mimicry rather than genuine improvements in environmental performance. Our findings corroborate this argument. Under local economic downturn pressure, firms' real decarbonization behavior deteriorates significantly, yet firms can maintain unchanged ESG ratings through symbolic actions. This pattern further highlights the central importance of objective carbon-emissions measures in environmental finance research.

These results suggest that local growth pressure does not translate into a broad-based improvement in firms' ESG standing. This pattern is consistent with the view that growth pressure weakens substantive environmental performance, while leaving room for firms to maintain or even improve more visible or institutionally mediated non-environmental dimensions of ESG assessment. Taken together, the evidence suggests that the increase in carbon intensity documented in the baseline results is unlikely to be explained by a uniform shift in overall ESG ratings. Rather, growth pressure appears to worsen environmental performance specifically, which is consistent with our interpretation that carbon-related constraints become easier to relax under local growth pressure.

[Insert Table 17 here]

7.2 Policy Regimes

To further explore how institutional changes shape the growth-environment trade-off, we examine whether the effect varies across major policy regimes. In particular, we focus on two key milestones: the post-2016 period, following the Paris Agreement, and the post-2020 period, marked by the introduction of China's "dual carbon" targets.

As reported in Table 18, the coefficient on growth pressure is positive and statistically significant in the post-2016 period. This suggests that firms located in cities facing growth pressure increase their carbon intensity when environmental objectives become more salient but still allow for local discretion. In contrast, the coefficient becomes substantially smaller and statistically insignificant in the post-2020 period. This pattern indicates that the relationship between growth pressure and carbon emissions weakens when environmental constraints become more binding.

The results are consistent with a regime-dependent interpretation of environmental governance. In the post-2016 period, environmental regulation becomes more prominent, increasing the salience of the growth–environment trade-off while still leaving room for discretionary enforcement adjustments. As a result, local governments may respond to growth pressure by relaxing effective environmental

stringency, leading to higher carbon intensity. However, after the introduction of the dual-carbon targets in 2020, environmental regulation appears to become more binding and less discretionary. Under tighter constraints, local governments have limited scope to accommodate economic pressure through environmental channels, thereby attenuating the impact of growth pressure on firms' carbon emissions.This pattern is consistent with a non-monotonic effect of institutional stringency, where moderate levels of regulation amplify the growth – environment trade-off, while sufficiently strict regulation constrains it. When environmental protection gains policy salience, growth pressure crowds out environmental efforts; yet when environmental protection becomes a non-negotiable policy mandate, this crowd-out mechanism is significantly muted.

[Insert Table 18 here]

8. Conclusion

This paper studies how target-based growth management can generate unintended climate externalities through discretionary implementation. Using prefecture-level growth targets from government work reports and firm-level carbon outcomes for 2012-2022, we show that *Growth Pressure*, defined as a first-half shortfall relative to the announced annual target, predicts a subsequent increase in firm *Carbon Intensity*. The effect is economically meaningful, persists into the second year, and fades thereafter, consistent with policy and production adjustment lags. Interpreted through a multitask governance framework, the evidence suggests that when growth targets are at risk, local governments reallocate scarce administrative capacity toward growth stabilization in ways that relax effective environmental stringency.

A central contribution is that we do not rely solely on the baseline reduced-form relationship. Instead, we test a set of sharp comparative statics implied by the model, and the heterogeneity patterns line up tightly with the incentive and constraint channels. The pressure-carbon link is stronger where the marginal penalty of missing growth targets is plausibly higher, e.g., in jurisdictions led by officials with stronger ex ante promotion incentives and in cities that set hard (binding-language) targets. It is also stronger where discretion is larger, as proxied by weaker market institutions and lower administrative efficiency. The effect concentrates among SOEs and politically connected firms, consistent with selective accommodation rather than a uniform local downturn mechanism. Finally, it attenuates or disappears when top-down oversight increases the expected cost of enforcement slack, including in high-visibility polluting settings and under central environmental inspection exposure. Taken together, these results support the view that the main effect operates through state-contingent environmental governance under growth pressure, rather than only through mechanical changes in demand or production composition. Growth pressure reduces carbon-reduction priority in government work reports and weakens environmental enforcement. This communication and enforcement pattern

indicates that local governments reallocate attention away from carbon governance when growth targets become binding.

The results imply that climate policy effectiveness in target-based, decentralized systems hinges on implementation incentives, especially during stress periods when growth targets are at risk. When growth shortfalls carry sharper, more continuous penalties than environmental underperformance, local governments have predictable incentives to relax effective enforcement and accommodate carbon-intensive production, often selectively among state-linked or politically connected firms, while maintaining pro-environment rhetoric. Policymakers can mitigate this growth-environment trade-off by redesigning accountability to keep environmental constraints binding when growth is under pressure (e.g., using continuous, verifiable enforcement and emissions metrics), and by strengthening monitoring and audit mechanisms that raise the expected cost of enforcement slack (such as credible inspections). In practice, this also means conditioning growth-stabilization tools, such as credit facilitation, project approvals, and SOE mobilization, on emissions performance so that short-run stabilization is not achieved through higher carbon intensity.

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Table 1. Growth Pressure and Traditional Pollutant Emissions

This table reports the effects of growth pressure on various industrial pollution emission indicators, with control variables including GDP per capita, industrial structure, marketization index, information infrastructure, degree of trade openness, government administrative efficiency, economic agglomeration, consumption level, overall corporate profitability, Rain, Temperature, Sunshine, Humidity. Coefficients are reported in the first row, and standard errors clustered at the city level are reported in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Air quality	Waste Gas	Waste water	Smoke dust	SO2	NO2	CO
$Pressure_{t-1}$	0.004	0.007	0.037	-0.052	0.778	-0.404	0.537
	(0.011)	(0.032)	(0.062)	(0.053)	(0.984)	(0.688)	(0.627)
GDP per capita	-0.029	0.199*	-0.069	-0.089	-0.322	5.580***	-6.592
	(0.055)	(0.104)	(0.105)	(0.163)	(4.386)	(2.122)	(5.267)
Industrial composition	0.023	-0.109	0.098	0.302**	-3.703	-4.999***	14.586
	(0.036)	(0.104)	(0.086)	(0.135)	(2.441)	(1.372)	(12.487)
Marketization level	-0.017	-0.109***	-0.055	-0.060	-1.381	-0.129	0.226
	(0.015)	(0.042)	(0.049)	(0.054)	(1.040)	(0.737)	(0.501)
Digital infrastructure	-0.004**	0.013*	-0.030	0.020*	0.379**	0.225***	0.209
	(0.002)	(0.007)	(0.054)	(0.012)	(0.150)	(0.082)	(0.217)
Trade openness	-0.031	0.005	-0.059	0.064	3.273	1.650	3.865
	(0.025)	(0.102)	(0.114)	(0.117)	(2.909)	(1.211)	(4.035)
Government efficiency	-0.031	-0.141	-0.059	-0.017	-3.333	-0.769	-1.371
	(0.054)	(0.121)	(0.121)	(0.204)	(4.289)	(2.224)	(1.740)
Consumption level	-0.011	0.114	-0.011	0.082	5.571*	-2.311	4.093
	(0.026)	(0.105)	(0.164)	(0.168)	(3.066)	(1.445)	(3.134)
Aggregate firm profits	-0.007*	0.002	0.000	0.021**	-0.285	0.271***	-0.385
	(0.004)	(0.008)	(0.007)	(0.009)	(0.314)	(0.081)	(0.346)
Rain	0.000	0.000	0.000	0.000	0.013	-0.005	0.027
	(0.000)	(0.001)	(0.001)	(0.001)	(0.015)	(0.010)	(0.023)
Temperature	-0.005	-0.004	-0.003	-0.033	3.961**	-0.973	-0.084
	(0.016)	(0.035)	(0.033)	(0.055)	(1.701)	(0.714)	(1.046)
Sunshine	-0.008	-0.012	0.010	0.054	0.366	0.060	0.817
	(0.011)	(0.028)	(0.037)	(0.040)	(0.752)	(0.430)	(0.796)
Humidity	-0.002	-0.002	-0.013	0.005	0.456*	-0.101	-0.799
	(0.003)	(0.008)	(0.009)	(0.011)	(0.241)	(0.147)	(0.865)
Constant	5.465***	8.134***	11.439***	9.036***	-112.642	15.475	56.621
	(0.618)	(1.613)	(2.599)	(2.725)	(98.364)	(33.224)	(68.752)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.790	0.915	0.914	0.846	0.700	0.795	0.015
Observations	933	1,093	896	1,209	1,197	1,197	1,197

Table 2. Growth pressure and sectoral CO_2 emissions: evidence from MEIC

This table reports the effect of local growth pressure on sectoral CO_2 emissions using MEIC data at the city level. Coefficients are reported in the first row, and standard errors clustered at the city level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Model	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Total	Industry	Power	Residential	Transportation
$Pressure_{t-1}$	5.759**	3.784**	1.783**	-0.037	0.229
	(2.677)	(1.887)	(0.883)	(0.154)	(0.209)
GDP per capita	7.268	5.404*	2.537	-0.381*	-0.292
	(5.985)	(2.988)	(3.622)	(0.216)	(0.484)
Industrial composition	-0.786	0.133	0.939	-0.616***	-1.242***
	(3.452)	(2.263)	(1.531)	(0.153)	(0.388)
Marketization level	-0.106	0.146	-0.192	0.024	-0.083
	(0.867)	(0.608)	(0.362)	(0.056)	(0.080)
Digital infrastructure	1.387***	0.767***	0.302*	0.098***	0.221***
	(0.450)	(0.252)	(0.172)	(0.029)	(0.057)
Trade openness	6.629***	3.686***	1.325	0.332***	1.287***
	(1.725)	(1.023)	(0.977)	(0.074)	(0.208)
Government efficiency	2.993	2.615	-0.252	1.042***	-0.412
	(4.314)	(2.543)	(2.065)	(0.263)	(0.526)
Consumption level	5.142	3.298	0.945	0.283	0.617
	(5.502)	(2.830)	(3.291)	(0.227)	(0.485)
Aggregate firm profits	0.798**	0.435**	0.262	0.071**	0.029
	(0.381)	(0.201)	(0.205)	(0.033)	(0.031)
Rain	-0.008	-0.003	-0.012	0.000	0.007
	(0.039)	(0.025)	(0.018)	(0.002)	(0.006)
Temperature	-0.195	0.127	-0.324	0.018	-0.016
	(0.314)	(0.172)	(0.201)	(0.022)	(0.036)
Sunshine	1.170	0.292	0.732	0.081	0.065
	(1.596)	(1.000)	(0.736)	(0.091)	(0.133)
Humidity	-0.083	-0.185	0.143	-0.005	-0.036
	(0.284)	(0.185)	(0.126)	(0.010)	(0.036)
Constant	-155.471***	-105.608**	-43.245*	-4.626	-1.992
	(56.294)	(40.671)	(21.980)	(3.993)	(5.768)
Province FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.566	0.556	0.428	0.647	0.638
Observations	1,122	1,122	1,122	1,122	1,122

Table 3. Economic growth shortfall and carbon emission intensity

This table reports the baseline estimates examining the effect of local economic growth pressure on firm-level carbon emission intensity. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Dependent Variable	Carbon Intensity			
Model	(1)	(2)	(3)	(4)
Growth Pressure $_{t-1}$	0.025**	0.024**		
	(0.011)	(0.012)		
Growth Pressure $_{t}$			0.006	0.005
			(0.011)	(0.011)
Firm size	0.002	0.004	0.002	0.004
	(0.003)	(0.009)	(0.003)	(0.009)
Financial leverage	-0.032	-0.064*	-0.033	-0.060
	(0.023)	(0.036)	(0.023)	(0.037)
Profitability	-0.001*	-0.001	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Asset turnover	0.009	0.013	0.009	0.014
	(0.009)	(0.018)	(0.009)	(0.018)
Operating cash flow ratio	0.084*	0.075	0.076	0.064
	(0.047)	(0.053)	(0.047)	(0.053)
Inventory turnover	-0.007	-0.039	-0.007	-0.042
	(0.030)	(0.059)	(0.030)	(0.060)
Fixed asset turnover	-0.022	-0.011	-0.021	-0.001
	(0.023)	(0.042)	(0.023)	(0.043)
Revenue growth	-0.014	-0.013	-0.015*	-0.013
	(0.009)	(0.010)	(0.009)	(0.010)
Loss indicator	0.001	-0.000	0.001	0.000
	(0.009)	(0.010)	(0.009)	(0.010)
Board independence	-0.000	0.000	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.001)
CEO duality	0.002	0.005	0.001	0.005
	(0.006)	(0.010)	(0.006)	(0.010)
Tobin's Q	-0.002	-0.001	-0.001	-0.001
	(0.002)	(0.004)	(0.002)	(0.004)
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	No	Yes	No
Industry FE	Yes	No	Yes	No
Firm FE	No	Yes	No	Yes
Adjusted R-squared	0.877	0.875	0.878	0.876
Observations	17,564	17,564	17,404	17,404

Table 4. Dynamic effects of growth pressure on carbon intensity

This table reports estimates examining the dynamic effects of local economic growth pressure on firm-level carbon emission intensity. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	Carbon	Carbon	Carbon	Carbon
$Pressure_{t-1}$	0.034**	0.030*	0.031**	0.026*
	(0.014)	(0.015)	(0.014)	(0.016)
$Pressure_{t-2}$	0.028**	0.029**	0.025**	0.020
	(0.012)	(0.013)	(0.012)	(0.014)
$Pressure_{t-3}$		0.012		0.010
		(0.022)		(0.023)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.881	0.892	0.88	0.892
Observations	11,761	9,416	11,761	9,416

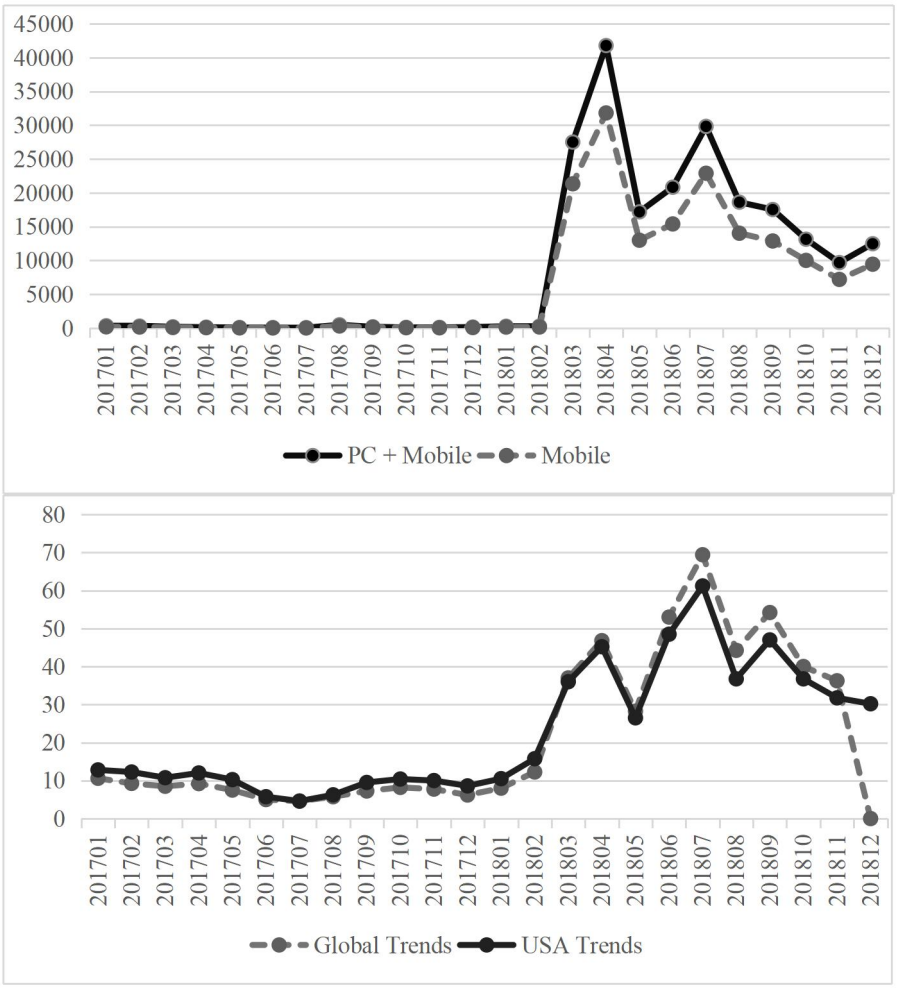


Figure 1. Monthly search intensity for trade war on Baidu and Google, 2017 – 2018

This figure plots the monthly search intensity for the term "trade war" on Baidu (China) and Google (United States) from January 2017 to December 2018. Search volumes remained consistently low before March 22, 2018, when the U.S. President formally announced tariff measures under the Section 301 investigation. Search intensity surged sharply and simultaneously in both countries after the announcement, indicating that the China – U.S. trade war was a salient and largely unanticipated information shock. The absence of pre-trend movements supports the exogeneity of the event and mitigates concerns about anticipatory adjustments by firms or local governments. Note that the two platforms differ in measurement units and computational methods, so their levels are not directly comparable, while both series exhibit a pronounced and abrupt upward shift after the policy announcement.

Table 5. The China – U.S. trade war

This table reports two-stage least squares estimation results using the 2018 – 2019 China – U.S. trade war as an exogenous shock. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Dependent Variable	$Pressure_{t-1}$	Carbon
	(1)First-stage	(2)Second-stage
$TradeWar_i \times Post_t$	0.452***	
	(0.032)	
$Pressure_{t-1}$		0.216*
		(0.113)
Controls	Yes	Yes
Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Kleibergen-Paap rk F		195.52
Kleibergen-Paap LM		140.08
Cragg-Donald Wald F		208.70
Centered R2		0.869
Observations	16,955	16,955

Table 6. The China – U.S. trade war using direct exports to the U.S.

This table reports robustness check results where overall export dependence is replaced by the three-year average share of direct exports to the United States. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Dependent Variable	$Pressure_{t-1}$	Carbon
	(1)First-stage	(2)Second-stage
$TradeWar_i \times Post_t$	1.383***	
	(0.059)	
$Pressure_{t-1}$		0.362**
		(0.173)
Controls	Yes	Yes
Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Kleibergen-Paap rk F		547.65
Kleibergen-Paap LM		163.44
Cragg-Donald Wald F		87.72
Centered R2		0.864
Observations	16,728	16,728

Table 7. Promotion incentives

This table reports regression results examining the effect of local economic growth shortfalls on firm-level carbon emission intensity across officials with different promotion incentives. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	High promotion	Low promotion	High promotion	Low promotion
$Pressure_{t-1}$	0.045***	0.007	0.045***	0.004
	(0.016)	(0.017)	(0.016)	(0.017)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.878	0.875	0.877	0.874
Observations	9,213	8,351	9,213	8,351

Table 8. Growth target stringency

This table reports the effect of local economic growth shortfalls on firm-level carbon emission intensity across cities with hard versus soft growth targets. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	Hard constraint	Soft constraint	Hard constraint	Soft constraint
$Pressure_{t-1}$	0.036**	-0.012	0.036**	-0.019
	(0.016)	(0.021)	(0.016)	(0.021)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.890	0.846	0.888	0.850
Observations	11,174	5,648	11,174	5,648

Table 9. Marketization level

This table reports the effects of local economic growth shortfalls on firm-level carbon emission intensity across regions with high and low levels of marketization. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	High market	Low market	High market	Low market
$Pressure_{t-1}$	0.009	0.044**	0.005	0.047**
	(0.017)	(0.019)	(0.017)	(0.020)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.908	0.046	0.905	0.253
Observations	12,076	5,488	12,076	5,488

Table 10. Administrative efficiency

This table reports regression results examining the moderating role of local administrative efficiency in the relationship between economic growth shortfalls and firm-level carbon emission intensity. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	High efficiency	Low efficiency	High efficiency	Low efficiency
$Pressure_{t-1}$	0.010	0.051**	0.008	0.050**
	(0.015)	(0.021)	(0.015)	(0.022)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.907	0.027	0.905	0.211
Observations	12,065	5,499	12,065	5,499

Table 11. Ownership structure

This table reports regression results examining the effect of local economic growth shortfalls on firm-level carbon emission intensity by ownership type. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	SOEs	Private firms	SOEs	Private firms
$Pressure_{t-1}$	0.057***	0.016	0.053***	0.010
	(0.020)	(0.017)	(0.020)	(0.017)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Observations	5,625	10,222	5,625	10,222
Adjusted R-squared	0.849	0.888	0.849	0.886

Table 12. Political embeddedness

This table reports regression results examining how political connections moderate the effect of local economic growth shortfalls on firm-level carbon emission intensity. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	Politically	Non-Embedded	Politically	Non-Embedded
$Pressure_{t-1}$	0.031**	0.007	0.031**	0.002
	(0.015)	(0.019)	(0.015)	(0.020)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.848	0.905	0.846	0.906
Observations	11,170	6,394	11,170	6,394

Table 13. Industry pollution Intensity

This table reports heterogeneity in the effect of local economic growth shortfalls on firm-level carbon emission intensity across industries with different pollution intensities. HP denotes heavily polluting firms. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	HP	Non-HP	HP	Non-HP
$Pressure_{t-1}$	-0.004	0.045***	-0.008	0.044***
	(0.019)	(0.015)	(0.019)	(0.016)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.742	0.712	0.739	0.709
Observations	5,135	10,738	5,135	10,738

Table 14. Central environmental inspection

This table reports regression results examining how the Central Environmental Inspection (CEI) program moderates the relationship between local economic growth shortfalls and firm-level carbon emission intensity. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	CEI	Non-CEI	CEI	Non-CEI
$Pressure_{t-1}$	0.020	0.065**	0.020	0.056*
	(0.013)	(0.031)	(0.013)	(0.031)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	No	No
Industry FEs	Yes	Yes	No	No
Firm FEs	No	No	Yes	Yes
Adjusted R-squared	0.910	0.402	0.909	0.406
Observations	11,748	5,797	11,748	5,797

Table 15. Carbon reduction priority

This table reports two-stage instrumental variable (IV) estimates examining whether local governments' carbon reduction priority (CRP) mediates the effect of economic growth pressure on firm-level carbon emissions. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Dependent Variable	CRP	Carbon
Model	(1)First-stage	(2)Second-stage
$Pressure_{t-1}$	-2.510***	
	(0.454)	
CRP		-0.009*
		(0.005)
Controls	Yes	Yes
Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Kleibergen-Paap rk F		32.379
Kleibergen-Paap LM		30.538
Cragg-Donald Wald F		42.703
Adjusted R-squared		0.845
Observations	13,638	13,306

Table 16. Growth pressure and environmental enforcement

This table examines the effect of lagged growth pressure on local environmental enforcement outcomes. Coefficients are listed in the first row, and standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Model	(1)	(2)	(3)	(4)
Dependent Variable	No. of Cases	No. of Cases	Penalty Amount	Penalty Amount
$Pressure_{t-1}$	-0.340***	-0.319***	-0.823***	-0.746***
	(0.033)	(0.033)	(0.080)	(0.083)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	No	Yes	No
Industry FEs	Yes	No	Yes	No
Firm FEs	No	Yes	No	Yes
Adjusted R-squared	0.778	0.752	0.701	0.637
Observations	14,050	14,050	5,929	5,929

Table 17. Effect of local growth pressure on ESG scores

This table reports the effect of local growth pressure on firm-level ESG ratings. Columns (1) – (3) use alternative aggregate ESG rating measures from different data providers. The dependent variables are Huazheng ESG score, Wind ESG score, and Bloomberg ESG score. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively

Model	(1)	(2)	(3)
Dependent Variable	HuazhengESG	WindESG	BloombergESG
$Pressure_{t-1}$	-0.021	-0.023	0.285
	(0.028)	(0.018)	(0.637)
Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
City FEs	No	No	No
Industry FEs	No	No	No
Firm FEs	Yes	Yes	Yes
Adjusted R-squared	0.457	0.704	0.517
Observations	17,449	10,524	12,804

Table 18. Policy regime

This table reports the impact of local growth pressure on firm carbon intensity across different policy regimes. Coefficients are listed in the first row, and standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Model	(1)	(2)	(3)	(4)
Policy regime	Post-2016	Post-2016	Post-2020	Post-2020
$Pressure_{t-1}$	0.026**	0.024*	0.010	0.012
	(0.012)	(0.013)	(0.015)	(0.015)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	No	Yes	No
Industry FEs	Yes	No	Yes	No
Firm FEs	No	Yes	No	Yes
Adjusted R-squared	0.901	0.900	0.941	0.941
Observations	13,495	13,495	6,688	6,688

Table A1. Economic growth pressure and changes in environmental performance

This table reports descriptive data combining the Environmental Performance Index (EPI) reports and the Annual National Growth Index (ANNI). Columns (1) – (3) report countries' EPI scores for 2018, 2020, and 2022, respectively. Δ EPI (2022 – 2018) measures the cumulative change in environmental performance, where negative values indicate deterioration. Columns (5) – (6) report the annual growth rates of ANNI for 2018 and 2020. Across the sample, environmental performance tends to deteriorate following periods of weak or negative economic growth, a pattern observed in both developing and advanced economies.

	(1)	(2)	(3)	(4)	(5)	(6)
Country	EPI 2018	EPI 2020	EPI 2022	ΔEPI (2022-2018)	ANNI Growth 2018	ANNI Growth 2020
Switzerland	87.4	81.5	65.9	-21.5	2.09	-5.53
France	84	80	62.5	-21.5	1.61	-9.69
Sweden	80.5	78.7	72.7	-7.8	2.36	-2.01
UK	79.9	81.3	77.7	-2.2	2.19	-6.83
Germany	78.4	77.2	62.4	-16	1.36	-4.73
Spain	78.4	74.3	56.6	-21.8	2.31	-12.94
Italy	77	71	57.7	-19.3	1.47	-9.43
Australia	74.1	74.9	60.1	-14	2.89	-6.6
Canada	72.2	71	50	-22.2	2.33	-7.39
US	71.2	69.3	51.1	-20.1	2.26	-3.43
Philippines	57.7	38.4	28.9	-28.8	6.21	-9.58
Vietnam	47	33.4	20.1	-26.9	7.48	2.91
Turkey	53	42.6	26.3	-26.7	3.67	-9.43
Russia	63.8	50.5	37.5	-26.3	2.38	-3.23
Malaysia	59.2	47.9	35	-24.2	4.76	-6.62
China	50.7	37.3	28.4	-22.3	6.03	0.21
Brazil	60.7	51.2	43.6	-17.1	1.36	-2.65
Argentina	59.3	52.2	41.1	-18.2	2.98	-7.84
Indonesia	46.9	37.8	28.2	-18.7	3.88	-2.74
Mexico	59.7	52.6	45.5	-14.2	1.24	-11.44
Poland	64.1	60.9	50.6	-13.5	5.28	-2.89
Thailand	49.9	45.4	38.1	-11.8	4.12	-6.15
India	30.6	27.6	18.9	-11.7	6.47	-6.86
Chile	57.5	55.3	46.7	-10.8	3.94	-5.82
South Africa	44.7	43.1	37.2	-7.5	0.85	-4.5

Table A2: Variable definitions

Variable	Definition
Dependent variables	
Carbon emissions intensity	Firm carbon emissions scaled by operating revenue.
Independent variables	
Growth pressure	Dummy equal to 1 if a city's realized first-half gross domestic product growth falls below its announced annual gross domestic product growth target in the Government Work Report, 0 otherwise.
Firm size	Natural logarithm of total assets.
Financial leverage	Total liabilities divided by total assets.
Profitability	Net profit divided by total assets.
Asset turnover	Sales revenue divided by average total assets.
Operating cash flow ratio	Net cash flow from operating activities divided by total assets.
Inventory turnover	Net inventories divided by total assets.
Fixed asset turnover	Net fixed assets divided by total assets.
Revenue growth	Change in operating revenue from year t−1 to year t, scaled by operating revenue in year t−1.
Loss indicator	Dummy equal to 1 if net profit is negative, 0 otherwise.
Board independence	Number of independent directors divided by board size.
CEO duality	Dummy equal to 1 if the board chair concurrently serves as the chief executive officer (or general manager), 0 otherwise.
Tobin's Q	Market valuation measure from the accounting/market database used in the analysis.
Fixed effects	
Year FE	Dummies identifying the year.
City FE	Dummies identifying the city.
Industry FE	Dummies identifying the industry.
Firm FE	Dummies identifying the firm.

Table A3. Summary statistics

This table reports summary statistics for the sample, covering 17,564 firm-year observations from 2012 to 2022. The dependent variable is carbon emission intensity, defined as total firm-level carbon emissions divided by operating revenue. Pressure equals one if the city's realized GDP growth in the first half of year t is below the full-year growth target stated in its government work report, and zero otherwise.

Variable	N	Min	Mean	p25	p50	p75	Max
Carbon emissions intensity	17564	0.451	2.884	2.843	3.154	3.415	4.124
Growth Pressure	17564	0.000	0.098	0.000	0.000	0.000	1.000
Firm size	17564	19.563	22.389	21.399	22.166	23.142	26.452
Financial leverage	17564	0.052	0.420	0.256	0.412	0.565	1.143
Profitability	17564	-37.871	3.904	1.458	3.794	6.942	21.705
Asset turnover	17564	0.054	0.622	0.371	0.549	0.773	2.891
Operating cash flow ratio	17564	-0.199	0.052	0.014	0.050	0.091	0.266
Inventory turnover	17564	0.000	0.145	0.066	0.114	0.178	0.778
Fixed asset turnover	17564	0.002	0.218	0.103	0.189	0.300	0.725
Revenue growth	17564	-0.654	0.158	-0.020	0.106	0.259	3.808
Board independence	17564	28.570	37.737	33.330	36.360	42.860	60.000
CEO duality	17564	0.000	0.287	0.000	0.000	1.000	1.000
Tobin's Q	17564	0.803	2.006	1.221	1.595	2.286	16.647

Table A4. Using alternative baseline measures of trade-war exposure (2015–2017)

This table reports second-stage instrumental-variable robustness checks using alternative 2015–2017 pre-war measures of city-level China–U.S. trade war exposure. Coefficients are listed in the first row, t-statistics in parentheses. Economic growth pressure is instrumented by trade-war exposure × post-2018 indicator (first-stage results not reported). The dependent variable is firm-level carbon emission intensity, key regressor is lagged economic growth pressure. All specifications include full controls and firm and year fixed effects. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

	Export dependence			Direct Exports to the U.S		
Model	2015	2016	2017	2015	2016	2017
Dependent Variable	Carbon	Carbon	Carbon	Carbon	Carbon	Carbon
$Pressure_{t-1}$	0.312**	0.291**	0.322**	0.361**	0.363**	0.364**
	(0.143)	(0.143)	(0.146)	(0.173)	(0.172)	(0.173)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap rk F	547.15	523.19	515.37	547.41	547.21	547.88
Kleibergen-Paap LM	207.54	209.96	211.47	164.23	163.98	162.11
Centered R2	0.863	0.864	0.866	0.864	0.864	0.864
Observations	15,865	15,895	16,738	16,728	16,738	16,738

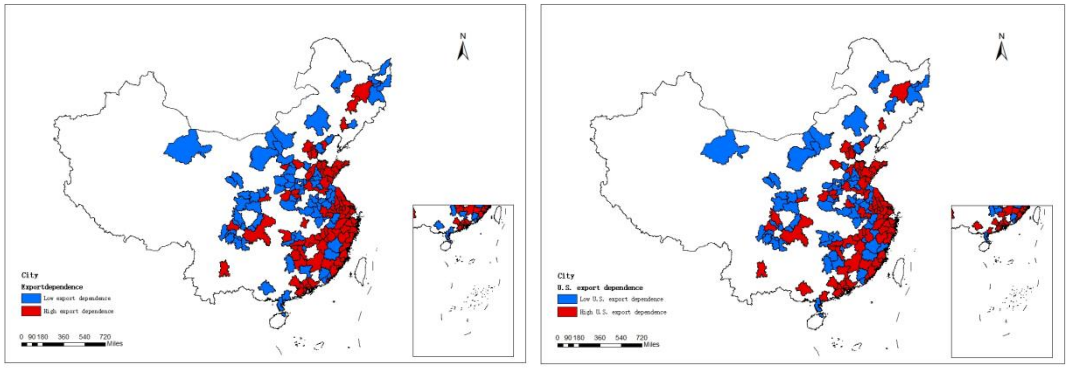


Figure A1. Spatial distribution of export dependence and U.S. export dependence

This figure maps the spatial distribution of city-level export dependence and U.S. export dependence across China before the China–U.S. trade war. Export dependence shows a distinct coastal-inland gradient, with coastal provinces having the highest external market exposure and thus greater vulnerability to the trade war's negative demand shock.

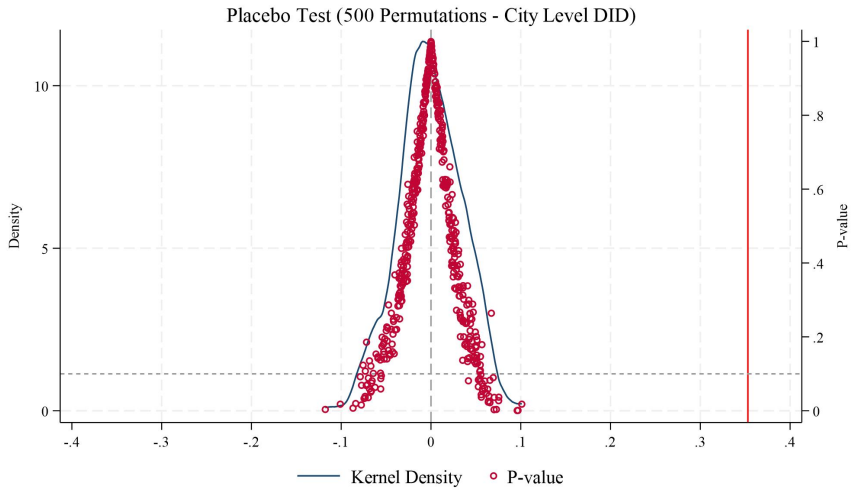


Figure A2. Placebo test

The figure plots the distribution of coefficients from 500 placebo regressions with randomly assigned policy shocks. The blue line shows the kernel density of placebo estimates and red dots denote p-values. The vertical red line marks the baseline estimate.

Table A5. Robustness checks

This table reports robustness check results for the effect of lagged economic growth pressure on firm-level carbon emission intensity. Coefficients are listed in the first row, t-statistics are reported in parentheses. Columns (1) redefine economic growth pressure using GDP growth in the third quarter (instead of the first half of the year). Columns (2) use an alternative dependent variable, where total carbon emissions are scaled by total assets (instead of operating revenue). Columns (3) report estimates with two-way clustered standard errors at the firm and year level. Columns (4) exclude first-tier cities (Beijing, Shanghai, Guangzhou, Shenzhen). Columns (5) exclude provincial-level municipalities. Odd-numbered columns include city, industry and year fixed effects; even-numbered columns include firm and year fixed effects. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Carbon	Carbon	Carbon	Carbon	Carbon
$Pressure_{t-1}$	0.021*	0.448***	0.024**	0.023*	0.025*
	(0.011)	(0.156)	(0.008)	(0.013)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.875	0.903	0.875	0.879	0.880
Observations	17,564	17,564	17,564	12,683	13,679

Table A6. Double machine learning estimation

This table reports results estimated using the Double Machine Learning (DML) framework. Coefficients are listed in the first row, standard errors clustered at the firm level are reported below in parentheses. Lasso, Elastic-Net, Random Forest, Neural Network, Ridge Regression, and Support Vector Machine are used to orthogonalize economic growth pressure from control variables. The dependent variable is carbon emission intensity. ***, ** and * indicate significance at 1%, 5% and 10% level, respectively.

Model	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Lasso	ElasticNet	RanForest	Neural	Ridge	SVM
$Pressure_{t-1}$	0.023**	0.024**	0.024**	0.030***	0.024**	0.022**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,183	17,183	17,183	17,183	17,183	17,183

Table A7. Carbon intensity based on IPCC method

This table reports the effects using carbon intensity based on an IPCC-style estimation as the dependent variable. Coefficients are listed in the first row, and standard errors clustered at the city level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Model	(1)	(2)
Dependent Variable	Carbon	Carbon
$Pressure_{t-1}$	0.021**	0.024*
	(0.010)	(0.013)
Controls	Yes	Yes
Year FEs	Yes	Yes
City FEs	Yes	No
Industry FEs	Yes	No
Firm FEs	No	Yes
Adjusted R-squared	0.980	0.977
Observations	11,635	11,635

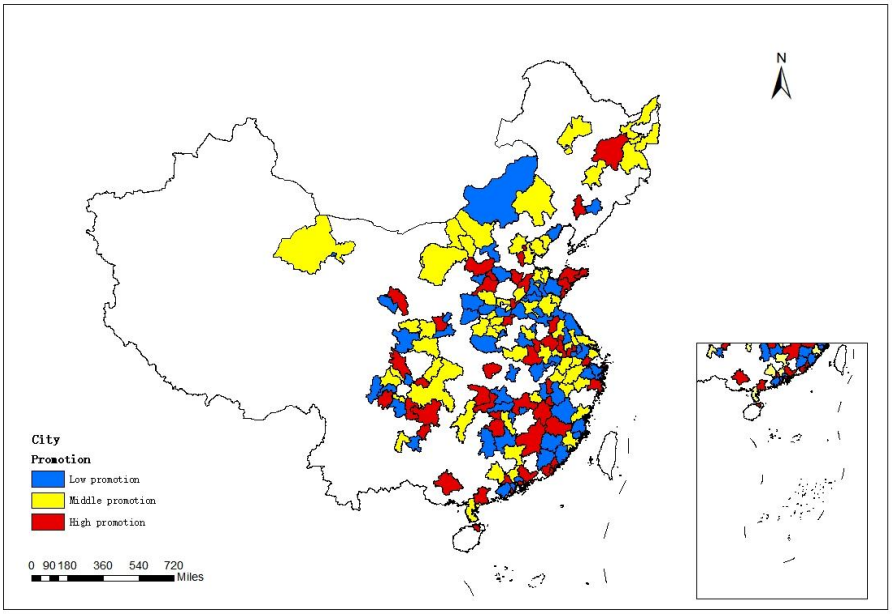


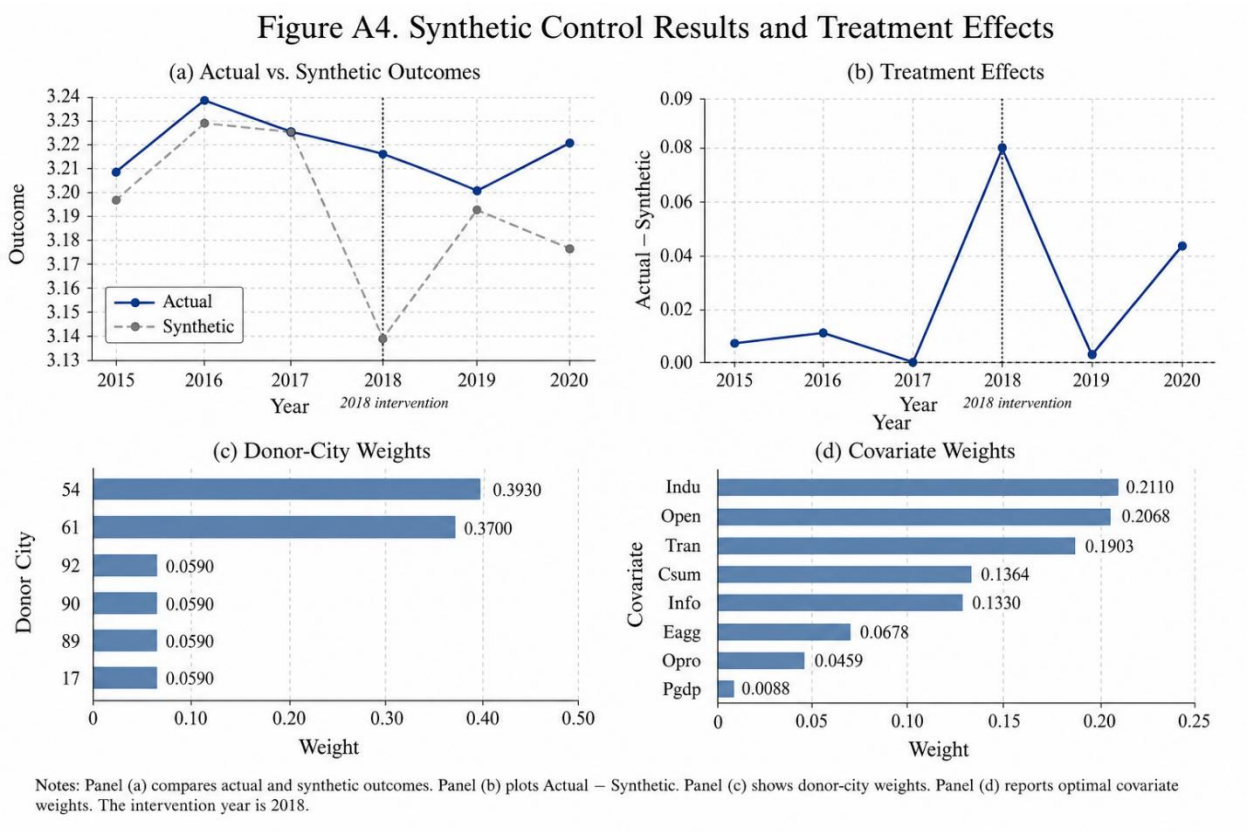
Figure A3. Spatial distribution of ex ante promotion probability among officials

This figure maps the spatial distribution of mayors predicted ex ante promotion probability across Chinese prefecture-level cities. Darker areas indicate higher ex ante promotion likelihood and stronger career concerns for local officials prior to policy shocks.

Table A8. Synthetic Control Results

Panel A reports the balance of predictor variables between the treated unit, the synthetic control, and the average of control units. Bias is defined as the percentage difference relative to the treated unit. Panel B reports the weights assigned to donor cities, which sum to one. Panel C reports treatment effects defined as the difference between observed and synthetic outcomes in the post-treatment period.

Panel A. Covariate Balance					
Variable	Treated	Synthetic	Bias (%)	Control Mean	Bias (%)
Pgdp	11.2088	11.2599	0.46	10.7112	-4.44
Indu	1.0467	0.9912	-5.3	1.2609	20.46
Tran	2.869	2.8651	-0.13	2.8208	-1.68
Info	14.2732	14.3352	0.43	13.4909	-5.48
Open	4.7425	4.3271	-8.76	2.8529	-39.84
Eagg	8899.03	6469.79	-27.3	2266.97	-74.53
Csum	10.4266	10.5676	1.35	9.7516	-6.47
Opro	7.4161	7.5881	2.32	7.0483	-4.96
Notes: Bias is defined as percentage difference relative to treated unit. RMSPE = 0.00462; R^2 = 0.8565.					
Panel B. Donor Weights					
City ID			Weight		
54			0.393		
61			0.37		
17			0.059		
89			0.059		
90			0.059		
92			0.059		
Others			0		
Notes: Weights sum to one.					
Panel C. Treatment Effects					
Year	Actual	Synthetic	Effect		
2018	3.2143	3.1341	0.0802		
2019	3.197	3.1949	0.0022		
2020	3.221	3.1779	0.043		
Mean	3.2108	3.169	0.0418		
Notes: Effect = Actual − Synthetic.					



Notes: Panel (a) compares actual and synthetic outcomes. Panel (b) plots Actual − Synthetic. Panel (c) shows donor-city weights. Panel (d) reports optimal covariate weights. The intervention year is 2018.

Figure A4. Synthetic control results and treatment effects

This figure reports the synthetic control weights and treatment effects. Panel (a) compares actual and synthetic outcomes, panel (b) plots the treatment effect (Actual − Synthetic) over time, panel (c) displays donor-city weights, and panel (d) shows optimal covariate weights. The intervention year is 2018.

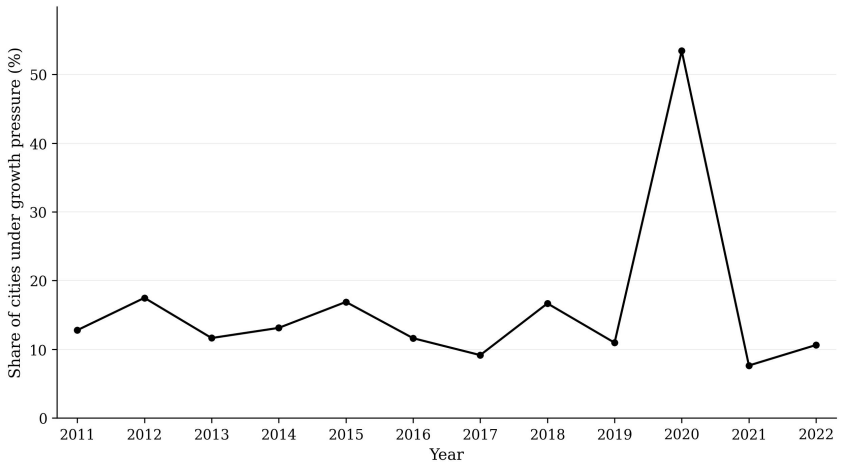


Figure A5. Annual share of cities under local growth pressure.

This figure plots the yearly share of cities classified as facing local growth pressure. The measure is defined at the city-year level. The figure shows that the incidence of growth pressure varies over time and rises sharply in 2020.

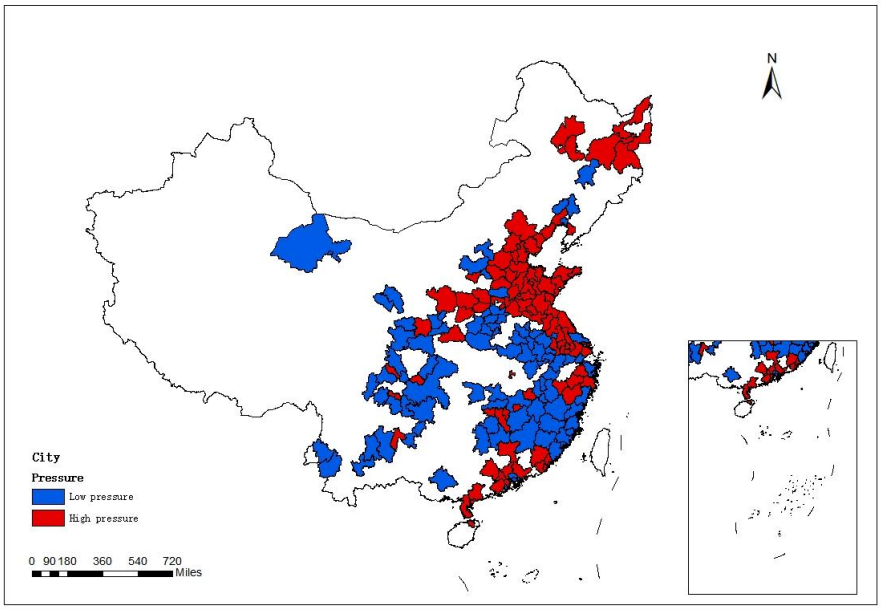


Figure A6. Spatial Distribution of Growth Pressure Across Chinese Cities

This figure plots the spatial distribution of growth pressure across Chinese cities in 2020. Growth pressure is defined as the shortfall of realized GDP growth relative to the official annual target. Cities are grouped into high-pressure and low-pressure categories. The analysis is based on cities with matched firm-level carbon emissions and financial data.