

Article

Policy Effect Evaluation of the Centralization of Environmental Supervision Authority: Evidence from China's Central Environmental Protection Inspection

Mengzhi Xu ¹, Jingxin Zhang ¹ and Qianming Zhang ^{2,3,*}

¹ School of Politics and Public Administration, Yunnan Minzu University, Kunming 650504, China

² School of Economics and Management, Chuxiong Normal University, Chuxiong 675000, China

³ School of Government Management, Yunnan University, Kunming 650500, China

* Correspondence: zqm1986@cxnc.edu.cn

Abstract

The Central Environmental Protection Inspection (CEPI) represents a landmark institutional arrangement for the centralization of environmental supervision authority from local governments to the central government. It has played a significant role in correcting the distortion of local government incentives for environmental governance under decentralized management. However, the persistence and spatial heterogeneity of its policy effects remain to be further examined. This paper treats the CEPI, which was progressively implemented from 2016, as a quasi-natural experiment of centralizing environmental supervision authority. Using daily city-level panel data from 333 Chinese cities over the period 2015–2022, we employ a multi-cutoff time regression discontinuity design in time to evaluate its policy effects. The findings are threefold. First, the CEPI significantly reduces urban concentrations of AQI, PM_{2.5}, PM₁₀, SO₂, NO₂, and CO, while O₃ concentrations increase significantly over the same period, indicating that the centralization of supervision authority is effective in mitigating conventional primary pollutants but faces limitations in addressing secondary pollutants. Second, the policy effects vary across inspection rounds, with the first round and the “look-back” generating larger short-run coefficients than the second round. These differences in short-run inspection-period coefficients across rounds indicate that marginal policy responsiveness diminishes as the inspection program becomes institutionalized. Third, the policy effects display significant spatial heterogeneity: cities in northern regions, resource-based cities, and cities with historically moderate air quality experience more pronounced improvements, while the grouping effect of administrative hierarchy is not statistically significant for most pollutants. This indicates that the governance effects of centralizing supervision authority vary across city characteristics but do not simply follow an administrative hierarchy gradient. This study provides empirical evidence for understanding the boundary of the effectiveness of centralizing environmental supervision authority and offers policy implications for optimizing the vertical allocation of environmental governance responsibilities.

Keywords: environmental supervision authority; central environmental protection inspection; air quality; regression discontinuity design

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1. Introduction

China's environmental governance system is undergoing profound institutional transformation, with its core lying in the restructuring of authority and responsibility between the central and local governments. The vertical allocation of environmental governance responsibilities is a central issue in environmental federalism. Theoretically, the decentralization of authority enables local governments to formulate differentiated policies according to local preferences [1]. However, when environmental pollution exhibits transboundary externalities, local governments' governance incentives may be insufficient, leading to a "race to the bottom" [2]. How to overcome local incentive distortions while preserving informational advantages constitutes the fundamental tension in the optimal allocation of environmental authority. As a key initiative in this transformation, the centralization of supervision authority is essentially an important institutional innovation aimed at addressing the structural contradiction between economic development and ecological protection. This reform is deeply rooted in an institutional context—the "tiao-kuai" (vertical-line and horizontal-block) governance structure with Chinese characteristics, which, during the early stages of reform and opening-up, effectively unleashed local economic vitality by strengthening local autonomy while weakening vertical management. However, as the stage of development has shifted, the contradiction between local governments' pursuit of economic growth and ecological protection has become increasingly acute. Against the backdrop of this "institutional failure" in environmental governance, the launch of the Central Environmental Protection Inspection (CEPI) in 2016 represents a milestone. From the perspective of authority allocation, the essence of the CEPI lies in transferring environmental supervision authority from local governments to the central government, thereby correcting the institutional deficiency of insufficient environmental governance incentives under decentralized local governance through direct central oversight of local governments [3]. Therefore, the CEPI is not merely a specific environmental regulation policy, but also a natural experiment for testing the validity of the institutional logic of centralizing environmental authority—that is, under what conditions the centralization of environmental supervision authority can effectively improve environmental governance performance. Against this backdrop, an in-depth investigation into the environmental governance effects of the CEPI not only helps to reveal the boundary of effectiveness of environmental governance but also provides policy implications for improving the ecological environmental governance system and optimizing the vertical allocation of environmental authority.

The vertical allocation of environmental authority is a core issue in environmental federalism. Classical environmental federalism theory posits that the allocation of environmental authority should strike a balance between efficiency and equity [1]. When environmental problems are localized, decentralization can better reflect local preferences and cost information; however, when pollutants are transboundary in nature, centralization tends to be more efficient [4]. This theoretical divergence reveals a fundamental tension: both centralization and decentralization of environmental authority have their respective advantages and disadvantages, and their optimal allocation depends on the spatial scope of pollutants, the governance capacity of local governments, and the information structure between central and local governments [5,6]. How to overcome local incentive distortions while preserving informational advantages constitutes the fundamental challenge in the optimal allocation of environmental authority. Empirical studies in the Chinese context have revealed the complex effects of environmental decentralization. Some studies find that environmental decentralization is detrimental to pollution reduction [7,8], through mechanisms such as the "race to the bottom" and soft budget constraints [6]; other studies suggest that moderate environmental decentralization can improve governance efficiency [9]. This divergence indicates that decentralization itself is

not the decisive factor in governance outcomes; rather, the specific configuration of authority division and its accompanying institutional arrangements matter more. If decentralization fails to effectively address local government incentive distortions, alternative institutional arrangements must be sought.

Facing the structural limitations of decentralized governance, the central government has, through institutional innovations such as environmental inspections, admonitory talks, and accountability mechanisms, effectively promoted the centralization of environmental supervision authority, constituting a “re-centralization” reform [10,11]. The theoretical logic underlying this reform direction is that when local governments’ environmental governance incentives are systematically distorted by competition for economic development, the central government can break the local-level “race to the bottom” equilibrium and internalize the externalities of environmental governance by centralizing supervision authority and strengthening vertical constraints. Regarding the institutional practices of authority centralization, scholars have extensively discussed the effects of the establishment of Regional Environmental Supervision Centers in 2002 and the CEPI in 2015 on environmental governance. The establishment of Regional Environmental Supervision Centers shifted environmental supervision from a parallel to a vertical mode, and the inspection authority for environmental protection began to be reclaimed by the central government, thereby strengthening central environmental inspection authority [10]. However, whether the establishment of Regional Environmental Supervision Centers effectively improved environmental governance within their jurisdictions remains contested in the literature. Some studies find that regional environmental supervision did not significantly improve air pollution within jurisdictions [12]. Given that the responsibilities of the supervision centers particularly focus on boundary pollution, other studies find that their establishment effectively curbed pollutant discharges from enterprises in boundary areas [13]. Some scholars have also questioned the effectiveness of the Regional Environmental Supervision Centers, arguing that “approximately 300 staff members supervising environmental work across 31 provinces, municipalities, and autonomous regions” are overstretched [14].

From regional supervision to central inspection, the centralization of environmental supervision authority in China has undergone institutional deepening and upgrading. If regional supervision represents the “primary form” of authority centralization, then the CEPI represents its “advanced form.” However, does the upgrading of institutional design necessarily lead to improved governance outcomes? Since the launch of the CEPI in 2016, scholars have conducted extensive discussions on the CEPI and environmental governance. In terms of research content, existing studies can be divided into two categories: mechanism analysis and effect evaluation. One strand focuses on the mechanisms through which the CEPI affects environmental governance [15,16]; the other focuses on the policy effect evaluation of the CEPI, with most studies concluding that the CEPI improved environmental governance in the short term. In terms of research scale and methods, macro-level studies focus on provinces and cities, typically using difference-in-differences (DID) methods [17] or textual analysis [18]; micro-level studies employ fuzzy-set qualitative comparative analysis (fsQCA) to examine governmental institutional response differences [19]. In terms of research subjects, the existing literature covers both “enterprise supervision” effects and “government supervision” effects. The “enterprise supervision” dimension focuses on corporate environmental investment [20] and the exit of polluting firms [12]. The “government supervision” dimension emphasizes air pollution governance effects [18,21,22]. In terms of research conclusions, most studies find that the CEPI improved environmental governance, but debate persists regarding the persistence of its effects—

namely, that campaign-style governance faces a “deterrence-effect diminishment” dilemma [23,24]. However, this conclusion is primarily based on evidence from the first two rounds of inspections.

In summary, the existing literature has laid an important foundation for understanding the environmental effects of the CEPI, but three gaps remain. In terms of theoretical perspective, most studies treat the CEPI as an isolated environmental regulation policy rather than a vehicle for institutional change. The theoretical logic of centralizing environmental authority has yet to be fully incorporated into the analytical framework of the CEPI—most discussions remain at the level of descriptive policy effects, failing to reveal the theoretical logic of the CEPI as a landmark institutional arrangement for centralizing environmental supervision authority from local to central governments. In terms of identification strategy, existing studies predominantly employ difference-in-differences methods [17,24] which may face identification challenges such as policy spillovers and non-random treatment assignment. In terms of dynamic effects, existing research has largely focused on effect estimation of the first two rounds of inspections [23], lacking systematic comparison of subsequent rounds and thus making it difficult to determine whether policy effects change over time. To address these gaps, this paper adopts the theoretical perspective of environmental authority allocation, treating the CEPI as a quasi-natural experiment of centralizing environmental supervision authority. We employ a multi-cutoff time regression discontinuity design in time, using the inspection start dates as exogenous cut-offs, and evaluate the governance effects, dynamic trends, and heterogeneity of the CEPI on air quality based on daily panel data from 333 Chinese cities over the period 2015–2022.

2. Institutional Background and Research Hypotheses

Protecting the ecological environment is a fundamental responsibility of government. When local governments or environmental protection departments fail to fulfill their duties in ecological environmental protection, the central government and its superior environmental authorities have the power to hold them accountable—this constitutes the basic logic of environmental supervision. Since the establishment of regional supervision centers in 2006, China’s environmental supervision system has gradually transitioned from a pattern of “weak vertical lines and strong horizontal blocks” to one of “strong vertical lines and weak horizontal blocks.” However, regional environmental supervision centers have been frustrated in their efforts to counteract “local protectionism.” As subordinate public institutions of the Ministry of Environmental Protection, the regional supervision centers have been characterized by relatively low organizational hierarchy, limited supervisory scope, and insufficient accountability enforcement, rendering them largely ineffective in combating “local protectionism” [14]. The experience of regional supervision suggests that partial centralization of supervisory authority, without adequate political authority backing, cannot fundamentally break the equilibrium of the local-level “race to the bottom.” To reclaim authority from local governments, the CEPI was officially implemented in 2016 and has since conducted three rounds of inspections across all provinces and municipalities nationwide. Compared with the regional supervision centers, the CEPI has achieved a complete centralization of inspection authority—from “department-level deployment” to “central government leadership,” from “professional guidance” to “shared Party-government responsibility,” and from “limited supervision” to “full coverage”—representing the highest form of centralizing environmental supervision authority (Table 1). Since the implementation of the CEPI, environmental inspection has evolved from a departmental initiative to a national strategy, representing the most authoritative exercise of environmental supervision power in contemporary China.

Table 1. Institutional Comparison of the Upward Shift of Environmental Supervision Authority.

Item	Regional Environmental Supervision (Before)	Central Environmental Protection Inspection (After)
Time	2002–2015	2016–present
Objectives	Implement environmental laws, regulations and central environmental policies; inspect and supervise polluting enterprises' compliance with environmental rules.	Implement the central government's decisions and arrangements on ecological civilization; rectify the territorialization bias in environmental governance; supervise local governments' enforcement of environmental laws and performance of environmental accountability.
Administrative Level	Departmental action; public institution dispatched by the former Ministry of Environmental Protection.	National strategic initiative; centrally led.
Supervision Approaches	Establish regional environmental supervision centers; conduct on-site inspections, document reviews, and handle public complaints.	On-site inspection by central inspection teams, grassroots targeted supervision, field return visits, public complaint acceptance, and verification of problem clues.
Supervision Targets	Dominated by local enterprises; local people's governments (after 2014).	Local Party committees, local people's governments, competent departments of the State Council, and central state-owned enterprises (after 2019).
Supervision Characteristics	Low administrative rank, limited coverage and weak enforcement; implemented mainly based on local environmental regulations and policies.	High administrative rank, nationwide coverage and strong enforcement; implementing the principle of dual accountability for Party and government officials and dual duties for one post.
Information Disclosure	Publicity mainly undertaken by local governments with limited social influence.	Inspection progress disclosed via press conferences, provincial TV stations, provincial Party newspapers and official government websites; higher-level publicity, greater outreach and wider coverage; extensive application of new media platforms such as Weibo and WeChat.
Public Participation	No dedicated channels for public participation.	Special hotlines and email addresses set up to receive public reports; handling results publicly released to accept social oversight.
Relationship with Local Governments	Direct liaison with local environmental bureaus without guiding their routine operations.	Direct coordination with local Party committees and government departments related to inspection tasks; guide the daily work of local environmental authorities; empowered to hold local governments and affiliated departments accountable.

Source: Compiled based on the relevant literature and official documents.

Examined from the theoretical perspective of environmental authority allocation, the institutional logic of the CEPI can be understood within the dual framework of environmental federalism and principal-agent theory. Environmental federalism posits that the transboundary externality of pollution determines the structural failure of decentralized governance, necessitating the upward transfer of supervision authority to higher-level governments to internalize the externality [4]—the CEPI is precisely a product of this institutional logic. However, the upward transfer of authority is merely a necessary condition. Principal-agent theory further reveals that the inconsistency in objective functions between the central and local governments, coupled with information asymmetry, gives rise to strategic behaviors such as selective implementation. The core issue lies in how to design effective supervisory incentives to align agent behavior with the principal's environmental governance objectives [25]. The institutional design of the CEPI addresses this agency problem through three mechanisms. First, the elevation of organizational authority compresses information asymmetry. The CEPI's authority derives directly from the Party Central Committee, with inspection teams led by ministers at the provincial-ministerial level, achieving hierarchical superiority over local governments. This enables inspection teams to penetrate local information barriers and obtain first-hand information, thereby compressing the space for "hidden information." Second, the expansion of accountability subjects reshapes the agent's objective function. The CEPI extends supervision from "enterprise supervision" to "government supervision," covering local Party committees and establishing the principles of "shared Party-government responsibility" and "dual accountability." This elevates environmental governance from "performance evaluation" to "political assessment," directly linking it to official promotion, transforming environmental constraints from "soft" to "hard," and correcting the imbalance in objective weighting. Third, the systematic transmission of pressure narrows the space for strategic behavior. The closed-loop system of inspection deployment—feedback and rectification—accountability "look-back," coupled with information disclosure and social oversight, establishes credible commitments to "ex post punishment," transforming environmental governance from "selective implementation" to "binding constraints." In sum, the CEPI corrects the distortion of local environmental governance incentives through three pathways: compressing information asymmetry, reshaping the objective function, and narrowing the space for strategic behavior. Based on this, we propose our first hypothesis:

Hypothesis 1: *The CEPI contributes to the improvement of air quality.*

However, according to organizational learning theory [26,27], in the early stages of policy implementation—characterized by high goal ambiguity and operational uncertainty—public organizations tend to adopt precautionary strategies that exceed policy expectations to avoid accountability risks, resulting in "overshooting." As policies become routinized, organizations accumulate experience through "learning by doing" and update their expectations, gradually developing standardized response patterns, with the intensity of responses correspondingly diminishing [28]. Applied to the CEPI context, in the early inspection stages, local governments have limited information regarding inspection formats, content, and accountability standards; facing high uncertainty, they tend to adopt more stringent and comprehensive governance measures. As the inspection system continues to operate, local governments deepen their understanding of inspection rules, their responses become increasingly standardized, and the marginal governance intensity declines. Accordingly, we propose our second hypothesis:

Hypothesis 2: *The marginal effect of the CEPI diminishes over time.*

We further note that the improvement effect of the CEPI on air quality is systematically moderated by urban resource endowments. Theoretically, when cities face external policy shocks, their response speed and magnitude of improvement depend on marginal governance costs and institutional implementation capacity. Accordingly, this paper examines the spatial heterogeneity of inspection effects across four dimensions: geographic location, resource endowment, historical governance foundation, and administrative hierarchy. In terms of geographic location, southern cities, benefiting from favorable atmospheric diffusion conditions and lighter industrial structures, exhibit lower marginal governance costs; northern cities, constrained by reliance on coal combustion during heating seasons, have higher pollutant emission baselines and relatively limited policy response elasticity [23]. In terms of resource endowment, non-resource-based cities, facing lower industrial transition costs, can respond more rapidly to environmental policies; resource-based cities, constrained by path dependence, experience industrial structure lock-in effects that weaken their short-term responsiveness to inspection pressure. In terms of governance foundation, cities with historically better air quality, already operating at lower emission levels, have limited further improvement space, making it difficult for marginal effects to achieve statistical significance; cities with historically moderate or poor air quality, having greater governance space, exhibit more pronounced marginal effects. In terms of administrative hierarchy, while higher-administrative-level cities possess stronger fiscal capacity and regulatory resources, the grouping effect is not statistically significant for most pollutants, indicating that inspection effects do not simply increase with administrative hierarchy. These differences stem from the combined effect of two factors: first, the pollutant emission baseline—cities with higher initial pollution levels have greater governance space, and the inspection's reduction effect on their high emission baseline is more pronounced; second, marginal governance costs—cities with lighter industrial structures and better governance foundations, although more responsive, have limited further improvement space due to their lower emission baselines [29,30]. Based on this, we propose our third hypothesis:

Hypothesis 3: *The policy intervention of the CEPI on air quality exhibits significant spatial heterogeneity.*

3. Research Design

3.1. Identification Strategy: Regression Discontinuity Design

The pilot inspection conducted by the CEPI team in Hebei Province from 31 December 2015 to 4 February 2016 can be theoretically regarded as a quasi-natural experiment. Before the inspection, the inspection targets were not known in advance, thus eliminating self-selection effects. Meanwhile, other cities remained in a wait-and-see state, and the policy spillover effects were relatively limited. Had it been possible to identify a control group with a similar air quality trend to Hebei through matching methods at that time, a difference-in-differences (DID) approach could have been employed to estimate the causal effects. However, the validity of DID estimation relies on the “parallel trends” assumption, which requires that the treatment and control groups maintain the same trend in the absence of policy intervention. This condition became difficult to satisfy as the CEPI progressed. Since the first round of inspections achieved nationwide coverage across all 31 provinces and regions in August 2017, and the second round of inspections again covered the entire country by March 2022, there no longer existed areas unaffected by the policy to serve as valid control groups. At the same time, the frequent accountability notifications appearing in the feedback of each round of inspections—ranging from departmental to county-level officials—indicate that the political pressure of the inspections has been

transmitted nationwide, with significant policy spillover effects. Under these circumstances, the “treatment-control” dichotomy required by DID is no longer valid.

To address the above identification challenges, this paper employs a multi-cutoff time regression discontinuity design (multi-cutoff time RDD). In this design, each city's inspection has its own independent cutoff (i.e., the inspection start date) [31–33]. All cities are aligned along the event-time axis to a common cutoff at $d = 0$ for estimation. This design is equivalent to aggregating the average effects across multiple cutoffs in the event-time dimension and exploits more cross-city variation in policy timing than a single-cutoff design. The estimation model is specified as follows:

$$y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 d_{it} + \beta_3 T_{it} \times d_{it} + \gamma x_{it} + \delta_i + \eta_t + \varepsilon_{it} \quad (1)$$

In Equation (1), the explained variable y_{it} denotes the air quality of city i on day t . T_{it} is the policy dummy variable for the CEPI, indicating whether city i was subject to the CEPI on day t . d_{it} represents the forcing variable, measured as the number of days away from the inspection date. $\beta_2 d_{it} + \beta_3 T_{it} \times d_{it}$ controls for the time trend effects on air quality on both sides of the breakpoint. A set of control variables x_{it} are incorporated into the model. City fixed effects δ_i and monthly fixed effects η_t are also included. The model also contains a residual term ε_{it} and a constant term β_0 . The core parameter β_1 denotes the parameter of primary interest in this study, measuring the average short-run differential in air quality between the inspection period and adjacent non-inspection periods—that is, the direct causal effect of the CEPI on air quality, net of the influence of covariates, and time trends.

3.2. Sample Selection

This study adopts daily data at the city level. After data matching and screening, 333 cities are included in the final sample. The research period covers the first round of CEPI, its reinspection, and the second round. Given that the inspection status of each city changes multiple times throughout the campaign, this study sets the observation window for each round to extend at least three months before and after the official inspection period. This design avoids overlapping observation intervals across different rounds and enables comparative analysis of their policy effects. The first round of CEPI was implemented from 31 December 2015 to 15 August 2017, with its corresponding observation period covering January 2015 to December 2017. The first-round reinspection was carried out from 30 May to 6 November 2018, and its observation period ranges from January 2018 to February 2019. The second round of CEPI was implemented from 11 July 2019 to 25 March 2022, with the observation period extending from January 2019 to December 2022. The full sample period covers 1 January 2015 to 31 December 2022, totaling 2922 days. With 333 prefecture-level and above cities, the theoretical maximum sample size is 973,026. After excluding observations with missing variables, the final regression sample consists of 895,608 city-day observations.

3.3. Variable Definition and Data Sources

3.3.1. Dependent Variable

The Air Quality Index (AQI) is employed to measure urban air quality. The AQI quantitatively reflects the degree of air pollution and encompasses six major pollutants: PM_{2.5}, PM₁₀, SO₂, NO₂, CO and O₃. The AQI ranges from 0 to 500 and is divided into six evaluation grades; a higher value indicates poorer air quality, while a lower value repre-

sents better air quality. The raw AQI and pollutant data are retrieved from the Tianqihoubao website (<http://www.tianqihoubao.com/aqi/>) (accessed on 11 July 2024), with original sources officially released by the Ministry of Ecology and Environment of China.

3.3.2. Core Explanatory Variable

The core explanatory variable T_{it} in this paper is whether a city is under the CEPI. The running variable d_{it} is defined as the event time, measured as the number of days between the actual observation date and each city's inspection start date: $d_{it} = Date_{it} - Inspection\ Start\ Date_i$, where $Inspection\ Start\ Date_i$ denotes the official start date of the inspection group for the province to which city i belongs. The treatment indicator T_{it} is defined as $T_{it} = 1$ ($d_{it} \geq 0$), taking the value 1 when $d_{it} \geq 0$ (during or after the inspection period) and 0 when $d_{it} < 0$ (prior to the inspection). It is important to note that $T_{it} = 1$ covers not only the inspection start date ($d_{it} = 0$) but also all subsequent days with $d_{it} > 0$ until the inspection ends. Therefore, β_1 captures the average treatment effect during the inspection period, leveraging the local randomness near the cutoff to compare outcome differences between the inspection period ($T_{it} = 1$), and the non-inspection period ($T_{it} = 0$) in the vicinity of the cutoff.

A specific coding example is as follows: For the first inspection round that entered Hebei Province on 31 December 2015, and exited on 4 February 2016, the period from 31 December 2015 to 4 February 2016 is coded as $d = 0$, $T = 1$ for all cities in Hebei; 30 December 2015 is coded as $d = -1$, $T = 0$; 29 December 2015 as $d = -2$, $T = 0$; 5 February 2016 as $d = 1$, $T = 1$ (still within the inspection period); 6 February 2016 as $d = 2$, $T = 1$; and so on, until $T = 0$ after the inspection exit date. The data on the CEPI are obtained from the official website of China's Ministry of Ecology and Environment. Since the specific dates of inspection entry for each city are not publicly released, this paper equates each city's inspection period with that of its province.

It should be noted that this paper employs a multi-cutoff time regression discontinuity design, rather than a standard single-cutoff RDD. In a standard RDD, treatment changes permanently at the cutoff; in this paper, however, treatment switches from 0 to 1 on the inspection entry date, and from 1 back to 0 on the inspection exit date. Consequently, β_1 captures the average short-run difference between the inspection period and neighboring non-inspection periods, rather than a Local Average Treatment Effect (LATE) at the cutoff. This paper does not further distinguish daily dynamics within the inspection period or the persistence of effects after the inspection concludes for two reasons. First, our core research question is whether the Central Environmental Protection Inspection improved air quality, and estimating the average treatment effect during the inspection period suffices to answer this question. Second, the behavioral responses of local governments and firms during the inspection period may vary with the duration of the inspection team's stay, the identification of which would require higher-frequency data structures.

3.3.3. Control Variables

Following the exogeneity principle, control variables are selected from meteorological conditions and human interference factors.

Meteorological factors are important confounders affecting ambient pollutant concentrations. Temperature significantly regulates PM_{2.5} and O₃ concentrations by influencing atmospheric chemical reaction rates and secondary aerosol formation processes; wind speed and temperature affect the horizontal and vertical dispersion of pollutants; precipitation removes particulate matter through wet deposition; and temperature stratification

alters the vertical dispersion space of pollutants by affecting boundary layer height [34–37]. Failure to control for these meteorological variables would confound the estimated policy effects with short-term weather fluctuations. Accordingly, this paper adopts daily precipitation (Precip), daily average temperature (Temp), daily maximum temperature (MaxTemp), daily minimum temperature (MinTemp), and daily average wind speed (Wind) as meteorological control variables. The meteorological data are obtained from the U.S. National Centers for Environmental Information (NCEI). We extract data from ground observation stations in China, generate daily grid data using inverse distance weighting (IDW) interpolation, and then aggregate to the city-day level by zoning statistics according to administrative boundaries.

In terms of human disturbances, statutory holidays are the primary observable factor affecting short-term air quality. During holiday periods, industrial production and office activities decline, but residential travel, tourism, and dining activities may increase, producing a combined effect on pollutant emissions with ambiguous directionality [38]. Accordingly, this paper constructs a statutory holiday dummy variable (StatHoliday), coded as 1 for holidays and 0 otherwise, based on the holiday schedules published annually by the General Office of the State Council. In addition, economic structural characteristics such as geographic location, industrial structure, GDP, energy consumption structure, and vehicle ownership typically vary at the annual level and exhibit limited short-term fluctuations at the daily frequency. To mitigate omitted variable bias, this paper controls for city fixed effects to absorb time-invariant or slowly changing city-specific characteristics. Given the daily frequency of our data, the choice of time fixed effects requires a balance between precision and estimation efficiency: overly coarse time fixed effects (e.g., quarterly or annual) fail to adequately capture seasonal fluctuations, while overly fine time fixed effects (e.g., weekly) may introduce estimation noise due to an excessive number of time dummies. Therefore, this paper employs month fixed effects to absorb confounding influences from nationwide policy shocks, macroeconomic cycles, and seasonal fluctuations.

Detailed descriptive statistics of all variables are presented in Table 2.

Table 2. Descriptive Statistics of Variables.

Type	Var	Unit	Mean	Std. Dev.	Min	Max
<i>Dependent variables</i>	AQI	-	65.98	44.16	0	500
	PM2.5	µg/m ³	38.49	35.50	0	1782
	PM10	µg/m ³	70.34	71.36	0	3176
	SO ₂	µg/m ³	14.45	17.04	0	800
	NO ₂	µg/m ³	25.92	15.87	0	471
	CO	mg/m ³	0.85	0.490	0	18.92
	O ₃	µg/m ³	60.94	29.14	0	435
<i>Independent variables</i>	T	-	0.03	0.18	0	1
<i>Control variables</i>	Precip	mm	3.18	7.400	0	278.1
	Temp	°C	14.48	10.99	−37.34	36.43
	MinTemp	°C	9.35	11.65	−43.48	30.7
	MaxTemp	°C	19.77	10.86	−27.59	42.23
	Wind	m/s	8.96	3.290	2.02	32.23
	StatHoliday	-	0.32	0.460	0	1

4. Results Analysis

4.1. Graphical Evidence and Covariate Balance Test for Regression Discontinuity Design

Prior to the formal estimation of the regression discontinuity in time, this paper first presents graphical evidence of the changes in AQI on either side of the cutoff. To balance fitting precision and estimation stability, and drawing on the optimal bandwidth selection method of Imbens and Kalyanaraman (2012) [39] in conjunction with the characteristics of our sample distribution, we select 45 days as the bandwidth for graphical presentation. Specifically, we conduct local linear fitting using 30-day, 45-day, and 60-day bandwidths as alternatives. The results show that the direction of the jump at the cutoff is consistent across different bandwidths, with the 45-day bandwidth yielding smoother fitted curves on both sides of the cutoff, which facilitates a more intuitive visualization of the AQI variation pattern. Therefore, we adopt the 45-day bandwidth for plotting the discontinuity graph. Figure 1 reports the nonlinear relationship of AQI on both sides of the cutoff, without adding any control variables, after aligning all cities along the event-time axis to their respective inspection start dates ($d_{it} = 0$), with the vertical line corresponding to the inspection deployment date. As shown in Figure 1, AQI exhibits a clear downward jump at the inspection deployment point, indicating a declining trend in AQI after the inspection, which is consistent with the negative effect estimated in the subsequent regression discontinuity analysis. It should be noted that this figure provides only descriptive visual evidence and serves as a preliminary reference for the subsequent regression discontinuity analysis; formal causal inference relies on the parametric regression discontinuity results presented below.

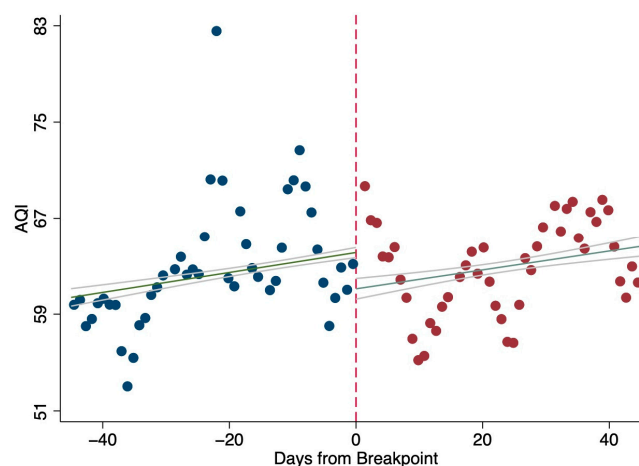


Figure 1. AQI Trends Around the Event-Time Cutoff. Note: This figure is constructed by aligning daily AQI observations across all cities along the event-time axis, with each city's CEPI start date serving as the cutoff ($d_{it} = 0$). Circles represent the average AQI value within each bin; the green line denotes local linear fitting; the shaded area between the gray lines indicates the 95% confidence interval.

To ensure the validity of the regression discontinuity design, this paper further conducts a balance test on the covariates. Table A1 in the Appendix A reports the smoothness test results of the covariates at the cutoff. The 95% confidence intervals for all covariates contain zero, and the mean differences between the left and right sides of the cutoff for each covariate are not statistically significant ($p > 0.1$), indicating that there is no significant jump in covariates at the cutoff. This satisfies the continuity assumption of RDD and verifies the validity of the regression discontinuity design employed in this paper.

4.2. Baseline Regression: CEPI Significantly Improves Air Quality

The inspection cycle of the CEPI teams in each province is typically one month. Accordingly, this paper adopts a bandwidth of 30 days (window period) for the baseline regression. The core identification strategy of RDD relies on local randomness around the cutoff. Although global polynomials have stronger fitting capacity, they are susceptible to time-varying confounders such as meteorological factors in regions far from the cutoff, thereby reducing estimation precision. Given the local identification nature of RDD, and drawing on existing studies [33], this paper adopts a first-order linear functional form for the running variable and employs the rectangular kernel function for local linear regression. Table 3 reports the impact of CEPI on air quality under a 30-day bandwidth while controlling for various fixed effects. Columns (1)–(2) show that CEPI intervention significantly reduces the AQI, implying an improvement in urban air quality and providing empirical support for research hypothesis 1.

Different air pollutants vary greatly in their sources and formation mechanisms, and their responses to CEPI also exhibit heterogeneous governance effects. This paper further decomposes the impact of CEPI on individual air pollutants, with the regression results presented in Columns (3)–(8) of Table 3. The results in Columns (3)–(7) indicate that CEPI exerts a significant inhibitory effect on PM_{2.5}, PM₁₀, SO₂, NO₂ and CO concentrations. In particular, it substantially reduces the concentrations of PM_{2.5}, PM₁₀, and SO₂, while the mitigation effects on NO₂ and CO are relatively weaker. Compared with NO₂ and CO, PM_{2.5} and PM₁₀ directly affect atmospheric visibility, and SO₂ has obvious irritant properties. The public is highly sensitive to and concerned about these three pollutants, and such public attention further drives governmental environmental governance actions. For example, the Ministry of Ecology and Environment required a 10% reduction in national total sulfur dioxide emissions by 2010 relative to the 2005 level, aiming to lower urban atmospheric SO₂ concentrations. (See the Notice on Printing and Distributing the “11th Five-Year Plan for National Acid Rain and Sulfur Dioxide Pollution Prevention and Control” (Huan Fa (2008) No. 1).) In 2014, the State Council explicitly incorporated the declining proportions of PM_{2.5} and PM₁₀ concentrations into official performance assessment schemes. (See the Notice of the General Office of the State Council on Printing and Distributing the Measures for the Assessment of the Implementation of the Air Pollution Prevention and Control Action Plan (Trial) (Guo Ban Fa (2014) No. 21).) Moreover, as primary pollutants, PM_{2.5} and PM₁₀ entail relatively low marginal abatement costs, making them more readily addressed by targeted regulatory interventions.

Notably, the estimated coefficient for O₃ in column (8) is 1.784, statistically significant at the 1% level, contrasting sharply with the negative effects observed for primary pollutants such as PM_{2.5}, PM₁₀, SO₂, NO₂, and CO. This result is not unexpected but rather closely related to the distinct formation mechanism of O₃ compared with conventional pollutants. O₃ is not emitted directly from production or consumption activities but rather formed secondarily through photochemical reactions of VOCs and NO_x under sunlight [40,41]. This implies that the stringent control of primary pollutant emissions during the inspection period may have altered the precursor balance for O₃ formation—the reduction in NO_x concentrations may have weakened the “titration effect” on O₃, leading to elevated O₃ concentrations [42]. In addition, the decline in PM_{2.5} concentrations during the inspection period reduced aerosol scattering and absorption of solar radiation, increasing the intensity of ultraviolet radiation reaching the ground and further accelerating the photochemical formation of O₃ [30]. These mechanisms jointly explain why O₃ concentrations increased rather than decreased during the inspection period. This finding reveals the limitation of the current inspection model, which predominantly focuses on primary pollutant control, in addressing O₃ pollution—relying solely on emission source regulation is insufficient to effectively mitigate secondary pollutant formation.

The estimation results for the control variables are consistent with theoretical expectations and the existing literature. In terms of meteorological variables, increased precipitation significantly reduces AQI, PM_{2.5}, PM₁₀, and O₃ concentrations through wet deposition, reflecting the scavenging effect of precipitation on pollutants. Daily average temperature is positively correlated with most pollutants, consistent with the seasonal pattern in China where high summer temperatures favor O₃ photochemical formation and low winter temperatures exacerbate emissions from coal combustion for heating. The negative coefficient for minimum temperature captures the nonlinear effect of diurnal temperature range on atmospheric diffusion conditions. The negative effect of wind speed indicates that enhanced horizontal dispersion facilitates pollutant dilution and transport [36]. The estimated coefficients for statutory holidays vary in direction across different pollutants, reflecting the differentiated effects of reduced industrial emissions and increased residential travel during holiday periods, which is consistent with the findings of Chen et al. (2025) [38]. The significance and directionality of the above control variables are consistent with theoretical expectations, supporting the validity of our model specification.

Table 3. Baseline Regression Results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	AQI	AQI	PM _{2.5}	PM ₁₀	SO ₂	NO ₂	CO	O ₃
<i>T</i> (h = 30)	−0.607 ** (0.271)	−0.877 *** (0.269)	−1.655 *** (0.194)	−1.152 ** (0.470)	−1.071 *** (0.074)	−0.575 *** (0.081)	−0.030 *** (0.003)	1.784 *** (0.180)
Precip		−0.136 *** (0.017)	−0.070 *** (0.012)	−0.124 *** (0.030)	−0.003 (0.005)	−0.011 ** (0.005)	0.000 (0.000)	−0.158 *** (0.011)
Temp		1.777 *** (0.143)	1.040 *** (0.103)	1.267 *** (0.250)	0.680 *** (0.039)	0.700 *** (0.043)	0.022 *** (0.001)	1.279 *** (0.095)
MinTemp		−1.148 *** (0.082)	−0.618 *** (0.059)	−1.312 *** (0.142)	−0.571 *** (0.022)	−0.652 *** (0.024)	−0.010 *** (0.001)	−1.221 *** (0.054)
MaxTemp		−0.259 *** (0.084)	−0.219 *** (0.061)	0.166 (0.147)	−0.184 *** (0.023)	0.005 (0.025)	−0.010 *** (0.001)	0.520 *** (0.056)
Wind		−0.966 *** (0.048)	−0.973 *** (0.034)	−1.192 *** (0.083)	−0.234 *** (0.013)	−0.585 *** (0.014)	−0.012 *** (0.000)	0.355 *** (0.032)
StatHoliday		0.258 (0.257)	0.321 * (0.186)	−0.474 (0.450)	0.281 *** (0.071)	−0.248 *** (0.077)	0.001 (0.003)	1.864 *** (0.172)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
N	82,507	82,507	82,507	82,507	82,507	82,507	82,507	82,507
R ²	0.373	0.383	0.400	0.325	0.418	0.558	0.417	0.483

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Two-way clustering follows Cameron et al. (2011) [43] and accounts for serial correlation within cities and cross-sectional correlation across cities on the same date, given that treatment assignment occurs at the provincial level.

To confirm the unbiasedness and validity of the estimation results, this paper conducts robustness checks from ten aspects: (1) winsorizing all continuous variables at the 1st and 99th percentiles to eliminate potential drivers of extreme values; (2) adjusting the optimal bandwidth (20 days, 45 days, and 60 days) to test whether the conclusions depend on a specific bandwidth choice; (3) excluding observations near the cutoff (donut regression) to rule out the possibility of data manipulation; (4) employing alternative polynomial orders to test sensitivity to model specification; (5) testing the discreteness of the running variable to confirm the absence of manipulation at the cutoff; (6) replacing the kernel function (triangular vs. rectangular kernels) to examine the sensitivity of estimates to

weighting functions; (7) using provincial capitals as the benchmark and advancing/lagging the inspection start and end dates by 3 and 7 days to test whether measurement errors arising from using provincial dates as proxies for city-level dates affect the conclusions; (8) excluding provinces with provincial-level party or government leadership adjustments during the inspection period to reduce political schedule interference; (9) employing four alternative clustering schemes—city-level, province-level, province-round, and province-year clustering—to test the sensitivity of standard errors to the choice of clustering level; and (10) replacing the time fixed effects with city-by-week, city-by-quarter, and city-by-semester fixed effects, respectively, to examine whether the core findings are sensitive to the granularity of temporal controls. The results of these tests (see Appendix B for details) consistently support our baseline conclusions.

4.3. Diminishing Marginal Effects of the Policy Across Inspection Rounds

The CEPI has progressed through multiple phases, including the pilot program, the first round, the first-round “look-back,” and the second round. Each round differs in inspection targets, focal points, and implementation contexts, and the policy effects may vary accordingly. To examine these differences, we extend the baseline model by incorporating interaction terms between round dummies and the inspection policy variable and re-estimate the model. (To maximally distinguish and identify the policy effects of different rounds of environmental inspections, this paper sets the observation period for each round as five months before and after the actual inspection implementation. The observation period for the first round of CEPI is defined from January 2015 to December 2017; that for the reinspection of the first round ranges from January 2018 to February 2019; and the observation period for the second round of CEPI spans from March 2019 to September 2022.) The results are presented in Table 4. Panel A shows that, using the first-round “look-back” and the second round as the reference category, the total marginal effect of the first round on AQI is 6.014, significantly larger than that of subsequent rounds (−5.669), with a difference of 11.683 between the two, significant at the 1% level. Panel B further shows that, using the second round as the reference category, the total marginal effect of the “look-back” on AQI is 7.935, significantly larger than that of the second round (−4.164), with a difference of 12.099 between the two, significant at the 1% level. Taken together, although the two panels are estimated on different samples and the point estimates are thus not directly comparable across panels, both panels consistently indicate that earlier rounds generate significantly larger effects than their respective later-round counterparts. This consistent within-panel pattern supports the conclusion of diminishing marginal policy effects as the inspection rounds proceed. This pattern is largely consistent across PM_{2.5}, PM₁₀, SO₂, NO₂, and CO. The coefficient direction for O₃ is opposite to that of other pollutants, which is consistent with the baseline results.

Several factors may contribute to the observed differences. First, diminishing marginal effects of policy implementation. As the inaugural institutional practice of centralizing supervisory authority, the first round of inspections concentrated the highest level of “political momentum” and attracted the greatest public attention. Local governments faced the highest uncertainty and accountability pressure, thereby generating the strongest short-term governance responses. As the inspection system has become routinized, local governments have gradually formed expectations and coping strategies, and the marginal governance responses triggered by the same level of inspection pressure have tended to diminish. Second, the phased adjustment of inspection priorities. According to publicly available reports, the first round of inspections and its “look-back” focused on resolving prominent ecological and environmental issues affecting the public, including a large number of petitions related to air pollution. (See Xinhua News Agency, “By the Numbers: First Round of Central Ecological and Environmental Protection Inspection and Follow-Up

Reinspection: Over 150,000 Environmental Issues, Fines of 2.46 Billion Yuan, 2264 People Detained", https://www.gov.cn/hudong/2019-05/15/content_5391977.htm, published on 15 May 2019, accessed on 29 February 2024.) The second round, while continuing to address pollution prevention and control, has directed more attention toward the implementation of major regional strategies, the strict control of blind expansion of “two high” projects (high energy consumption and high emissions), and broader ecological damage issues such as illegal and irregular mining. (See Xinhua News Agency, “Over 3000 People Held Accountable! What Issues Are Targeted in the Second Round of Central Ecological and Environmental Protection Inspection?”, http://www.news.cn/politics/2023-06/25/c_1129715732.htm, published on 25 June 2023, accessed on 29 February 2024.) This diversification of inspection priorities may have, to some extent, diluted the concentrated investment in air pollution control, thereby affecting the marginal effects of air quality improvement. Third, superimposed disturbances from the external environment. The second round of inspections (2020–2022) coincided with the global COVID-19 pandemic, during which production activities, travel patterns, and energy consumption structures were all impacted to varying degrees. The superimposed effects of these disruptions may have exerted complex influences on the estimation results, such that the comparison across rounds should not be directly interpreted as a simple “decay of deterrence effects.” In summary, this paper finds systematic differences in the short-run marginal responses across inspection rounds. The first round exhibits the largest response magnitude, followed by the “look-back,” with the second round showing relatively smaller effects. This pattern is likely attributable to the combination of diminishing marginal policy effects, shifts in inspection priorities, and external environmental disturbances.

Table 4. Policy Effects of the CEPI across Different Rounds.

Variables	(1) AQI	(2) PM _{2.5}	(3) PM ₁₀	(4) SO ₂	(5) NO ₂	(6) CO	(7) O ₃
Panel A: First Round vs. Subsequent Rounds (Reference: Subsequent Rounds)							
$T \times \text{round1}$	11.683 *** (0.474)	8.298 *** (0.342)	11.864 *** (0.829)	6.458 *** (0.129)	5.561 *** (0.141)	0.232 *** (0.005)	−0.081 (0.317)
Subsequent Rounds	−5.669 *** (0.331)	−5.059 *** (0.239)	−6.019 *** (0.580)	−3.720 *** (0.090)	−2.857 *** (0.099)	−0.125 *** (0.003)	1.818 *** (0.222)
First Round	6.014 *** (0.333)	3.239 *** (0.240)	5.845 *** (0.583)	2.738 *** (0.091)	2.704 *** (0.099)	0.107 *** (0.003)	1.737 *** (0.223)
95% CI	[5.361, 6.667]	[2.769, 3.709]	[4.702, 6.988]	[2.560, 2.916]	[2.510, 2.898]	[0.101, 0.113]	[1.300, 2.174]
N	82,507	82,507	82,507	82,507	82,507	82,507	82,507
R ²	0.388	0.404	0.326	0.435	0.567	0.433	0.483
Panel B: First Round “Look-Back” vs. Second Round (Reference: Second Round)							
$T \times \text{round2}$	12.099 *** (0.819)	5.552 *** (0.548)	11.206 *** (1.537)	2.664 *** (0.162)	3.267 *** (0.231)	0.118 *** (0.006)	−5.016 *** (0.540)
Second Round	−4.164 *** (0.503)	−3.007 *** (0.337)	−2.227 *** (0.944)	−1.607 *** (0.100)	−1.501 *** (0.142)	−0.060 *** (0.004)	3.715 *** (0.332)
“Look-Back”	7.935 *** (0.634)	2.545 *** (0.423)	8.979 *** (1.193)	1.057 *** (0.126)	1.766 *** (0.178)	0.058 *** (0.005)	−1.301 *** (0.419)
95% CI	[6.692, 9.178]	[1.716, 3.374]	[6.641, 11.317]	[0.810, 1.304]	[1.417, 2.115]	[0.048, 0.068]	[−2.122, −0.480]
N	52,335	52,335	52,335	52,335	52,335	52,335	52,335
R ²	0.385	0.396	0.330	0.446	0.561	0.465	0.522

Note: Robust standard errors, clustered at the city-time level (two-way clustering), are reported in parentheses; t-statistics are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Panel A is estimated using the full sample, with the first-round “look-back” and the second round as the reference category. The coefficient on T_{it} (the subsequent-round effect) captures the inspection effect for the

reference category; the interaction term captures the differential effect between the first round and the reference category; and the total first-round effect is the sum of the two. Panel B restricts the sample to the first-round “look-back” and the second round, with the second round as the reference category. The coefficient on T_{it} (the second-round effect) captures the inspection effect of the second round; the interaction term captures the differential effect between the “look-back” and the second round; and the total “look-back” effect is the sum of the two.

4.4. Spatial Differentiation in the Policy Effects of the CEPI

To further capture the spatial heterogeneity of the policy effects of the CEPI, and given that the coefficients obtained from separate subgroup regressions are not directly comparable across groups due to sample differences, this paper conducts heterogeneity analysis by constructing interaction terms. The estimation model is specified as follows:

$$y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 d_{it} \times T_{it} + \beta_3 d_{it} + \beta_4 T_{it} \times hete_i + \beta_5 hete_i + \gamma x_{it} + \delta_i + \eta_t + \varepsilon_{it} \quad (2)$$

In Equation (2), $hete_i$ denotes a set of grouping variables. The groupings are defined as follows: ① Based on the Qinling Mountains–Huaihe River geographic divide. (Southern China includes Shanghai, Jiangsu Province, Zhejiang Province, Anhui Province, Fujian Province, Jiangxi Province, Hubei Province, Hunan Province, Guangdong Province, Guangxi Zhuang Autonomous Region, Hainan Province, Chongqing Municipality, Sichuan Province, Guizhou Province, Yunnan Province, and Xizang Autonomous Region. Northern China covers Beijing, Tianjin, Hebei Province, Shanxi Province, Inner Mongolia Autonomous Region, Liaoning Province, Jilin Province, Heilongjiang Province, Shandong Province, Henan Province, Shaanxi Province, Gansu Province, Qinghai Province, Ningxia Hui Autonomous Region, and Xinjiang Uygur Autonomous Region.) $south = 1$ indicates city i is located in southern China, and 0 otherwise. ② Based on urban resource characteristics. (Classification is based on the Notice of the State Council on Issuing the National Sustainable Development Plan for Resource-Based Cities (2013–2020) (Guo Fa (2013) No. 45).) $resource = 1$ indicates city i is a non-resource-based city, and 0 otherwise. ③ Based on historical air quality levels. (Classification is based on the 2015 *China Environmental Status Bulletin* issued by the Ministry of Ecology and Environment of China.) $aqi = 1$ indicates city i had a proportion of days with good air quality exceeding 80% in 2015, and 0 otherwise. ④ Based on administrative hierarchy. (Classification is based on the 2023 China Statistical Yearbook, Statistical Table of Administrative Divisions of the People's Republic of China, and the Notice of the Central Organization Establishment Committee on Issuing Opinions on Several Issues Concerning Sub-Provincial Cities (Zhong Bian Fa (1995) No. 5), among other official documents.) $hierarchy = 1$ indicates city i is a municipality directly under the Central Government, a sub-provincial city, or a provincial capital city, and 0 otherwise. Other variables are the same as those in Equation (1). In this model, β_1 captures the inspection effect for the reference group ($hete_i = 0$) at the cutoff, and β_4 is the interaction term coefficient, which identifies the differential effect between the treatment group and the reference group. The inspection effect for the treatment group ($hete_i = 1$) is given by $(\beta_1 + \beta_4)$. To assess the statistical significance of the treatment group effect, we calculate the standard error and the 95% confidence interval for $(\beta_1 + \beta_4)$ using a linear combination. Table 5 reports the estimation results for each grouping dimension.

Table 5. Spatial Heterogeneity of the Policy Effects of CEPI.

Variables	(1) AQI	(2) PM _{2.5}	(3) PM ₁₀	(4) SO ₂	(5) NO ₂	(6) CO	(7) O ₃
Panel A: North–South Divide							
$T_{it} \times south$	1.246 ** (0.501)	2.381 *** (0.362)	3.414 *** (0.876)	0.407 *** (0.138)	0.832 *** (0.151)	0.013 ** (0.005)	−2.514 *** (0.335)
Northern Cities	−1.549 *** (0.382)	−2.940 *** (0.275)	−2.994 *** (0.667)	−1.290 *** (0.105)	−1.024 *** (0.115)	−0.036 *** (0.004)	3.141 *** (0.255)
Southern Cities	−0.303 (0.630)	−0.559 (0.455)	0.420 (1.101)	−0.883 *** (0.173)	−0.192 (0.190)	−0.023 *** (0.006)	0.627 (0.421)
95% CI	[−1.538, 0.932]	[−1.450, 0.332]	[−1.738, 2.578]	[−1.223, −0.543]	[−0.564, 0.180]	[−0.036, −0.010]	[−0.198, 1.452]
Panel B: Resource Characteristics							
$T_{it} \times resource$	2.036 *** (0.531)	0.875 ** (0.383)	3.706 *** (0.928)	−0.924 *** (0.146)	−1.777 *** (0.159)	−0.011 ** (0.005)	−0.338 (0.355)
Resource-based Cities	−2.231 *** (0.444)	−2.237 *** (0.321)	−3.619 *** (0.776)	−0.456 *** (0.122)	0.608 *** (0.133)	−0.022 *** (0.004)	2.009 *** (0.297)
Non-resource-based Cities	−0.195 (0.531)	−1.362 *** (0.383)	0.087 (0.928)	−1.380 *** (0.146)	−1.169 *** (0.159)	−0.033 *** (0.005)	1.671 (0.355)
95% CI	[−1.236, 0.846]	[−2.113, −0.611]	[−1.731, 1.905]	[−1.667, −1.093]	[−1.481, −0.857]	[−0.043, −0.023]	[−0.006, 3.348]
Panel C: Historical Air Quality							
$T_{it} \times aqi$	3.054 ** (1.474)	2.972 *** (1.065)	4.929 * (2.576)	1.081 *** (0.406)	2.711 *** (0.443)	0.015 (0.015)	−1.666 * (0.985)
Historically Moderate Cities	−0.969 *** (0.273)	−1.745 *** (0.197)	−1.302 *** (0.476)	−1.104 *** (0.075)	−0.658 *** (0.082)	−0.030 *** (0.003)	1.835 *** (0.182)
Historically Good Cities	2.085 (1.449)	1.227 (1.047)	3.627 (2.533)	−0.023 (0.399)	2.053 *** (0.436)	−0.015 (0.015)	0.169 (0.968)
95% CI	[−0.804, 4.974]	[−0.860, 3.314]	[−1.422, 8.676]	[−0.818, 0.772]	[1.184, 2.922]	[−0.045, 0.015]	[−1.762, 2.100]
Panel D: Administrative Hierarchy							
$T_{it} \times hierarchy$	−0.131 (0.803)	−0.137 (0.580)	0.184 (1.402)	0.373 * (0.221)	−0.066 (0.241)	−0.002 (0.008)	0.142 (0.536)
Low-hierarchy Cities	−0.877 *** (0.269)	−1.655 *** (0.194)	−1.152 *** (0.470)	−1.071 *** (0.074)	−0.575 *** (0.081)	−0.030 *** (0.003)	1.784 *** (0.180)
High-hierarchy Cities	−1.008 *** (0.284)	−1.792 *** (0.205)	−0.968 *** (0.496)	−0.698 *** (0.078)	−0.641 *** (0.086)	−0.032 *** (0.003)	1.926 *** (0.190)
95% CI	[−1.565, −0.451]	[−2.194, −1.390]	[−1.940, 0.004]	[−0.851, −0.545]	[−0.810, −0.472]	[−0.038, −0.026]	[1.554, 2.298]

Note: All panels are estimated using the same model specification. N = 82,507 and R² = 0.383 for all panels. Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.4.1. Heterogeneity by Geographic Location

The Qinling Mountains–Huaihe River line serves as the geographic demarcation between northern and southern China. Northern regions are characterized by heavy chemical industries, with higher pollution emission intensity and resource consumption, while southern regions concentrate on advanced manufacturing and modern services, featuring a relatively lighter industrial structure [44]. Moreover, northern cities face greater challenges in ecological and environmental governance [45]. Panel A of Table 5 shows that the interaction term coefficient for the north–south grouping is 1.246 (AQI), significant at the 5% level, indicating a significant difference in inspection effects between southern and

northern cities. The inspection effect for northern cities (the reference group) is -1.549 (AQI), significant at the 1% level. The marginal effect for southern cities is -0.303 , with its 95% confidence interval of $[-1.538, 0.932]$ containing zero, indicating that the inspection effect on AQI in southern cities is not statistically significant. For other pollutant indicators, the marginal effects for southern cities exhibit a similar pattern—point estimates are negative, but the confidence intervals all contain zero ($PM_{2.5}$, PM_{10} , NO_2 , O_3), with only SO_2 and CO remaining significant. This result suggests that the improvement effect of the inspection is more concentrated and detectable in northern cities, rather than southern cities exhibiting stronger effects.

4.4.2. Heterogeneity by Urban Resource Characteristics

Resource-based cities refer to special types of cities developed based on the exploitation and processing of local mineral and forest resources, and they have long faced industrial structure lock-in characterized by high energy consumption and high pollution. (See the Notice of the National Development and Reform Commission, Ministry of Finance, and Ministry of Natural Resources on Printing and Distributing the “14th Five-Year Plan” Implementation Plan for Promoting High-Quality Development of Resource-Based Areas (Fa Gai Zhen Xing (2021) No. 1559).) In 2020, SO_2 emissions from resource-based cities accounted for approximately 60% of the national total, and carbon emissions per 10,000 yuan of GDP were 1.6 times those of non-resource-based cities [46,47]. Panel B of Table 5 shows that the interaction term coefficient for the resource characteristics grouping is 2.036 (AQI), significant at the 1% level, indicating a significant difference in inspection effects between non-resource-based and resource-based cities. The inspection effect for resource-based cities (the reference group) is -2.231 (AQI), significant at the 1% level. The marginal effect for non-resource-based cities is -0.195 , with its 95% confidence interval of $[-1.236, 0.846]$ containing zero, indicating that the inspection effect on AQI in non-resource-based cities is not statistically significant. This pattern also holds for PM_{10} and O_3 . Non-resource-based cities have a lighter and more diversified industrial structure with lower marginal costs of pollution control, but due to their smaller pollution emission baseline, the inspection offers limited further improvement space, making it difficult to detect significant effects statistically. Although resource-based cities face greater governance difficulties, the inspection's reduction effect on their high emission baseline is more pronounced, making it more readily detectable statistically.

4.4.3. Heterogeneity by Historical Air Quality Levels

Historical air quality levels vary significantly across cities and may influence their response patterns to inspection pressure. This paper groups cities based on whether the proportion of days with good or excellent air quality exceeded 80% in 2015. Panel C of Table 5 shows that the interaction term coefficient for the historical air quality grouping is 3.054 (AQI), significant at the 5% level. The inspection effect for cities with historically moderate air quality (the reference group) is -0.969 (AQI), significant at the 1% level. The marginal effect for cities with historically good air quality is 2.085 , with its 95% confidence interval of $[-0.804, 4.974]$ containing zero, indicating that the inspection effect on AQI in historically good cities is not statistically significant, and the point estimate is positive in direction. This result is consistent with the law of diminishing marginal returns—when pollution emissions are already at a low level, the inspection offers limited further improvement space, and thus a significant negative effect cannot be detected.

4.4.4. Heterogeneity by Administrative Hierarchy

Cities with higher administrative hierarchies are generally closer to central power, face stronger political constraints, have more accessible channels for public expression of

grievances, and possess advantages in the agglomeration of high-quality production factors [48]. When the CEPI is deployed, cities with higher administrative hierarchies, subject to stronger political and public constraints, are more likely to translate inspection pressure into momentum for improving ecological and environmental governance efficiency. According to administrative hierarchy, Chinese cities rank as follows in terms of political constraint: municipalities directly under the central government > sub-provincial cities > provincial capital cities > prefecture-level cities > county-level cities. Among these, sub-provincial cities include some cities with separate planning status, which are equivalent to provincial capital cities in administrative rank but enjoy greater economic autonomy. (Note: After 1997, China's sub-provincial cities include 15 cities such as Guangzhou, Wuhan, Harbin, Shenyang, Chengdu, Nanjing, Xi'an, Changchun, Jinan, Hangzhou, Dalian, Qingdao, Ningbo, Xiamen, and Shenzhen. Among them, Dalian, Qingdao, Ningbo, Xiamen, and Shenzhen are cities specifically designated in the state plan. Considering that sub-provincial cities evolved from cities specifically designated in the state plan and emphasize political level, cities specifically designated in the state plan are not listed separately here.) This paper classifies municipalities directly under the central government, sub-provincial cities, and provincial capital cities as high-administrative-level cities and the rest as low-administrative-level cities. Panel D of Table 5 reveals that the interaction terms for the administrative hierarchy grouping are insignificant for AQI (-0.131) and most other pollutants ($p > 0.1$), with only SO_2 (0.373) being marginally significant at the 10% level ($p \approx 0.09$). For low-administrative-level cities (the reference group), the inspection effect on AQI is -0.877 ($p < 0.01$), while the marginal effect for high-administrative-level cities is -1.008 ($p < 0.01$), with a 95% confidence interval of $[-1.565, -0.451]$, which substantially overlaps with the reference group's effect. This indicates that the moderating role of administrative hierarchy on the inspection effect lacks sufficient statistical support, and the empirical evidence in this paper is insufficient to infer that cities with higher administrative levels achieved systematically greater air quality improvements. The marginal significance of SO_2 suggests that high-administrative-level cities may have certain advantages in industrial point source governance [49], but this pattern is not consistently verified across other pollutants.

5. Discussion: Potential Limitations and Reflections on the Inspection Effect

It must be acknowledged that while the Party-led environmental accountability mechanism in China has driven improvements in ecological and environmental governance, such policy effects are not sustainable. The underlying reason is that economic development remains the primary task in government attention allocation. First, the implementation of environmental policies requires economic support. Ecological and environmental governance itself depends on substantial financial and technological inputs, and less economically developed regions often struggle to maintain high-standard environmental measures over the long term. Second, there is a contradiction in performance incentives under the official tenure system. Within limited terms of office, ecological and environmental governance typically requires a long cycle to yield visible results, whereas economic growth can generate demonstrable performance in the short term. Consequently, when the two conflict, local officials tend to prioritize economic development [50]—a tendency that not only aligns with the practical considerations of local governments but also receives tacit acceptance from higher-level authorities and the local public [51]. Finally, from the perspective of governing philosophy, environmental protection remains at the stage of instrumental rationality. Although policies such as environmental inspections have strengthened the binding force of environmental governance, the prior-

ities of regime stability and economic development still far outweigh environmental protection [50]. As one study points out, “without local economic development, social and political instability will be exposed” [52]. Even against the backdrop of official performance assessment reforms, ecological and environmental governance is still regarded as “icing on the cake” for economic development rather than a core objective.

In addition, it is necessary to understand the potential negative effects of the CEPI. First, the costs of mobilizing administrative resources driven by high-level political momentum may be difficult to estimate [53]. Although precise quantification is lacking, it is undeniable that the CEPI may involve costs including those generated by the inspection work itself (such as evidence collection, inspection, accountability, rectification, and effect evaluation), additional costs incurred by local governments in responding to inspections, costs associated with enterprises choosing transformation or closure in response to inspections, and public reporting and oversight costs. Moreover, existing case studies have revealed significant negative impacts on local economies, including substantial unemployment and economic losses [54]. Similar issues have been exposed in land supervision. The special campaign against land law violations conducted by land supervision authorities for three consecutive years from 2007 to 2009, while rectifying many land law violations, consumed enormous resources through high-intensity campaigns, and land issues rebounded after the campaigns ended, accompanied by a period of inspection fatigue [55]. Second, violent law enforcement by local governments under accountability pressure may trigger social risks and even weaken government credibility. Third, environmental inspections may disrupt routine law enforcement and undermine the rule of law [56]. Finally, while emphasizing the effectiveness of the CEPI in air governance under the principle of shared Party-government responsibility, attention must also be paid to the inherent deficiencies in self-supervision of Party power and the issue of sharing Party and government responsibilities in ecological and environmental governance.

6. Conclusions and Future Directions

6.1. Conclusions

The CEPI represents a distinctive Chinese approach to the centralization of environmental supervision authority. This paper treats the CEPI, which was progressively implemented from 2016, as a quasi-natural experiment. Using daily city-level panel data from 333 Chinese cities over the period from 1 January 2015 to 31 December 2022, we employ a regression discontinuity design in time to evaluate the governance effects of the CEPI on urban air quality. The main conclusions are as follows:

First, the CEPI has a significant short-term effect on air quality improvement, but this effect is mainly limited to primary pollutants. The inspection significantly reduces urban concentrations of AQI and primary pollutants such as $PM_{2.5}$, PM_{10} , SO_2 , NO_2 , and CO, with $PM_{2.5}$, PM_{10} , and SO_2 exhibiting the most pronounced reductions. This finding is broadly consistent with the existing literature documenting the beneficial effects of the CEPI on environmental quality [23,52]. Notably, O_3 concentrations increase significantly during the inspection period, a finding consistent with previous observations [17,23,30]. This mixed evidence suggests that while the centralization of supervision authority is effective in mitigating conventional primary pollutants, its effectiveness in addressing complex secondary pollutants is conditional.

Second, the short-run marginal responses to the inspection exhibit a declining trend across rounds. This paper finds that the Central Environmental Protection Inspection generates significant short-run improvements in air quality during the inspection period. However, the marginal effects vary across rounds: the first round and the “look-back” produce larger short-run coefficients than the second round, indicating that marginal policy responsiveness diminishes as the inspection program becomes institutionalized. This

dynamic pattern is consistent with existing studies [23,30]. Based on sample evidence covering 333 cities and three inspection rounds, this paper further confirms the generality of this declining pattern.

Third, the policy effects exhibit significant spatial heterogeneity. Northern cities, resource-based cities, and cities with historically moderate air quality experience more pronounced improvements, consistent with existing findings that more polluted cities are more responsive to inspections [23,29,30]. However, the grouping effects of administrative hierarchy are not statistically significant for most pollutants, indicating that the governance effects of centralizing supervision authority do not simply follow an administrative hierarchy gradient. This finding differs from some existing studies [49,57]. Potential sources of these differences include the following: first, this paper extends the sample period from the first round to the second round of inspections, more comprehensively capturing the long-term effects of the inspection system; second, this paper employs interaction terms and tests the statistical significance of between-group coefficient differences via linear combination, avoiding potential misjudgments arising from direct comparisons of subgroup regression coefficients due to overlapping confidence intervals.

The above findings offer the following policy implications for improving the inspection system design. First, facilitate the transition of the inspection system from “campaign-style” to “routinized” governance. It is advisable to establish a long-term follow-up mechanism for inspection rectification, incorporating identified issues into the routine performance evaluation system for local officials to avoid cyclical fluctuations of “tight during inspections, lax afterward.” Simultaneously, regular assessments and public disclosure of rectification outcomes should be implemented to create sustained institutional pressure rather than concentrated campaign-style shocks. Second, strengthen the coordinated governance of O_3 and $PM_{2.5}$. In subsequent rounds, inspection teams should place greater emphasis on the coordinated reduction of VOCs and NO_x , encouraging local governments to shift from “single-pollutant control” to “multi-pollutant synergistic control” to avoid the “seesaw effect” in pollution governance. At the same time, attention should be paid to aligning the promotion of industrial-source $PM_{2.5}$ and ultrafine particle abatement technologies with inspection pressure. The technical feasibility of acoustic agglomeration [58] and low-temperature plasma electrostatic filtration technologies [59] in efficiently capturing particulate matter provides technical references for enterprises to implement inspection rectification requirements. Third, implement differentiated inspection strategies. For northern cities, resource-based cities, and other regions with weak governance foundations and large emission reduction potential, inspection intensity should be increased and rectification follow-up periods extended. For southern cities, non-resource-based cities, and other regions with diminishing marginal effects, greater autonomous governance space could be granted, with the inspection focus shifting toward institutional development and capacity assessment.

6.2. Research Limitations and Future Directions

First, the external validity of causal identification is limited. The regression discontinuity design in time estimates the local average treatment effect (LATE) within a 30-day window around the inspection cutoff. Whether these findings can be generalized to longer time horizons or other policy contexts requires additional assumptions. RDD serves as a complementary estimation strategy for causal effects under specific identification assumptions. Future research could employ longer time windows or combine other identification strategies—such as event study designs—to examine the duration and long-term effects of inspection impacts.

Second, measurement errors in city-level inspection timing arise. Since the exact dates of inspection team deployment to each city are not fully disclosed through official

channels, this paper uses the provincial-level inspection start and end dates as proxies for city-level treatment timing, which may introduce measurement error. While we verify the robustness of our core conclusions through timing reassignment sensitivity analyses (advancing/lagging by 3 and 7 days), more precise identification awaits the complete public release of city-level inspection schedules. Moreover, within the same province, different cities may differ in actual inspection team arrival times, petition transfer efficiency, and local government response rhythms. Future research could further validate these findings with more granular inspection schedule data.

Third, mechanism tests are lacking. Based on principal-agent theory and organizational learning theory, this paper logically deduces the policy effects of the CEPI through three pathways—information compression, objective reshaping, and strategic space narrowing—rather than conducting direct mechanism variable tests. Due to data constraints, we are unable to directly incorporate mechanism variables such as public complaint volumes, petition transfer case counts, penalty amounts, inspection reports, or firm closure lists for empirical verification. Therefore, our mechanism discussion should be understood as possible explanations grounded in theoretical frameworks rather than established causal pathways. Although the empirical results are consistent with theoretical expectations, whether these three pathways indeed constitute the actual transmission channels of the inspection effect awaits further testing by future research when data conditions permit.

Despite these limitations, this paper, through the regression discontinuity design in time, provides a feasible path for identifying local short-term causal effects of environmental regulations—when policies are progressively implemented at the national level and valid control groups are difficult to find, focusing on the “threshold” around the policy shock still enables reliable causal estimation. Our contribution primarily lies in the identification and quantitative evaluation of inspection effects rather than a complete revelation of transmission mechanisms. Ultimately, environmental governance research requires more comprehensive theoretical frameworks and rigorous empirical strategies to uncover the multiple causes of policy effects and their heterogeneity across different conditions. However, current research still faces dual challenges at the data and methodological levels. The most promising direction in the short term is to accumulate reliable evidence on individual policy instruments through refined research designs, test their heterogeneity, and gradually reveal transmission mechanisms. Based on such accumulation, policymakers can design differentiated governance strategies that balance environmental improvement and economic development. This paper represents an attempt in this direction—taking the centralization of supervision authority as the theoretical thread, regression discontinuity as the identification tool, and air quality as the outcome variable—to provide empirical reference for understanding institutional effectiveness in China’s environmental governance transition.

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Data Availability Statement: The air quality index (AQI) and other air pollutant concentration data used in this study were obtained from the “Weather Post” website (<http://www.tianqihoubao.com/aqi/>) (accessed on 11 July 2024), with the original data sourced from the Ministry of Ecology and Environment of the People’s Republic of China. The central environmental inspection data were collected from the official website of the Ministry of Ecology and Environment of the People’s Republic of China. The statutory holiday dummy variable (StatHoliday) was constructed based on the annual holiday arrangement notices issued by the General Office of the State Council of China. Meteorological data, including temperature and other weather variables, were derived from the National Centers for Environmental Information (NCEI) of the United States. The datasets generated and/or analyzed during the current study are available from the corresponding author upon reasonable request.

Conflicts of Interest: The authors declare no conflicts of interest.

Appendix A

Table A1. Covariate Balance Test Results for RDD.

Variable	Mean (Left)	Mean (Right)	T (h = 30)	Robust Standard Error	95% CI
Precip	3.401	3.463	0.383	−0.118	[1.415, −0.681]
Temp	15.705	16.520	−0.291	−0.157	[−0.158, 0.757]
MinTemp	10.563	11.585	−0.374	−0.167	[−0.260, 0.718]
MaxTemp	20.975	21.610	−0.291	−0.152	[−0.190, 0.700]
Wind	8.860	8.831	0.121	−0.048	[−0.128, 0.540]
StatHoliday	0.302	0.311	0.002	−0.007	[−0.044, 0.005]

Note: Local linear regression with triangular kernel and bandwidth $h = 30$. Robust standard errors (in parentheses) are two-way clustered at the city and time levels. Significance is based on robust inference. Confidence intervals are based on bias-corrected robust inference.

Appendix B

To confirm the unbiasedness and validity of the estimation results, this paper conducts robustness checks from ten aspects: (1) winsorization of continuous variables; (2) bandwidth sensitivity analysis; (3) donut regression (excluding observations near the cut-off); (4) sensitivity to alternative functional forms (polynomial orders); (5) discreteness test of the running variable; (6) alternative kernel functions (triangular vs. rectangular); (7) timing reassignment sensitivity analysis (advancing/lagging cutoff dates by 3 and 7 days); (8) excluding provinces with leadership transitions during inspection periods; (9) alternative clustering levels for standard errors (city-level, province-level, province-round, and province-year); and (10) alternative time fixed effects.

It should be noted that no other environmental policies were implemented nationwide at the daily frequency across all cities during the sample period. Therefore, we do not additionally exclude the potential effects of concurrent policies in the robustness checks.

Appendix B.1. Winsorization Sensitivity Test

To mitigate the potential influence of extreme values on estimation results, this paper winsorizes all continuous variables at the 1st and 99th percentiles. The descriptive statistics of the winsorized variables are presented in Table A2. Prior to winsorization, the maximum values of PM_{2.5} and PM₁₀ reached 1782 µg/m³ and 3176 µg/m³, respectively, which may stem from extreme weather events such as dust storms or instrument anomalies. After winsorization, the maximum values of PM_{2.5} and PM₁₀ decreased to 150 µg/m³ and 278 µg/m³, respectively, falling within a reasonable range for heavy pollution episodes. The maximum values of SO₂, NO₂, and CO are all below the Grade II daily average concentration limits stipulated in the Ambient Air Quality Standards (GB 3095-2012). The mean values of core variables change by less than 5%, indicating that the sample distribution remains largely consistent with the original data. This suggests that the winsorization procedure effectively eliminates the influence of extreme values while preserving the core characteristics of the sample.

Table A2. Descriptive Statistics of Winsorized Variables.

Type	Var	Unit	Mean	Std. Dev.	Min	Max
Dependent variables	AQI	-	64.24	38.90	4.00	198.00
	PM _{2.5}	µg/m ³	36.67	30.63	2.00	150.00
	PM ₁₀	µg/m ³	67.86	58.99	4.00	278.00
	SO ₂	µg/m ³	13.93	13.57	1.00	76.00
	NO ₂	µg/m ³	25.53	15.04	2.00	75.00
	CO	mg/m ³	0.83	0.41	0.08	2.40
	O ₃	µg/m ³	60.63	28.54	0.48	159.00
Independent variables	T	-	0.03	0.18	0	1
Control variables	Precip	mm	2.78	5.50	0	24.60
	Temp	°C	14.31	10.78	-23.00	33.00
	MinTemp	°C	9.14	11.43	-28.99	27.36
	MaxTemp	°C	19.64	10.68	-11.24	37.91
	Wind	m/s	8.75	3.01	2.85	18.00
	StatHoliday	-	0.32	0.46	0	1

We re-estimate the baseline model using the winsorized sample, with results reported in Table A3. Compared with the original baseline estimates, the changes in coefficients for each pollutant are in the expected direction: the absolute magnitudes of the coefficients for AQI, PM_{2.5}, PM₁₀, NO₂, and CO all decrease, while SO₂ shows a slight increase and the positive effect on O₃ attenuates somewhat. The relatively larger change for PM_{2.5} (37%) is mainly attributable to the fact that its original maximum value reached 1782 µg/m³, which was substantially compressed by the winsorization procedure. Nevertheless, the direction, statistical significance, and magnitude of the coefficients for all variables remain consistent with the baseline estimates, confirming that our main findings are not substantively driven by extreme values.

Table A3. Baseline Regression Results with Winsorized Sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	AQI	PM _{2.5}	PM ₁₀	SO ₂	NO ₂	CO	O ₃
T (h = 30)	-0.847 *** (0.273)	-1.037 *** (0.187)	-1.081 ** (0.465)	-1.171 *** (0.069)	-0.564 *** (0.076)	-0.028 *** (0.002)	1.653 *** (0.175)
Controls	Y	Y	Y	Y	Y	Y	Y

City FE	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y
N	82,507	82,507	82,507	82,507	82,507	82,507	82,507
R ²	0.383	0.400	0.325	0.418	0.558	0.417	0.483

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The winsorization procedure retains the full sample size; it replaces extreme values with the values at the corresponding percentiles rather than dropping any observations.

Appendix B.2. Bandwidth Sensitivity Test

RDD estimation aims to capture the local average treatment effect, and its accuracy is largely affected by bandwidth selection, namely the distance from samples to the cutoff point. The choice of bandwidth represents a trade-off between estimation unbiasedness and efficiency [33]. Adopting a smaller bandwidth helps reduce smoothing bias but involves fewer observations for estimation, which may increase the variance of estimated coefficients and lower the efficiency of RDD estimation. A larger bandwidth includes more observations within the bandwidth range, which helps reduce variance but tends to cause greater smoothing bias.

In addition to the baseline bandwidth of 30 days, this paper further estimates changes in air quality within five alternative time windows of 15 days, 45 days, 60 days, 90 days, and 120 days before and after the inspection, corresponding to 0.5, 1.5, 2, 3, and 4 times the baseline bandwidth. The results are presented in Table A4. When the bandwidth is set to 15 days, 45 days and 60 days, CEPI exerts a significantly negative effect on AQI, which supports the baseline regression results and verifies the rationality of the manually set baseline bandwidth. Nevertheless, the treatment effect gradually becomes unstable over time. When the bandwidth extends to 90 days, the impact of CEPI on AQI is no longer statistically significant. When the bandwidth reaches 120 days, air quality even shows a rebound. Notably, air quality is generally affected by multiple confounding factors. By this time, the CEPI has ended for four months, and the correlation between CEPI and air quality gradually weakens. Accordingly, this paper concludes that the air quality improvement effect of the CEPI is significant in the short run, while its long-term correlation tends to decay.

Table A4. Sensitivity Tests for Five Alternative Bandwidths.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	AQI	PM _{2.5}	PM ₁₀	SO ₂	NO ₂	CO	O ₃
Panel A: 15-day bandwidth							
T (h = 15)	−0.934 *** (0.309)	−1.281 *** (0.225)	−1.133 ** (0.531)	−0.637 *** (0.083)	−0.514 *** (0.093)	−0.029 *** (0.003)	2.191 *** (0.203)
N	55,864	55,864	55,864	55,864	55,864	55,864	55,864
R ²	0.389	0.409	0.339	0.452	0.575	0.437	0.511
Panel B: 45-day bandwidth							
T (h = 45)	−0.431 * (0.249)	−1.504 *** (0.184)	−0.563 (0.420)	−0.875 *** (0.071)	−0.484 *** (0.076)	−0.021 *** (0.003)	1.389 *** (0.169)
N	110,310	110,310	110,310	110,310	110,310	110,310	110,310
R ²	0.373	0.390	0.318	0.381	0.543	0.400	0.461
Panel C: 60-day bandwidth							
T (h = 60)	−0.838 *** (0.238)	−1.442 *** (0.177)	−1.170 *** (0.391)	−0.822 *** (0.071)	−0.601 *** (0.074)	−0.017 *** (0.002)	1.062 *** (0.163)
N	137,511	137,511	137,511	137,511	137,511	137,511	137,511
R ²	0.360	0.378	0.312	0.353	0.529	0.383	0.447

Panel D: 90-day bandwidth							
<i>T</i> (<i>h</i> = 90)	0.126 (0.226)	−0.675 *** (0.172)	−0.058 (0.371)	−0.694 *** (0.073)	−0.425 *** (0.072)	−0.008 *** (0.002)	0.562 *** (0.155)
N	192,176	192,176	192,176	192,176	192,176	192,176	192,176
R ²	0.350	0.364	0.303	0.315	0.516	0.356	0.421
Panel E: 120-day bandwidth							
<i>T</i> (<i>h</i> = 120)	0.522 ** (0.221)	−0.696 *** (0.178)	0.379 (0.355)	−1.005 *** (0.081)	−0.457 *** (0.070)	−0.012 *** (0.002)	1.042 *** (0.148)
N	243,325	243,325	243,325	243,325	243,325	243,325	243,325
R ²	0.346	0.340	0.305	0.300	0.524	0.356	0.415

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.3. Donut Hole Test

Agents may respond in advance to upcoming policy enforcement, giving rise to anticipatory evasion or adaptive behavior. Under the pressure of administrative accountability, local governments have incentives to manipulate environmental data [60]. If local governments are capable of manipulating pollutant readings, observations immediately before and after the inspection are the most likely to be tampered with. Some studies have cast doubt on the credibility of officially released air quality data in China. Ghanem and Zhang (2014) found an abnormal distribution of air quality readings around the officially defined “blue sky” threshold during 2001–2010 [60]. (Environmental authorities generally classify an AQI below 100 as indicating a “blue sky,” giving rise to topics such as “Olympic Blue,” “Parade Blue,” “APEC Blue,” and “Beijing Blue.”) Nevertheless, other evidence suggests that the quality of China’s air pollution data has improved substantially since 2013 [61,62].

To rule out the potential interference of data manipulation on our estimates, this paper adopts the donut hole RDD approach. (The donut hole test is designed to detect potential manipulation of the running variable by re-estimating the model after excluding observations near the cutoff. In this study, one of the objectives of the inspections was to reduce AQI levels. Therefore, we focus on the potential falsification of AQI data. Specifically, we re-estimate the model after removing observations in the AQI range susceptible to manipulation. If the original conclusions remain robust, it suggests that data manipulation is not severe.) We exclude observations with AQI values lying in manipulation-prone intervals, namely the intervals of 98–102, 95–105, and 92–108, and re-estimate the baseline specification for each trimmed sample. The results in Table A5 show that, after dropping observations in potential data-manipulation intervals, CEPI still significantly reduces urban AQI, which is consistent with the baseline findings.

Table A5. Donut Hole Test.

	(1)	(2)	(3)
Variables	98~102	95~105	92~108
<i>T</i>	−0.771 *** (0.272)	−0.629 ** (0.276)	−0.556 ** (0.281)
Controls	Y	Y	Y
City FE	Y	Y	Y
Month FE	Y	Y	Y
N	81,120	79,384	77,639
R ²	0.382	0.381	0.380

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.4. Sensitivity Test of Functional Form Specification

Following Lee and Lemieux (2010) and Hausman and Rapson (2018) [32,33], this paper conducts a global polynomial estimation test by changing the order of the fitted polynomial. Notably, applying high-order polynomials in regression discontinuity design may lead to several problems, including noisy estimates, high sensitivity of results to polynomial order, and poor coverage of confidence intervals [63]. Accordingly, this paper examines the estimation results under the 30-day bandwidth using second-order and third-order polynomial specifications. As reported in Table A6, after controlling for quadratic and cubic trends of the running variable, CEPI still significantly reduces air pollutant concentrations, verifying the robustness of the baseline conclusions.

Table A6. Estimation with Different Polynomial Orders.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	AQI	PM _{2.5}	PM ₁₀	SO ₂	NO ₂	CO	O ₃
Panel A: Second-order							
T	−0.881 *** (0.311)	−1.259 *** (0.224)	−1.820 *** (0.542)	−0.807 *** (0.086)	−0.528 *** (0.093)	−0.032 *** (0.003)	2.714 *** (0.207)
N	82,507	82,507	82,507	82,507	82,507	82,507	82,507
R ²	0.383	0.400	0.325	0.418	0.559	0.417	0.484
Panel B: Third-order							
T	−0.829 *** (0.310)	−1.230 *** (0.224)	−1.734 *** (0.542)	−0.796 *** (0.086)	−0.521 *** (0.093)	−0.031 *** (0.003)	2.725 *** (0.207)
N	82,507	82,507	82,507	82,507	82,507	82,507	82,507
R ²	0.385	0.401	0.327	0.419	0.559	0.417	0.484

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.5. Discreteness Test of the Running Variable

The running variable in time regression discontinuity design exhibits strong discreteness, which may cause certain problems for the estimation of the local polynomial method based on the continuity assumption. Specifically, assuming that samples receive treatment at the cutoff point, the treatment effect of regression discontinuity design is expressed as $\tau_{RD} = E[Y_i(1) | X_i = 0] - E[Y_i(0) | X_i = 0]$, which refers to the difference in the expected value of counterfactual outcomes between samples with treatment and without treatment at the cutoff point. When the running variable is a continuous variable, to obtain the counterfactual expected value of samples without receiving treatment at the cutoff point, the treatment effect formula is set as $\lim_{X_i \downarrow 0} E[Y_i(0) | X_i] = E[Y_i(0) | X_i = 0]$. That is, when the running variable is located on the left side of the cutoff and infinitely close to the cutoff, the expected value of its outcome variable is approximately equal to the counterfactual scenario where the sample does not receive treatment at the cutoff point. However, when the running variable is a discrete variable, it is necessary to assume that $E[Y_i(0) | X_i = 0] = E[Y_i(0) | X_i = -1]$. Since the observations on the left side of the cutoff may not be close enough to the cutoff, or even far away from it, estimation bias may be generated.

In the context of this study, the running variable d_{it} is measured in days and contains 61 discrete values within the 30-day bandwidth (ranging from −30 to 30). After alignment in event time, a large number of cross-city observations are pooled at each discrete value, making the distribution of the running variable approximate to that of a continuous variable. Moreover, this paper controls not only city individual fixed effects but also

monthly time fixed effects. The AQI values of the same city shortly before the cutoff are highly close to the counterfactual AQI level that would prevail at the cutoff in the absence of policy treatment. Accordingly, the estimation bias caused by the discreteness of the running variable should be limited. To ensure robustness, this paper follows the approach proposed by Cattaneo et al. (2023) [64] and adopts the local randomization regression discontinuity design for supplementary testing. When the running variable is discrete, the local randomization method no longer imposes the strict continuity assumption. Its core rationale is that within a sufficiently narrow bandwidth around the cutoff, the running variable is essentially independent of potential outcomes, thereby converting the regression discontinuity framework into a quasi-randomized experiment near the threshold [65]. On this basis, this paper sets the bandwidth at 15 days and conducts the local randomization regression discontinuity estimation. As shown in Table A7, the estimated treatment effect on AQI is statistically significant and negative at -1.931 , which verifies the robustness of the baseline regression results.

Table A7. Local Randomization Regression Discontinuity Estimates of AQI.

	Left of the Cutoff	Right of the Cutoff
Mean of the outcome variable	64.492	62.562
Effective sample size	13,784	42,080
Bandwidth window	−15	15
Treatment Effect Estimates	−1.931	
<i>p</i> -value from large sample test	0.000	

Appendix B.6. Robustness Test with Alternative Kernel Functions

Given that parametric regression discontinuity design relies heavily on the specific functional form, model misspecification may arise. In the baseline regression, this paper follows the existing literature [66] and adopts the rectangular kernel. To test the sensitivity of the estimated results to the choice of kernel function, the Epanechnikov kernel and triangular kernel are further employed for re-estimation. The corresponding results are reported in Table A8. All three estimated coefficients for AQI are significantly negative, confirming the genuine existence of the policy effect of environmental inspection.

Table A8. Regression Discontinuity Estimates under Different Kernel Functions.

	(1)	(2)
	AQI	AQI
Variables	Epanechnikov Kernel	Triangular Kernel
T	−2.772 *** (0.504)	−2.115 *** (0.514)
Fixed effects	Y	Y
N	895,608	895,608
h	45	45

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.7. Timing Reassignment Sensitivity Analysis

Since the exact dates on which the central environmental protection inspection teams arrived in each city are not fully disclosed through official channels, this paper treats each city's inspection start and end dates as equivalent to those of its respective province. This approximation may introduce two types of measurement error. First, within the same province, different cities may differ in the timing of inspection team arrivals, the depth of

on-site inspections, and the efficiency of petition transfer and handling—some cities may receive inspections earlier or later than the provincial-level inspection period. Second, the transmission of inspection pressure within a province is not simultaneous: inspection teams typically arrive in provincial capitals earlier than in prefecture-level cities, and there are time lags in the processing of petitions and the communication of rectification requirements. The expected direction of such measurement error is attenuation bias, whereby non-classical measurement error tends to bias the estimated policy effects toward zero [32], implying that our estimated CEPI effects are likely conservative relative to the true effects.

To test the robustness of our results to this approximation, we re-estimate the model using alternative cutoff dates, advancing and lagging the provincial inspection start and end dates by 3 and 7 days, respectively, with provincial capitals as the benchmark. Table A9 reports the regression discontinuity estimates under these alternative timing specifications. The results show that across all four specifications, the coefficients on T are negative and statistically significant at the 1% level, consistent with the baseline estimate (−0.877 ***). The coefficients exhibit a systematic pattern: the absolute magnitudes are smaller under the advanced specifications (which introduce more measurement error): −0.852 for the 3-day advance and −0.823 for the 7-day advance. They are larger under the lagged specifications (which better approximate the true window): −0.901 for the 3-day lag and −0.934 for the 7-day lag. This pattern conforms to the theoretical expectation of attenuation bias. The maximum relative deviation of the coefficients across the four specifications from the baseline is less than 6.5%, and the R^2 values remain stable between 0.381 and 0.385. Collectively, these results indicate that the approximation in policy timing does not pose a substantial threat to our estimates, and our baseline conclusions are robust.

Table A9. Regression Discontinuity Estimates under Alternative Timing Specifications.

	(1)	(2)	(3)	(4)
Variables	AQI	AQI	AQI	AQI
	3-Day Advance	3-Day Lag	7-Day Advance	7-Day Lag
T	−0.852 *** (0.272)	−0.901 *** (0.268)	−0.823 *** (0.276)	−0.934 *** (0.265)
Controls	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
R^2	0.382	0.384	0.381	0.385
N	82,507	82,507	82,507	82,507

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.8. Excluding Interference from Political Schedules

Changes in provincial-level Party or government leadership may interfere with local governance responses during inspection periods by altering local governments' attention allocation and decision-making rhythms. If a province experiences a replacement of the Party Secretary or Governor shortly before or after an inspection, the incoming leader's

adjustments in policy priorities or the outgoing leader's cautious decision-making tendencies may affect the efficiency of inspection rectification, thereby superimposing personnel turnover effects onto the policy shock of inspection timing and introducing additional measurement noise. Excluding these samples helps test whether our core estimates are driven by political schedule factors.

In practice, we adopt two exclusion strategies. First, targeted exclusion (routine personnel adjustments). Some personnel changes are themselves institutional outcomes of inspection accountability (e.g., the personnel accountability following the Qilian Mountain notification in Gansu, and the voluntary surrender of a vice governor following the special inspection of the Muli mining area in Qinghai). Excluding such samples may remove genuine transmission channels of the inspection effect, leading to downward-biased estimates. Therefore, the targeted strategy excludes only provinces with routine personnel adjustments that have no direct causal relationship with inspection accountability—those primarily driven by term expiration or work transfers. These include Shanxi (June 2021, Lou Yangsheng transferred to Henan), Yunnan (October 2021, Ruan Chengfa retired due to age), and Hainan (December 2020, Liu Cigui's term adjustment). Although these personnel changes occurred during sensitive windows around the inspection feedback periods, the changes themselves were not direct consequences of inspection accountability. Second, strict exclusion (all adjustments). This strategy excludes all provinces with provincial-level Party or government leadership changes shortly before or after inspections, including Gansu (April 2017, Party Secretary adjustment), Shanxi (June 2021, Party Secretary adjustment), Yunnan (October 2021, Party Secretary adjustment), Qinghai (September 2020, Vice Governor voluntary surrender), and Hainan (December 2020, Party Secretary adjustment). This strategy minimizes political schedule interference and yields the most conservative results.

Table A10 reports the estimates under both exclusion strategies. In column (1), after the targeted exclusion of three provinces, the coefficient is -0.881 , differing from the baseline (-0.877) by only 0.4%, indicating that routine personnel adjustments do not substantially affect our estimates. In column (2), after the strict exclusion of five provinces, the coefficient is -0.912^{***} , approximately 4.0% larger in absolute magnitude than the baseline. This increase mainly stems from the exclusion of Gansu and Qinghai—two provinces where personnel changes were driven by inspection accountability. As these two provinces embody concentrated manifestations of the inspection effect, their exclusion increases the sample variance of the “inspection effect,” leading to a slight rise in the absolute coefficient magnitude. This also indirectly confirms that Gansu and Qinghai carry important information regarding the inspection effect. The estimates under both strategies remain consistent with the baseline, indicating that our core conclusions are not substantially affected by political schedule interference.

Table A10. Excluding Interference from Provincial-Level Leadership Adjustments.

Variables	(1)	(2)
	AQI	AQI
	Targeted Exclusion (3 Provinces)	Strict Exclusion (5 Provinces)
T	-0.881^{***} (0.258)	-0.912^{***} (0.281)
Controls	Y	Y
City FE	Y	Y
Month FE	Y	Y
N	0.384	0.387
R ²	71,962	64,348

Note: Robust standard errors, clustered at the city and date levels (two-way clustering), are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B.9. Excluding Interference from Political Schedules

Given that the CEPI schedules are arranged at the provincial level, according to Abadie et al. (2017), when treatment is assigned at a higher level of aggregation, standard errors should be clustered at that level; otherwise, standard errors may be underestimated and statistical significance overestimated [67]. To examine the sensitivity of our statistical inference to the choice of clustering level, Table A11 reports the estimation results for AQI under four alternative clustering schemes. Column (1) presents city-level clustering, column (2) province-level clustering, column (3) province-round clustering (directly corresponding to the level at which the treatment variable is assigned), and column (4) province-year clustering. The results show that although standard errors increase as the clustering level becomes more aggregated—consistent with theoretical expectations—the estimated effect of the Central Environmental Protection Inspection on AQI remains significantly negative across all four specifications. This indicates that our findings are not materially affected by the choice of clustering level.

Table A11. Baseline Regression Results under Alternative Clustering Levels.

	(1)	(2)	(3)	(4)
	AQI	AQI	AQI	AQI
Variables	City Cluster- ing	Province Clustering	Province-Round Clustering	Province-Year Clustering
T	−0.847 *** (0.269)	−0.857 *** (0.315)	−0.862 ** (0.342)	−0.859 *** (0.298)
Controls	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
R ²	0.383	0.383	0.383	0.383
N	82,507	82,507	82,507	82,507

Note: Clustered robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B.10. Alternative Time Fixed Effects

In the baseline regression, this paper controls for city-by-month fixed effects to absorb unobservable time-varying shocks specific to each city in each month. However, air quality may be simultaneously influenced by higher-frequency (weekly) and lower-frequency (quarterly, semi-annual) temporal factors, and a single granularity of time fixed effects may not fully capture all types of time-varying confounders. To test whether the core conclusions are sensitive to the choice of temporal granularity in fixed effects, this paper adopts three alternative specifications for re-estimation: first, controlling for city-by-week fixed effects to capture high-frequency fluctuations across different weeks within a month (e.g., intra-week production rhythm differences, weekend travel pattern changes, etc.); second, controlling for city-by-quarter fixed effects to absorb seasonal macroeconomic fluctuations (e.g., increased emissions during winter heating periods, aggravated ozone pollution in summer, etc.); and third, controlling for city-by-semester fixed effects to capture longer-term business cycle or policy cycle characteristics. Among these, the city-by-quarter and city-by-semester fixed effects provide relatively coarse controls for time trends and cannot fully absorb high-frequency shocks at the weekly level, thus constituting a more conservative specification—if the core conclusions remain robust under such specifications, the credibility of our estimation results is further enhanced.

Table A12 reports the estimation results for the three alternative time fixed effects. The results demonstrate that, regardless of whether weekly, quarterly, or semi-annual fixed effects are employed, the coefficient estimates of the core explanatory variable exhibit no substantial changes in either sign or statistical significance, indicating that our conclusions do not depend on the specific granularity choice of time fixed effects.

Table A12. Alternative Time Fixed Effects.

	(1)	(2)	(3)	(4)
	AQI	AQI	AQI	AQI
Variables	Baseline	Weekly FE	Quarterly FE	Semi-Annual FE
T	−0.877 *** (0.269)	−0.842 *** (0.271)	−0.813 *** (0.265)	−0.786 *** (0.268)
Controls	Y	Y	Y	Y
City × Month FE	Y	Y	Y	Y
City × Week FE	N	Y	N	N
City × Quarter FE	N	N	Y	N
City × Semester FE	N	N	N	Y
N	82,507	82,507	82,507	82,507
R ²	0.383	0.385	0.381	0.379

Note: Two-way cluster-robust standard errors (clustered at city and month levels) are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Column (1) replicates the baseline specification in Column 2 of Table 3.

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