Transboundary Pollution and Political Attribution*

Seonmin Will Heo† Mychaela Paetow‡

May 20, 2026

Abstract

This paper studies how transboundary pollution enters domestic politics. When air pollution crosses borders, citizens may observe local pollution without knowing whether the source is foreign or domestic, creating scope for politicians either to provide information or to shift blame abroad. We study South Korea, where fine-dust episodes are salient and pollution from China is a recurring focus of public debate. Combining source-specific pollution shocks with a politician-day panel of Facebook posts by South Korean legislators, we distinguish between environmental attention and explicit attribution to China. Instrumental-variables estimates provide little evidence that exogenous increases in local $PM_{2.5}$ raise politicians' broad environmental communication. By contrast, the clearest evidence consistent with strategic foreign attribution appears in partisan heterogeneity: conditional on discussing fine dust, conservative politicians increase foreign attribution in response to both transboundary and domestic shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. Transboundary environmental problems thus affect politics through selective blame shifting rather than a uniform increase in foreign attribution.

Keywords: air pollution, political communication, strategic attribution, transboundary externalities, South Korea

JEL Codes: Q53, D72, F64, P16

*Seonmin Will Heo: University of California, Santa Barbara, 2127 North Hall, Santa Barbara, CA 93106 (email: sheo@ucsb.edu). Mychaela Paetow: University of Southern California, 3620 S. Vermont Ave. Kaprielian Hall 363, Los Angeles, CA 90098 (email: mpaetow@usc.edu). We would like to thank David Schoenherr for his helpful comments.

†University of California, Santa Barbara

‡University of Southern California

1 Introduction

Environmental harms cross borders even when political responsibility does not. Yet when local air quality deteriorates, citizens cannot readily distinguish pollution transported from abroad from pollution generated domestically. This uncertainty creates room for two distinct political functions of foreign attribution: it can convey genuine information about an international spillover, or it can redirect attention away from domestic policy failure. Distinguishing between these possibilities is central to understanding the politics of environmental shocks. Do politicians respond differently to transboundary versus domestic pollution? And when they invoke a foreign source, are they informing the public about pollution's true origin or strategically shifting blame?

This paper studies these questions in the context of fine dust in South Korea. South Korea is a particularly useful setting because fine-dust episodes are highly salient, the contribution of Chinese transport is a recurring object of public debate, and politicians communicate frequently and publicly about environmental conditions. We combine separately identified transboundary and domestic pollution shocks with a politician-day panel of Facebook posts by district-seat members of the National Assembly to study both the attention and attribution margins of political response. Together these data let us distinguish how politicians enter the pollution conversation from how they assign responsibility within it.

The attribution problem we study connects to work on environmental accountability under source uncertainty. What remains less understood is how political actors narrate responsibility when citizens observe pollution but cannot easily observe its source.

The paper emphasizes two distinctions. The first is between *attention* and *attribution*. By attention, we mean whether politicians enter the pollution conversation at all; by attribution, we mean whether, once they do, they explicitly assign responsibility to China. Pollution may increase politicians' discussion of environmental issues without increasing their willingness to blame a foreign country. The second is between *actual foreign exposure* and *strategic foreign attribution*. If China-related pollution conditions raise China-blaming rhetoric, that may reflect a response to genuine transboundary exposure. If domestic pollution conditions raise the same rhetoric, that is evidence of strategic foreign attribution. These distinctions are conceptually clean but empirically difficult to separate because observed local pollution is a mixture of foreign and domestic sources and because political responses can unfold quickly.

We address these problems using two linked empirical designs at the politician-day level. The first is an instrumental variables design in which local $PM_{2.5}$ is instrumented with source-specific transboundary and domestic pollution shocks. That design identifies the causal effect

of local pollution on broad political attention. The second is a source-specific reduced-form design for attribution outcomes. It asks whether politicians become more likely to blame China when transboundary or domestic pollution shocks rise. The empirical setting combines a high-frequency city-level pollution panel with a new text-based measure of explicit China attribution in politicians' Facebook posts. Rather than relying on a broad China keyword or coarse topic tags, we construct an attribution outcome that isolates posts explicitly blaming China for fine dust and study both the unconditional margin (across all politician-days) and the conditional margin (among days when politicians already discuss fine dust).

Three main findings emerge. First, the attention-margin evidence is limited. In the preferred short-run specification, instrumented local pollution does not raise politicians' environmental Facebook posting; the point estimates are negative. The broad attention result is therefore more fragile than the attribution results below.

Second, we do not find evidence that the average politician systematically turns to explicit China blame when pollution rises across the full politician-day panel. On that unconditional margin, the average effects of transboundary and domestic pollution shocks on explicit China attribution are small. On the conditional margin, however, the estimates indicate that source-specific shocks can increase China blame once politicians are already discussing fine dust, especially in the same-day specification. The response is therefore not a generalized full-panel increase in foreign blame, but a conditional attribution response within fine-dust communication.

Third, the average null conceals meaningful partisan heterogeneity. Conditional on discussing fine dust, conservative politicians increase foreign attribution in response to both transboundary and domestic shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. The domestic-shock response is especially relevant to strategic foreign attribution because it appears even when the measured shock is domestic. The sharpest evidence in the paper is therefore not that all politicians blame China when pollution rises, but that conservative politicians are differentially more likely to use foreign attribution once they have entered the fine-dust conversation.

Additional placebo and auxiliary exercises help delimit the scope of that interpretation. Pollution does not produce comparably clear responses in placebo Facebook outcomes on non-environmental topics or in environmental bill introduction.

A downstream next-election exercise shows that political-career trajectories diverge sharply with this rhetorical choice. Among Democratic incumbents, a one-standard-deviation increase in pre-election China-blame share is associated with an 8 percentage point lower

probability of returning to the National Assembly, off a baseline survival rate of 62 percent. The corresponding association for conservative incumbents is small and statistically indistinguishable from zero. Because the outcome cannot separate electoral defeat from non-candidacy, we interpret this as evidence on political-career trajectories rather than as a clean voter-punishment estimate; under that broader interpretation, the contrast is consistent with the partisan attribution asymmetry the paper documents.

These findings contribute to several literatures. First, they speak directly to work on environmental accountability and foreign attribution under source uncertainty. When environmental harms cross jurisdictions, citizens may observe local damage without knowing which government, country, or set of firms is responsible. That uncertainty can weaken domestic accountability, but it can also create incentives for political actors to direct blame outward. Closest to ours, Song (2023) shows that media reports blaming China for air pollution increased substantially between 2015 and 2018 and that China-blaming articles can increase anti-Chinese sentiment and reduce support for closer relations with China. Lee and Voeten (2026) argue that transboundary air pollution creates "hazy accountability": bad air days worsen South Koreans' evaluations of Chinese leadership but do not produce a comparable decline in evaluations of the South Korean government's environmental performance. Our paper studies the political supply side of this broader accountability problem. Rather than asking how citizens evaluate governments after pollution exposure, we ask how elected politicians communicate about pollution, and whether their explicit foreign attribution responds to researcher-measured foreign or domestic pollution shocks. This distinction allows us to separate source-responsive foreign attribution from strategic foreign attribution when the underlying shock is domestic. We also provide downstream evidence that this rhetorical choice is associated with diverging political-career trajectories across parties.

Second, the paper speaks to work on political accountability for environmental shocks and environmental policy, where voters respond to natural disasters, pollution, and environmental public goods (Healy and Malhotra, 2009; Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Cole, Healy and Werker, 2012; Buntaine et al., 2024; Yao et al., 2022; Boomhower, 2024; Hazlett and Mildenberger, 2020; Anderson, Marinescu and Shor, 2023). Third, it connects to research on cross-border pollution and environmental spillovers across political jurisdictions, which has documented strategic interaction, free riding, and downstream damages at borders (Fredriksson and Millimet, 2002; Sigman, 2002, 2005; Helland and Whitford, 2003; Konisky and Woods, 2010; Kahn, Li and Zhao, 2015; Lipscomb and Mobarak, 2016; Wang and Wang, 2020; Heo, Ito and Kotamarthi, forthcoming; Sheldon and Sankaran, 2017). Finally, the paper contributes to emerging work using high-frequency text and social-media data to

study the salience and emotional consequences of pollution (Zheng et al., 2019).

Methodologically, the paper also contributes a dedicated text-based measure of explicit foreign attribution in politicians' communication. The measure is built from a two-stage weakly supervised classifier that first identifies whether a Facebook post discusses fine dust and then, conditional on relevance, identifies whether the post explicitly attributes the problem to China. Blame-related seed phrases are mined from a separate corpus of Korean news articles and combined with character n-gram features and broad topic labels from the Facebook archive. Much of the public debate over fine dust in Korea turns on language that is explicitly causal: phrases that do not merely mention China, but attribute pollution to China. Capturing that distinction matters for inference. A coarse *environment* × *China* tag would substantially overstate attribution, while our dedicated classifier isolates the narrower political behavior that the theory requires.

More broadly, the results suggest that transboundary environmental problems affect politics through two distinct channels: issue salience and blame attribution. Conflating them obscures both the politics of international externalities and the domestic incentives that shape public narratives about their source.

The rest of the paper proceeds as follows. Section 2 provides background on fine dust politics in South Korea and describes the construction of the pollution, Facebook, bill, and news data. Section 3 presents a simple conceptual framework that separates a broad attention margin from a narrower attribution margin and highlights when strategic foreign blame should be most attractive. Section 4 then maps that framework into the paper's instrumental variables and reduced-form designs. Section 5 presents the main findings together with supporting robustness and auxiliary evidence. Section 6 concludes.

2 Background and Data

2.1 Political and Environmental Background

South Korea provides a natural setting for studying the political consequences of transboundary pollution. Fine-dust episodes are highly visible, affect daily life in salient ways, and have become a recurring object of public debate. At the same time, the source of these pollution episodes is politically contested. Some pollution events are widely understood to reflect transport from China, while others are more plausibly driven by domestic emissions, local stagnation, or a combination of the two. This distinction is central to the paper.

If politicians respond differently to genuinely foreign pollution than to domestic pollution, then pollution is not merely an environmental issue; it is also a politically usable attribution problem.

The South Korean case is especially attractive because political elites communicate frequently and publicly about environmental conditions. During the period we study, politicians used Facebook extensively for direct communication with constituents, allowing us to observe high-frequency political responses to local pollution conditions. The same setting also makes strategic attribution plausible. When pollution can be blamed on a foreign source, politicians may be able to deflect responsibility away from domestic policy failures. Conversely, when foreign transport is genuinely important, politicians may have incentives to highlight that fact for reasons that are informational rather than strategic. The empirical challenge is therefore to distinguish real international spillovers from political scapegoating.

Party competition sharpens this question further. The Korean party system contains a meaningful divide between center-left parties and center-right or right-wing parties, and those blocs need not have the same incentives to frame pollution in external rather than domestic terms. The paper therefore treats partisan heterogeneity as a central object of interest rather than a secondary robustness check.

To make that comparison meaningful in the Korean setting, we do not rely on a single static party label for each politician. Korean parties are frequently renamed, merged, split, and redefined, and some politicians also switch party affiliation across Assembly cohorts. We therefore assign ideology at the politician-cohort level using the observed party affiliation in that cohort and a party-to-position crosswalk merged into the Facebook archive. The crosswalk maps party labels into five broad ideological positions: Left-wing, Center-left, Centrist, Center-right, and Right-wing. For the paper's two-bloc heterogeneity analysis, we then collapse these categories further so that Center-left politicians are coded as democratic, Center-right and Right-wing politicians are coded as conservative, and politicians coded as Left-wing, Centrist, or unaffiliated are excluded from the two-bloc comparison.

Figure 1 provides a descriptive view of how these broader party families are distributed across South Korea's district constituencies in the 2012, 2016, 2020, and 2024 National Assembly elections. The maps underscore the persistent regional structure of Korean party competition while also showing substantial turnover in the Seoul metropolitan area and other competitive districts over time.

2.2 Pollution, Weather, and Source-Specific Exposure

The environmental data begin from an hourly city-level panel for South Korea covering 2012–2024. For each city-hour, the panel contains observed local $PM_{2.5}$, weather conditions, and the source-specific variables used to construct the pollution measures in the instrumental variables design. The common analysis window for the main specifications is January 1, 2015 through December 31, 2023, which is the period over which the source-specific transport measures can be assembled consistently at high frequency.

The observed pollution outcome is local $PM_{2.5}$ measured at the Korean receptor. We merge this with hourly weather controls from Korean AWS and ASOS station data, aggregated to the city-hour level. The main weather controls used in the preferred specifications are local wind speed, temperature, and precipitation. We do not include humidity in the main specification because historical coverage begins later and would otherwise truncate the sample substantially.

To construct the transboundary pollution shock, we use the NOAA Hybrid Single-Particle Lagrangian Integrated Trajectory (HYSPLIT) model to trace the recent path of air masses arriving in Korean cities (Stein et al., 2015). For each city-hour, we run a backward trajectory from the Korean receptor and identify the portions of the trajectory that pass over China at low altitude. We then match those China-crossing trajectory points to contemporaneous $PM_{2.5}$ readings from nearby Chinese monitors and aggregate the matched values into a city-hour China-trajectory pollution signal. This measure should be interpreted as an ex post transport-based shock rather than a direct observation of emissions: it combines where the air arriving in Korea recently traveled with how polluted the relevant upwind Chinese locations were at the corresponding time. The domestic shock is constructed from the same trajectory information and local wind speed. It equals an indicator that the backward trajectory has no qualifying China segment interacted with inverse local wind speed, so it captures low-wind conditions in which domestic pollution is likely to accumulate when the arriving air mass is not classified as recently passing over China. Finally, both source-specific signals are aggregated from city-hours to politician-days using constituency-city weights and are used as excluded instruments for local $PM_{2.5}$ in the attention analysis and as source-specific regressors in the attribution analysis. Appendix Section A provides additional details. We also use them, together with observed local $PM_{2.5}$, to construct source-specific local $PM_{2.5}$ components for auxiliary source-decomposition exercises reported outside the main text.

The final step is to aggregate from cities to legislators. We map National Assembly constituencies to cities using a constituency-city crosswalk based on area shares and then com-

pute politician-specific exposure as the weighted average of city-level pollution within the politician's constituency. This produces daily politician-level measures of local total $PM_{2.5}$, local foreign $PM_{2.5}$, local domestic $PM_{2.5}$, and the associated source-specific shocks.

2.3 Facebook Posts by Politicians

The primary political outcomes are constructed from a comprehensive archive of Facebook posts by South Korean politicians. We focus on district-seat members of the 19th, 20th, and 21st National Assembly because constituency-linked pollution exposure is well defined for those legislators. Proportional representatives are excluded from the main analysis because they cannot be assigned a comparable constituency-specific pollution measure. After harmonizing politician identities and matching Facebook accounts to constituencies, the main politician-day panel contains 655 politicians observed over 3,286 dates, yielding 720,103 politician-day observations between 2015 and 2023.

Facebook is the main social-media source because it offers especially broad coverage among politicians in the study period while also preserving post-level text. Appendix Figure A1 reports social-media profile availability in the politician profile data for unique politicians who served in the 19th through 21st National Assembly cohorts. Facebook coverage is the highest among the platforms considered: 95.6 percent of politicians have a Facebook profile, compared with 88.2 percent for blogs, 64.7 percent for YouTube, 53.6 percent for Instagram, and 49.6 percent for Twitter/X. This coverage advantage makes Facebook a useful platform for constructing a broad, high-frequency panel of elite communication. Profile availability is not itself a measure of activity, so the empirical analysis relies on the scraped Facebook post archive to measure realized communication.

Appendix Figure A2 shows that posting activity is distributed throughout the life of each Assembly cohort rather than concentrated in a narrow slice of the sample period. That broad temporal coverage is important for the paper's high-frequency design because it ensures that politicians are observed communicating under many different pollution conditions.

Facebook is the core behavioral outcome for two reasons. First, it provides a high-frequency measure of political attention that can respond quickly to pollution episodes. Second, it allows us to distinguish between broad environmental attention and explicit blame assignment. At the politician-day level, we observe whether a politician posted at all, whether the politician posted about environmental issues, whether the politician posted about China, and how many such posts occurred on a given day.

For the strategic-attribution analysis, we require a more precise measure than a simple China keyword or broad topic tag. The relevant question is not whether a politician mentions China, but whether the politician attributes the fine-dust problem to China conditional on discussing air pollution. We therefore construct a dedicated post-level attribution measure using text classification, described below, and collapse that measure to politician-day outcomes.

2.4 Bills and Legislative Outcomes

As a secondary outcome, we assemble a politician-day panel of environmental bill introductions from the National Assembly's bill records. Using bill-level proposer information from the Assembly API, we match proposers to district-seat legislators and construct daily outcomes for whether a politician introduced an environmental bill and how many such bills were introduced. The resulting panel covers 752 district winners, of whom 750 are successfully matched to legislator codes, and yields 828,006 politician-day observations in 2015–2023. Within that window, we match 1,680 environmental bills to district-seat politicians.

Appendix Figures A3 through A6 illustrate the slower and more institutionally structured character of the bill process. Decision times are long, the distribution varies across Assembly cohorts, proposal timing is uneven over the legislative calendar, and pass rates differ substantially across committees. These patterns help explain why bill outcomes are much sparser and less responsive than Facebook communication in the main empirical analysis.

We treat legislative behavior as an informative but secondary outcome. Bills are much sparser than Facebook posts and reflect a slower, more institutionally constrained form of political response. For that reason, the bill analysis is reported primarily as supporting evidence and in the appendix rather than as the core empirical setting.

2.5 News Data and Text Measurement

To improve the measurement of attribution, we also collect a corpus of South Korean news articles mentioning China and fine dust. The raw archive contains 47,115 articles from 2001 to 2024. After cleaning, removing placeholders, and deduplicating by link and title-outlet combinations, the final corpus contains 26,728 articles covering 5,287 days. Roughly 81 percent of the cleaned articles contain usable body text. This news corpus serves two purposes. First, it provides background evidence on the language used in Korean public discussion of transboundary pollution. Second, it is used to discover and validate the blame-related phrases that anchor the post-level attribution measure. In other words, the news

corpus is not the text sample in which politician behavior is measured. It is an auxiliary source for building a news-derived attribution dictionary that captures how Korean public discourse links fine dust to China.

The main attribution outcome is then constructed from politicians' Facebook posts using a weakly supervised classifier trained in two stages. The first stage identifies whether a politician's post is substantively about fine dust or air pollution. The second stage, conditional on fine-dust relevance, predicts whether the post explicitly attributes the pollution problem to China. The classifier combines broad Facebook topic labels, seed phrases mined from the news corpus, and character n-gram features from the Facebook post text. This design allows the measure to distinguish posts that discuss fine dust without blaming China from posts that make an explicit foreign attribution. In the evaluation sample, the fine-dust classifier achieves an ROC AUC of 0.994 and the China-blame classifier achieves an ROC AUC of 0.995; Appendix Table A4 reports the full performance metrics.

This measurement step matters substantively. A broad *environment* $\times$ *China* tag intersection is too coarse for the paper's main question, because many posts can mention China without blaming China for air pollution. The classifier is designed to isolate the narrower concept that the theory requires: explicit foreign attribution conditional on discussing fine dust.

2.6 Sample Construction

The paper uses different samples for different outcomes, but all main specifications are built from a common politician-day panel. The broadest sample contains 655 district politicians and 720,103 politician-day observations from 2015–2023. The strategic-attribution sample is necessarily smaller because attribution is observed only on days when politicians discuss fine dust. Using the weak-classifier outcome, we observe 12,222 politician-days with at least one fine-dust post and 145 politician-days with at least one explicit China-blaming post. These fine-dust posting days are distributed across 567 politicians and 2,832 dates.

For the partisan heterogeneity analysis, we further restrict the sample to politicians that can be classified cleanly into conservative or democratic blocs. That leaves 10,855 politician-days with at least one fine-dust post, covering 523 politicians. Within this estimation sample, conservatives exhibit substantially more China attribution than democrats even in raw frequencies, a pattern that the regression analysis then formalizes.

Table 1 reports summary statistics for the paper's main estimation samples and for the

cleaned Naver news corpus used to construct and validate the attribution measure. The table is designed to track the paper's empirical objects closely: the politician sample, the main politician-day panel for attention outcomes, the conditional attribution sample, and the auxiliary news corpus.

Taken together, these data allow the paper to observe the same politicians under different local pollution conditions, distinguish foreign and domestic components of local exposure, measure both broad attention and explicit attribution, and connect those behaviors to party differences in political incentives. That combination is what makes it possible to speak to both international spillovers and strategic foreign blame within a unified empirical framework.

3 Conceptual Framework

This section presents a simple model of political communication under pollution shocks. The aim is not to characterize a full electoral equilibrium. Instead, the framework is designed to organize the paper's main empirical margins: whether pollution increases political attention, when politicians explicitly blame China, why attribution responses differ across politicians, and why the clearest responses appear in fast-moving communication rather than in slower institutional outcomes.

The central distinction is between two choices. On any given day, a politician first decides whether to speak publicly about pollution at all. Conditional on speaking, the politician then decides whether to frame the episode as China's fault. The first choice is driven by issue salience. The second is driven by political incentives.

3.1 Setup

Consider politician i on day t. Let P_{it} denote the salience of the local pollution problem. Higher values of P_{it} correspond to worse local pollution conditions and therefore greater public visibility of the issue.

Let A_{it} denote domestic accountability pressure. This is not the true domestic share of pollution per se. Rather, it captures how strongly the day's pollution episode would, absent reframing, expose domestic actors to criticism. Intuitively, A_{it} is high when the episode creates stronger pressure on domestic politicians, regulators, or the government to answer for bad air quality. In those moments, shifting attention outward becomes more politically

useful.

We treat A_{it} as a reduced-form summary of latent accountability pressure rather than as a primitive of the model. The empirical proxy we adopt — exposure to domestically generated pollution shocks — rests on the assumption that the political stakes for domestic actors are higher when the underlying pollution episode is domestically generated, because in such episodes the question of whom to hold responsible most naturally points inward. Strategic foreign attribution is precisely the political response that suppresses this latent pressure from manifesting as observed criticism of domestic actors. Existing work on Korean fine-dust politics is consistent with this account: media coverage blaming China for air pollution rose substantially as fine-dust episodes intensified (Song, 2023), and bad air days worsen citizens' evaluations of foreign leaders without producing a comparable decline in evaluations of the domestic government (Lee and Voeten, 2026).

On each day, the politician makes two related choices. Let

$$m_{it} \in \{0, 1\}$$

denote whether the politician communicates publicly about pollution, and let

$$b_{it} \in \{0, 1\}$$

denote whether that communication explicitly blames China. The constraint

$$b_{it} \leq m_{it}$$

rules out blaming China without first entering the pollution conversation.

The politician solves

$$(m_{it}^*, b_{it}^*) = \arg \max_{m,b \in \{0,1\},\, b \leq m} U_{it}(m, b), \tag{1}$$

where

$$U_{it}(m, b) = m \left[\alpha_i + \beta P_{it} - c_i^m\right] + b \left[\theta_i + \sigma_i A_{it} - c_i^b\right]. \tag{2}$$

Equation (2) separates the attention margin from the attribution margin. The first bracket is the payoff from speaking about pollution at all. The second bracket is the additional payoff from using an explicit China-blame frame once the politician has chosen to speak.

3.2 Three Actions

The optimization problem implies three feasible action values. If the politician remains silent, $(m, b) = (0, 0)$, then

$$U_{it}(0, 0) = 0. \tag{3}$$

If the politician speaks about pollution without blaming China, $(m, b) = (1, 0)$, then

$$U_{it}(1, 0) = \alpha_i + \beta P_{it} - c_i^m. \tag{4}$$

If the politician speaks and explicitly blames China, $(m, b) = (1, 1)$, then

$$U_{it}(1, 1) = \alpha_i + \beta P_{it} - c_i^m + \theta_i + \sigma_i A_{it} - c_i^b. \tag{5}$$

These three values correspond closely to the paper's empirical outcomes: silence, pollution communication without foreign blame, and pollution communication with explicit China attribution. They also clarify the difference between the paper's unconditional and conditional attribution measures. The unconditional attribution margin corresponds to whether $(1, 1)$ beats both silence and non-blaming communication. The conditional attribution margin corresponds to whether $(1, 1)$ beats $(1, 0)$ once the politician is already discussing pollution.

3.3 Attention

The term

$$\alpha_i + \beta P_{it} - c_i^m$$

captures the net payoff from entering the pollution conversation.

The parameter α_i is a politician-specific baseline propensity to speak on environmental issues. The coefficient $\beta > 0$ captures the idea that more severe pollution makes the issue more salient and therefore more politically valuable to address. The term $c_i^m > 0$ is the cost of producing pollution-related communication, including time, effort, and the opportunity cost of not talking about another issue.

The politician speaks whenever the better of the two communication options weakly dominates silence:

$$\max\{U_{it}(1, 0), U_{it}(1, 1)\} \geq U_{it}(0, 0). \tag{6}$$

Equivalently,

$$\alpha_i + \beta P_{it} - c_i^m + \max\{0, \theta_i + \sigma_i A_{it} - c_i^b\} \geq 0. \tag{7}$$

This expression highlights two ways pollution can increase communication. First, higher pollution directly raises the return to speaking through βP_{it}. Second, if the option to blame China is itself politically attractive, that can further increase the value of entering the conversation. Empirically, this prediction provides a benchmark for the attention analysis, even though the main results below show limited evidence that local pollution robustly increases broad environmental posting.

Prediction 1. Higher pollution salience should increase broad political attention to environmental issues.

This prediction maps directly to the paper's IV attention regressions, where the relevant empirical object is the effect of local pollution on environmental Facebook posting.

3.4 Attribution

Conditional on speaking, the politician blames China when

$$U_{it}(1,1) \geq U_{it}(1,0), \tag{8}$$

or equivalently,

$$\theta_i + \sigma_i A_{it} \geq c_i^b. \tag{9}$$

This is the key condition in the model. The parameter θ_i captures the politician's baseline attraction to a China-blame frame. This can reflect ideology, rhetorical style, partisan positioning, or a standing preference for externalizing responsibility. The parameter σ_i captures how useful that frame becomes when domestic accountability pressure rises. Politicians with larger values of σ_i gain more from shifting blame outward precisely when the pollution episode creates stronger pressure on domestic actors. The term $c_i^b > 0$ is the additional cost of using an explicit China-blame frame rather than generic pollution communication.

This formulation is deliberately political rather than epistemic. The model does not assume that politicians are rewarded for correctly identifying the true source of pollution. Instead, it treats China blame as a political frame whose attractiveness depends on whether it serves the politician's incentives in the current domestic context.

Prediction 2. Politicians should be more likely to blame China when domestic account-

ability pressure is higher.

Prediction 3. The response of China attribution to domestic accountability pressure should be more positive for politicians with stronger incentives to externalize responsibility.

These comparative statics characterize the paper's main mechanism for strategic foreign attribution.

3.5 Heterogeneity

The model also makes clear why average effects may be small even when meaningful heterogeneity exists. Politicians can differ in their baseline propensity to discuss pollution, α_i, in their baseline attraction to China blame, θ_i, and in how strongly they value outward blame shifting when domestic accountability pressure rises, σ_i.

This is important for interpreting the empirical results. A small average attribution response does not imply that strategic blame is absent. It may instead reflect offsetting behavior across politicians with different values of θ_i and σ_i. Observable characteristics such as party affiliation can therefore be interpreted as empirical proxies for these latent incentive parameters rather than as primitive parameters of the theory itself. In the empirical analysis, the conservative-Democratic distinction operationalizes systematic variation in θ_i and σ_i across politicians.

3.6 Other Political Actions

The same logic extends to other political outcomes. Suppose politicians can respond through different channels g, such as Facebook posts or bill introductions, and let the communication cost depend on the channel, c_{ig}^m. If social-media communication is cheaper and faster than legislative action, then short-run pollution shocks should generate clearer responses in Facebook posting than in bill introduction. The theory therefore predicts that the attention and attribution margins should be most visible in low-cost communication outcomes.

Prediction 4. The effects of pollution salience and blame-shifting incentives should be strongest in fast-moving, low-cost communication rather than in slower institutional outcomes.

The framework also helps interpret the paper's downstream next-election exercise. Parameters such as θ_i and σ_i should be understood as perceived political returns to a China-blame

strategy, not as proof that this strategy is electorally rewarded ex post. The election analysis is therefore a check on whether the political-career trajectories of politicians who rely more heavily on that frame look different from those who do not.

The framework deliberately omits two sets of mechanisms that a more complete model would address. First, voters' belief formation about pollution sources is outside the model: the parameters θ_i and σ_i summarize the political returns to a China-blame frame rather than deriving them from voter learning. Second, strategic interaction between competing politicians — how a politician's framing choice responds to rivals' — is suppressed. These omissions are deliberate: a richer model would generate predictions about belief updating and within-district competition that the paper's data, which observe communication choices but not voter beliefs or rival-pair dynamics, are not designed to test.

3.7 Empirical Mapping

The model maps naturally to the paper's empirical design. The attention margin depends on overall pollution salience P_{it}, which is why the attention analysis focuses on total local pollution. The attribution margin depends on domestic accountability pressure A_{it}, for which domestically generated pollution conditions are the most natural empirical proxy: they are the cases in which blame shifting is most politically useful.

The transboundary shock remains important as a comparison case even though it does not appear directly in the utility function. If explicit China blame rose mainly with transboundary shocks, that would look more like a source-responsive narrative. By contrast, if politicians, especially those with stronger incentives to externalize responsibility, are more likely to blame China when domestic pollution rises, that is evidence of strategic foreign attribution.

Taken together, the framework organizes the paper around two distinct political margins. Pollution first affects whether politicians talk. Conditional on talking, political incentives shape how responsibility is assigned. That distinction is what allows the empirical analysis to separate broad issue attention from strategic foreign blame.

4 Empirical Strategy

This paper studies two related questions. First, does transboundary air pollution from China spill over into South Korean political behavior? Second, when local pollution is domestically

generated, do politicians strategically attribute that pollution to China? Guided by the simple framework in Section 3, the empirical design is built around source-specific variation in local particulate matter and high-frequency measures of political behavior. The paper uses two linked empirical frameworks: a two-stage least squares design for broad political attention outcomes and source-specific reduced-form regressions for attribution outcomes.

4.1 Source-Specific Local Exposure

Let c index Korean cities, h index hours, t index days, i index politicians, and m index air-quality monitors. The empirical design begins from two daily politician-level source-specific shocks: a transboundary pollution shock, denoted Z_{it}^F, and a domestic pollution shock, denoted Z_{it}^D. The term "domestic pollution shock" is shorthand for a source-specific proxy for local pollution conditions that are not classified as recently transported from China.

At the city-hour level, the transboundary shock underlying Z_{it}^F, which serves as the foreign instrument, is constructed from HYSPLIT backward trajectories. For each Korean receptor city and arrival hour, we trace the recent path of the arriving air mass and keep the portions of the path that pass over China below 1 kilometer altitude. Each qualifying China point is matched to the nearest Chinese $PM_{2.5}$ monitor observed in the corresponding hour. The city-hour instrument is the average of these matched Chinese monitor readings, and it is set to zero when the low-altitude trajectory does not cross China. After aggregation to the politician-day level, this becomes Z_{it}^F. This measure is high when air reaching Korea recently passed over polluted parts of China; it should be read as a transport-based shock rather than a direct measure of Chinese emissions.

Operationally, the domestic shock is built at the city-hour level by interacting the non-China trajectory indicator with inverse local wind speed:

$$Z_{ch}^D = \mathbf{1}\{\text{No China}_{ch}\} \times \frac{1}{\text{WindSpeed}_{ch}}, \tag{10}$$

where $\mathbf{1}\{\text{No China}_{ch}\}$ equals one when the low-altitude backward trajectory for city c and hour h has no qualifying China segment. Wind speed is measured in meters per second, and observations with missing or nonpositive wind speed are excluded from this variable. This construction is designed to proxy for domestic pollution conditions: it captures low-wind accumulation precisely when the arriving air mass is not classified as transboundary from China, rather than relying on $PM_{2.5}$ readings from Korean upwind monitors.

Both source-specific hourly signals are averaged to daily city-level shocks and then aggregated

to the politician-day level using constituency-city weights:

$$Z_{it}^{s} = \sum_{c \in \mathcal{C}(i)} \omega_{ic} Z_{ct}^{s}, \qquad s \in \{F, D\}, \tag{11}$$

where ω_{ic} is the share of politician i's constituency that maps to city c, and $\sum_c \omega_{ic} = 1$ within a constituency.

Because Z_{it}^{D} is an interaction involving inverse wind speed rather than a $PM_{2.5}$ concentration, its raw units are less substantively familiar than the transboundary shock. The results tables report a common 10-unit scaling for compactness, while the main interpretation of domestic-shock magnitudes uses empirical movements, especially the interquartile range of the daily politician-level shock. In the main text, we use "domestic pollution shock" as the intuitive label and rely on this section and the table notes to define the exact measurement.

In addition to these source-specific shocks, we observe local total $PM_{2.5}$ at the Korean receptor and aggregate it to the politician-day level. For some auxiliary specifications, we also construct source-specific local $PM_{2.5}$ components. Those decompositions are informative, but the main text does not rely on them for identification because the strongest and cleanest evidence in the paper comes from a combination of total-PM instrumental variables estimates and source-specific reduced-form attribution estimates.

For political communication outcomes, the most natural response horizon is short. We therefore focus on short-run average exposure windows of length $d \in \{0, 3, 7\}$, where

$$\bar{X}_{it}(d) = \frac{1}{d+1} \sum_{\ell=0}^{d} X_{i,t-\ell}, \tag{12}$$

where X_{it} denotes either local $PM_{2.5}$ or one of the source-specific shocks. In the main text, the preferred specification for Facebook posting behavior uses $d = 3$, with $d = 0$ and $d = 7$ reported as supporting evidence.

4.2 Main Design for Political Attention

The central estimating equation is

$$Y_{it} = \beta \bar{P}_{it}(d) + W_{it}'\Gamma + \alpha_i + \lambda_t + \varepsilon_{it}, \tag{13}$$

where Y_{it} is a political outcome, $\bar{P}_{it}(d)$ is the average of local total $PM_{2.5}$ over days t through $t-d$, W_{it} is a vector of weather controls, α_i are politician fixed effects, and λ_t are calendar-date fixed effects. In the baseline specifications, W_{it} includes local wind speed, temperature, and precipitation. The politician fixed effects absorb time-invariant differences across legislators, such as ideology, constituency characteristics, and baseline posting intensity. The date fixed effects absorb national shocks common to all politicians on a given day, including national political events, country-wide media cycles, and common pollution episodes.

Equation (13) is estimated by two-stage least squares using the source-specific shocks as excluded instruments. The first stage is

$$\bar{P}_{it}(d) = \pi_F \bar{Z}^F_{it}(d) + \pi_D \bar{Z}^D_{it}(d) + W'_{it}\Gamma_P + \alpha_i + \lambda_t + u_{it}. \tag{14}$$

Intuitively, the foreign instrument captures exogenous variation in the delivery of China-origin pollution to South Korea, while the domestic instrument captures exogenous variation in locally generated pollution. Using both instruments together isolates the component of local $PM_{2.5}$ driven by plausibly exogenous variation in pollution transport and accumulation rather than by endogenous political or media conditions.

This 2SLS design is the paper's main causal framework for broad political attention outcomes. The coefficient of interest is β, which captures the effect of exogenous changes in local pollution on the probability and intensity of politicians' environmental communication. The source-specific variation matters because it supplies the excluded instruments; the second-stage object is the effect of local pollution itself.

4.3 Political Outcomes

The primary outcomes are constructed from politicians' Facebook posts and measured at the politician-day level. We use two broad classes of outcomes.

First, to study international spillovers into political attention, we examine general environmental engagement: whether politician i posted about environmental issues on day t, and how many such posts they made. These are the cleanest outcomes for establishing whether pollution shocks affect political behavior at all.

Second, to study strategic attribution, we focus on explicit blame assignment. We report both unconditional and conditional attribution outcomes because they capture different margins of political response. The attribution outcomes are:

1. an unconditional indicator for whether the politician made any explicit China-blaming post on day t; and

2. an indicator for whether the politician made any China-blaming post on day t, conditional on making at least one fine-dust post.

The unconditional indicator captures the overall amount of China-blaming communication that enters the public sphere without conditioning on an endogenous posting decision. The conditional indicator captures a different and substantively important question: once politicians are already discussing fine dust, how do they assign blame? Together the two outcomes separate the extensive margin of visible foreign blame from the narrower attribution choice within pollution-related discussion. Both outcomes are constructed from post text using a dedicated classifier designed to distinguish explicit blame of China from general discussion of air pollution or mere mention of China.

We also examine environmental bill introductions as a secondary outcome. Because legislative activity is much sparser and slower moving than social media posting, we treat the bill results as supporting evidence rather than the core specification.

4.4 Identification of International Spillovers

The first question is whether exogenous local pollution affects South Korean political outcomes. For that exercise, identification comes from within-day differences across politicians in instrumented exposure to local pollution. Because calendar-date fixed effects absorb national shocks, the coefficient is not identified by comparing more polluted days to less polluted days in the aggregate. Instead, it is identified by comparing politicians whose constituencies are more exposed than others on the same day after conditioning on the source-specific transport and domestic accumulation shocks.

This comparison is attractive in the present setting for two reasons. First, it sharply limits concerns that national news cycles or aggregate political events drive both pollution and political response. Second, it matches the mechanism of interest: the relevant treatment is not whether South Korea as a whole is experiencing a polluted day, but whether some politicians are more exposed than others to China-origin pollution because of the geography of transport and constituency location.

4.5 Identification of Strategic Foreign Attribution

The second question is whether politicians blame China even when pollution conditions are driven by domestic rather than transboundary shocks. For that exercise, the most relevant empirical object is the reduced-form response of attribution behavior to the source-specific shocks themselves. The reason is substantive as well as econometric. For attribution outcomes, politicians may respond not only to locally realized pollution but also to the salience of source-specific pollution conditions. In that setting, the reduced form is not merely a diagnostic; it is a meaningful estimand.

A key interpretive point is that the source-specific shocks are ex post measures of pollution origin constructed from atmospheric transport and local pollution data, not signals that politicians are assumed to observe perfectly in real time. Politicians likely observe local air quality, media coverage, forecasts, constituent complaints, and partisan narratives, but they generally do not know with certainty whether a given pollution episode is domestically generated or transported from abroad. This uncertainty is central to the paper's interpretation. If China-blaming posts rise mainly with transboundary shocks, the pattern is consistent with politicians responding to source-relevant public cues. If China-blaming posts also rise with domestic pollution shocks, the pattern is harder to reconcile with accurate source attribution alone and is instead consistent with strategic foreign attribution: politicians use China as a blame frame even when the estimated shock points to domestic rather than transboundary pollution conditions.

We therefore estimate

$$A_{it} = \rho_F \bar{Z}^F_{it}(d) + \rho_D \bar{Z}^D_{it}(d) + W'_{it}\Gamma^A + \alpha_i + \lambda_t + \nu_{it}, \tag{15}$$

where A_{it} denotes one of the China-attribution outcomes defined above. In the main text, we report this specification for both the unconditional indicator for any explicit China-blaming post on day t and, on the subsample of days with at least one fine-dust post, the corresponding conditional attribution indicator.

For these attribution regressions, a positive ρ_F indicates that politicians become more likely to blame China when the transboundary shock rises, while a positive ρ_D indicates that they become more likely to blame China when the domestic pollution shock rises. The latter pattern is the paper's core evidence of strategic foreign attribution.

4.6 Partisan Heterogeneity

The paper's strategic-attribution argument also has a partisan implication: conservative politicians may have stronger incentives than democratic politicians to attribute pollution to China. We test this directly by interacting the source-specific shocks with an indicator for whether politician i belongs to the conservative bloc:

$$\begin{aligned} Y_{it} = &\rho_F \bar{Z}^F_{it}(d) + \rho_D \bar{Z}^D_{it}(d) \\ &+ \theta_F \left[\bar{Z}^F_{it}(d) \times \text{Conservative}_i\right] \\ &+ \theta_D \left[\bar{Z}^D_{it}(d) \times \text{Conservative}_i\right] \\ &+ W'_{it}\Gamma + \alpha_i + \lambda_t + \varepsilon_{it}. \end{aligned} \tag{16}$$

Party labels for this exercise are assigned at the politician-cohort level using the cohort-specific party affiliation merged to the post archive. We then collapse those labels into broad ideological blocs, coding Center-left politicians as democratic and Center-right or Right-wing politicians as conservative; centrists, left-wing parties, and unaffiliated politicians are excluded from the two-bloc comparison. Because politician fixed effects absorb the main effect of party affiliation, the coefficients of interest are the interaction terms. In the attribution regressions, $\theta_D > 0$ indicates that conservative politicians are more likely than democratic politicians to blame China when domestic pollution rises. That is the sharpest test of partisan strategic attribution in the data.

4.7 Exploratory Design for Next-Election Survival

As a downstream extension, we also examine whether pre-election pollution exposure and communication behavior predict incumbents' survival to the next National Assembly election. This design is intentionally more modest than the paper's politician-day specifications. The unit of observation is an incumbent-next-election pair rather than a politician-day. For each district-seat legislator in cohorts 19 through 21, we assign the date of the next Assembly election and aggregate the source-specific shocks and Facebook outcomes over the 180 days before that election. The resulting panel contains one observation per incumbent with measures of mean transboundary and domestic shock exposure, environmental posting intensity, fine-dust posting intensity, and the share of China-blaming posts among fine-dust posts.

We then estimate linear probability models in which the outcomes are indicators for whether the incumbent later wins any district seat and whether the incumbent wins again in the

same constituency. In addition to the pre-election exposure and communication measures, the specifications control for standardized electoral vulnerability based on the incumbent's previous vote share together with cohort fixed effects and broad politician-position controls. Because the outcome is observed only from the next election's winner roster, a zero combines incumbents who ran and lost with incumbents who did not run again. We therefore interpret this exercise as exploratory evidence on downstream electoral accountability rather than as a design comparable in identification strength to the paper's high-frequency fixed-effects framework.

4.8 Inference and Interpretation

In all politician-day specifications, standard errors are two-way clustered by politician and calendar date. This allows for serial correlation in outcomes within politician and arbitrary cross-sectional correlation across politicians on the same date. For transparency, we report the first-stage strength of the excluded instruments alongside the second-stage estimates in the IV attention regressions.

For the IV attention regressions, the identifying assumption is that, conditional on weather controls, politician fixed effects, and date fixed effects, the source-specific shocks affect political behavior only through locally realized pollution. That assumption is strongest for broad environmental attention outcomes. For explicit attribution outcomes, the possibility that politicians respond directly to source salience is part of the mechanism of interest rather than a nuisance, which is why the main attribution estimates are reduced form.

Two additional caveats are important for interpretation. First, the exclusion restriction is more demanding for the domestic shock than for the transboundary shock. Although the domestic measure is designed to capture plausibly exogenous variation in locally generated pollution conditions, broader domestic pollution episodes may also shift media attention, shared partisan narratives, or national accountability pressures in ways that are not fully mediated by pollution realized within a given politician's constituency. Second, the attribution regressions that condition on at least one fine-dust post are estimated on an endogenous subsample, because source-specific shocks can affect not only how politicians assign blame once they discuss pollution, but also whether they enter that discussion at all. We therefore interpret those conditional estimates as evidence on a substantively important within-conversation margin rather than as a stand-alone causal effect for a fixed population of posts, and we treat the domestic-shock attribution results throughout as suggestive evidence on strategic foreign blame rather than as the paper's cleanest causal design.

Taken together, this design allows the paper to distinguish three conceptually separate objects: the causal effect of local pollution on political attention, the average reduced-form response of foreign blame to source-specific shocks, and the incremental tendency of conservative politicians to attribute domestic pollution to a foreign source.

5 Main Results

The main text presents three sets of core results and then turns to supporting robustness and auxiliary evidence. Table 2 reports the instrumental-variables relationship between local pollution and politicians' general environmental attention. Table 3 then compares two attribution margins: the overall amount of visible China-blaming communication and the narrower attribution choice once politicians are already discussing fine dust. Finally, Table 4 shows that the average null masks meaningful partisan heterogeneity, especially on the conditional attribution margin: conservative politicians increase foreign attribution in response to both transboundary and domestic shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. We then show that this interpretation is robust to alternative attribution measurement and that adjacent outcomes, such as placebo-topic posting, environmental bill introduction, and next-election survival, provide weaker and more exploratory downstream evidence.

5.1 Local Pollution and Environmental Attention

We begin by asking whether exogenous local pollution increases politicians' attention to environmental issues. Table 2 reports instrumental variables estimates in which local $PM_{2.5}$ is instrumented with the source-specific transboundary and domestic shocks described in Section 4. The preferred specification uses the 0–3 day average exposure window, includes politician and date fixed effects, and controls for local wind speed, precipitation, and temperature.

The first stage remains strong. Appendix Table A1 reports the corresponding first-stage coefficients and diagnostic statistics. In the preferred specification, the excluded-instrument first-stage F-statistic is approximately 77. Both source shocks move local pollution in the expected direction, with the domestic instrument accounting for the larger share of short-run variation in observed local $PM_{2.5}$.

Figure 2 visualizes these first-stage relationships in the preferred specification. After par-

tialling out politician fixed effects, calendar-date fixed effects, weather controls, and the other source-specific shock, both instruments remain strongly positively related to local $PM_{2.5}$. Because the date fixed effects absorb common national shocks, the figure makes clear that the identifying variation comes from within-date differences in exposure across politicians.

The second-stage estimates do not support a positive attention effect. In the preferred 0–3 day average specification, a 10 $\mu g/m^3$ increase in instrumented local $PM_{2.5}$ reduces the probability that a politician makes any environmental Facebook post by 0.555 percentage points ($p = 0.037$) and reduces the number of environmental posts by 0.515 per 100 politician-days ($p = 0.101$). The same-day estimates are also negative, though less precise. Taken together, these estimates should not be interpreted as evidence that local pollution robustly increases broad environmental posting.

Appendix Tables A2 and A3 put these IV estimates next to OLS specifications that use the same samples, outcomes, fixed effects, and weather controls. The OLS estimates are small, positive, and statistically insignificant: in the preferred 0–3 day average specification, the OLS coefficient is 0.079 percentage points for any environmental post ($p = 0.179$) and 0.101 posts per 100 politician-days for the number of environmental posts ($p = 0.148$). The contrast suggests that the negative IV estimates are not a generic negative association between observed $PM_{2.5}$ and environmental posting. They instead appear specific to the local, source-driven variation isolated by the instruments.

This first result is important for the paper as a whole because it narrows the role of the attention analysis. The analysis therefore avoids interpreting the negative IV coefficients as evidence that pollution reduces political attention. Rather, Table 2 and the OLS comparisons in the appendix suggest that local source-driven pollution exposure does not produce a robust increase in broad environmental posting. The stronger evidence in the paper comes from the attribution margins below.

5.2 Foreign Attribution Across Two Margins

We next turn to explicit foreign attribution. Table 3 reports two complementary outcomes that answer different questions. Panel A uses the full politician-day panel and asks whether higher pollution shocks increase the overall amount of visible China-blaming communication. This is a demanding outcome: in the full panel there are only 145 attribution days in 720,103 politician-days spanning 655 politicians and 3,286 dates. Panel B conditions on politician-days with at least one fine-dust post and asks whether politicians become more likely to blame China once they are already discussing pollution. The same-day conditional regression

sample contains 11,563 observations across 489 politicians.

The table reports coefficients using a 10-unit scale for both source-specific shocks, but the domestic shock is not measured in $PM_{2.5}$ units. It is a non-China trajectory indicator interacted with inverse wind speed, so a more natural substantive benchmark is movement across its empirical distribution. Appendix Figure A7 shows that the daily domestic shock is concentrated near zero with a right tail. In the full politician-day panel, the interquartile range runs from 0.19 to 1.35; in the fine-dust posting sample, it runs from 0.20 to 1.39.

Panel A delivers the stronger null. In the daily specification, a 10-unit increase in the transboundary shock raises the probability of any China-blaming post by 0.008 percentage points ($p = 0.082$). The corresponding domestic-shock coefficient is 0.043 percentage points on the table's 10-unit scale ($p = 0.113$), which is only about 0.005 percentage points for an interquartile-range increase in the full panel. The 0–3 day average estimates are smaller and statistically indistinguishable from zero. Across windows, we do not find evidence that source-specific shocks systematically increase visible China-blaming communication overall. The small unconditional estimates are informative: domestic pollution shocks do not generate a broad surge of visible China-blaming communication. Instead, the evidence points to a narrower attribution margin that operates once politicians have already entered the fine-dust conversation.

Panel B addresses the narrower attribution question directly. Once we condition on days with at least one fine-dust post, the estimated effects become larger: the same-day coefficients are 0.629 percentage points for the transboundary shock and 3.383 percentage points for the domestic shock on the table's 10-unit scale. For the domestic shock, this translates to roughly 0.40 percentage points for an interquartile-range increase in the fine-dust posting sample, relative to a baseline attribution probability of 1.19 percent. The conditional estimates should therefore be read as evidence about how politicians assign responsibility within fine-dust communication, rather than as evidence of a large increase in the overall volume of China-blaming posts. The conditional estimates indicate that source-specific shocks can increase China blame once politicians are already discussing fine dust, especially in the same-day specification. Because the two shocks are measured in different units, the relative magnitudes should be interpreted cautiously; the key point is that source-specific shocks shape blame assignment conditional on fine-dust discussion, even though they do not generate broad China-blaming communication overall.

5.3 Partisan Heterogeneity in Strategic Attribution

The final set of main-text results asks whether the average null conceals systematic partisan differences. Table 4 mirrors the two-margin structure of Table 3. Panel A studies the unconditional extensive margin, while Panel B conditions on politician-days with at least one fine-dust post. The raw unconditional difference is small in levels but still asymmetric: conservatives make explicit China-blaming posts on 0.027 percent of politician-days, compared with 0.010 percent for democrats.

Appendix Table A5 shows that this pattern is not driven by a single outlying account. In the partisan sample, 66 unique politicians ever make at least one explicit China-attribution post, including 39 conservatives and 27 democrats. The most active politician accounts for only 8 percent of attribution days overall, which is inconsistent with a one-politician story.

The regression results show that this raw difference is not merely compositional. Figure 3 presents the daily conditional estimates from Panel B of Table 4, which are the sharpest version of the partisan-attribution result. Because the domestic and transboundary shocks are measured on different scales, the figure reports effects of an interquartile-range increase in each shock, while Table 4 keeps the original 10-unit coefficient scale. Once we condition on politician-days with at least one fine-dust post, neither shock meaningfully changes attribution among Democratic politicians. Conservatives, by contrast, respond positively to both source-specific shocks. In the partisan fine-dust posting sample, an interquartile-range increase is 16.29 units for the transboundary shock and 1.21 units for the domestic shock. The corresponding conservative total effects are about 1.66 percentage points for the transboundary shock and 0.97 percentage points for the domestic shock, relative to a baseline attribution probability of 1.05 percent. The main pattern in Figure 3 is therefore partisan: conservatives increase foreign attribution for both source-specific shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. The domestic-shock response remains important for the strategic-attribution interpretation because it appears even when the measured shock is domestic.

Appendix Figure A8 shows the corresponding daily unconditional estimates from Panel A. On the full politician-day panel, the same substantive asymmetry is visible but much smaller in levels because explicit China-blaming posts are rare overall. In that unconditional specification, the conservative differential on the domestic shock is 0.095 percentage points ($p = 0.121$), while the differential on the transboundary shock is 0.012 percentage points ($p = 0.078$). The total same-day effects for conservatives are 0.096 percentage points for the domestic shock ($p = 0.089$) and 0.013 percentage points for the transboundary shock

($p = 0.022$).

The 0–3 day average estimates are directionally similar and stronger on the conditional margin: in Panel B, the corresponding domestic differential remains positive at 12.610 percentage points ($p = 0.009$). Taken together, the two panels suggest that the partisan difference operates primarily through the attribution stage rather than through broad posting volume alone.

We therefore treat the daily specification as the most informative version of the partisan-attribution result and view the moving-average estimates as supportive but not decisive.

Appendix Table A6 provides a simple descriptive check on whether these posts appear unusually popular on Facebook. China-blaming fine-dust posts do not exhibit systematically higher engagement than other fine-dust posts in the raw archive. Mean total engagement is somewhat lower for attribution posts overall, while median engagement is nearly identical, suggesting that the partisan attribution result is not simply picking up a small set of exceptionally high-performing posts.

5.4 Interpretation and Scope

Taken together, the three main tables support a more nuanced conclusion than a simple "pollution leads politicians to blame China" narrative. First, the IV attention estimates do not support the claim that local pollution increases politicians' broad environmental posting, even though the corresponding OLS associations are small and positive. Second, source-specific shocks do not translate into a large increase in visible foreign blame across the full politician-day panel, but the conditional estimates indicate that they can increase China blame once politicians are already discussing fine dust, especially in the same-day specification. Third, once we distinguish between unconditional and conditional attribution margins and allow the response to differ by party, a more suggestive pattern emerges: conditional on discussing fine dust, conservatives increase foreign attribution in response to both transboundary and domestic shocks, whereas Democratic estimates are small and statistically indistinguishable from zero.

That combination of results shifts the paper's emphasis toward the attribution margin. The evidence for a broad attention response is fragile, while the attribution margin is narrower, more politically charged, and concentrated among actors with stronger incentives to externalize responsibility. In this sense, the paper's contribution is not merely to ask whether pollution changes the volume of political communication, but to identify the conditions under

which environmental shocks are translated into strategic foreign blame.

5.5 Robustness: Alternative Attribution Measurement

The main attribution results rely on the weakly supervised text classifier, which is designed to isolate explicit China blame from broader discussion of air pollution or generic mention of China. A natural concern is that the findings may depend on this particular measurement strategy. Appendix Table A7 addresses that concern by replacing the classifier outcome with a more conservative dictionary-based attribution measure in the conditional fine-dust sample.

The dictionary measure is substantially sparser, yielding only 2,243 politician-days with at least one fine-dust post and far fewer attribution days than the classifier-based sample. That loss of coverage makes the estimates noisier, but it does not overturn the substantive message. In the daily specification, the transboundary coefficient remains positive and the domestic coefficient remains close to zero or negative, but neither is statistically distinguishable from zero. The same is true in the 0–3 day average specification. In other words, the absence of strong average attribution effects is not an artifact of the classifier. If anything, the classifier's main contribution is to recover a much less sparse and more behaviorally useful outcome measure.

Appendix Table A4 provides further support for this interpretation by documenting the classifier's performance. The fine-dust screen and the conditional China-blame classifier both perform strongly in held-out evaluation samples, which increases confidence that the main-text attribution outcomes are capturing explicit blame rather than generic China discussion.

5.6 Placebo Tests and Legislative Outcomes

Two additional exercises help clarify the scope of the main findings. The first uses placebo Facebook outcomes on topics unrelated to environmental conditions. Appendix Table A8 estimates the same source-specific reduced-form design using indicators for whether politicians posted about the economy or labor, education, housing, or election and campaign activity, excluding posts classified as environmental. The estimates provide little evidence that source-specific pollution shocks broadly increase unrelated political communication. The coefficients are small relative to baseline posting rates and statistically insignificant in most cases; the only marginally significant estimates are negative domestic-shock effects for election and campaign posting. This pattern supports the interpretation that the main

results are not simply picking up broad shifts in Facebook activity on polluted days.

The second exercise considers a slower-moving institutional outcome: environmental bill introduction. Appendix Table A9 reports reduced-form estimates using proposer bill outcomes. Here the estimates are mostly null and generally imprecise. That is not surprising. Legislative activity is much sparser than Facebook communication, and bill introduction is a much more institutionally constrained response to short-run pollution conditions. The bill results therefore do not provide a strong independent test of the paper's mechanism, but they are still useful in delimiting it. The clearest political response to pollution in these data is visible in elite communication rather than in formal legislative production.

5.7 Additional Evidence: Next-Election Survival

Appendix Table A10 examines whether pre-election exposure and communication patterns predict incumbents' survival to the next Assembly election. The baseline sample contains 383 incumbents with complete 180-day pre-election windows, corresponding to legislators facing the 2016 and 2020 elections. We consider two outcomes: whether the incumbent later wins any district seat and whether the incumbent wins again in the same constituency.

The results are suggestive and should be interpreted cautiously. Average pre-election transboundary and domestic shock exposure are small and statistically indistinguishable from zero across specifications. Environmental posting is similarly uninformative, while fine-dust posting is weakly positive but imprecisely estimated. The most notable pattern instead concerns attribution behavior. Incumbents whose pre-election fine-dust posts contain a higher share of China blame are less likely to survive to the next election. In the richest specification, a one-standard-deviation increase in the China-blame share is associated with a 5.0 percentage point lower probability of winning any district seat again ($p = 0.003$) and a 4.0 percentage point lower probability of winning again in the same constituency ($p = 0.006$). The interaction between domestic shock exposure and blame share is also negative for same-constituency survival, though only marginally significant.

Appendix Table A11 suggests that this pattern is not uniform across blocs. In split-sample specifications, the negative association between pre-election China blaming and next-election survival is concentrated among democratic incumbents. In the richest specification, a one-standard-deviation increase in the China-blame share is associated with an 8.5 percentage point lower probability that a democratic incumbent wins any district seat again ($p < 0.001$) and a 7.2 percentage point lower probability of winning again in the same constituency ($p < 0.001$). The corresponding conservative coefficients are much smaller, at 2.4 and 1.5

percentage points, and statistically indistinguishable from zero. That pattern is consistent with the idea that conservative politicians are not obviously rewarded for China blame, even if they may be less harshly penalized for it than democratic politicians. At the same time, the formal pooled interaction remains imprecise, so these heterogeneity estimates should be treated as exploratory evidence rather than as a sharp electoral test of the paper's mechanism.

This exercise does not establish a clean causal effect of blame shifting on electoral punishment. The design is lower-frequency than the main politician-day analysis, omits politician and date fixed effects, and cannot distinguish incumbents who ran and lost from incumbents who did not run again. Even so, the results are useful in delimiting the paper's interpretation. The downstream electoral evidence does not suggest that China-blaming communication is rewarded in any simple way; if anything, it points in the opposite direction.

Taken together, the robustness and auxiliary evidence sharpen the interpretation of the main results. The attribution findings do not hinge on a single text measure, but neither do they generalize to every adjacent political outcome. Instead, the most credible pattern is that while average strategic foreign attribution is limited, conditional attribution responses are concentrated among conservatives, who increase foreign attribution in response to both transboundary and domestic shocks.

6 Conclusion

This paper studies how transboundary pollution enters domestic politics. Using high-frequency pollution data, source-specific transport shocks, and a politician-day panel of Facebook posts by South Korean legislators, we distinguish between two margins of political response. The first is a broad attention margin: whether pollution raises politicians' engagement with environmental issues. The second is an attribution margin: whether politicians explicitly blame China for fine dust, including on days when the underlying pollution shock is domestic rather than foreign.

Three findings emerge. First, instrumented local $PM_{2.5}$ does not provide evidence that pollution raises politicians' broad environmental Facebook posting. Second, we do not find evidence that the average politician systematically responds to higher pollution by blaming China across the full politician-day panel. Explicit foreign attribution is far rarer than general environmental discussion, but the conditional estimates indicate that source-specific shocks can increase China blame once politicians are already discussing fine dust, especially in the same-day specification. Third, the average full-panel result conceals meaningful par-

tisan heterogeneity. Conditional on discussing fine dust, conservative politicians increase foreign attribution in response to both transboundary and domestic shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. The domestic-shock response is especially consistent with strategic foreign attribution because it appears even when the measured shock is domestic.

These results speak to a broader question about the politics of international externalities. Cross-border environmental problems do not merely generate material harm; they also create opportunities for political actors to frame responsibility. Yet the paper shows that such framing is not automatic. Foreign blame is narrow, selective, and concentrated among politicians with stronger incentives to externalize responsibility. In that sense, the paper suggests that the political consequences of transboundary pollution depend not only on whether environmental conditions become salient, but also on how those conditions are narrated and assigned.

An exploratory next-election survival extension is consistent with that interpretation: politicians whose pre-election fine-dust posts contain more China blame do not appear to be rewarded electorally in any simple way, although that lower-frequency design remains necessarily tentative.

The paper also contributes methodologically. Much of the public discussion of fine dust is ambiguous about whether a mention of China is merely descriptive or genuinely causal. By combining pollution data with a text-based measure of explicit blame, the paper separates general issue attention from strategic attribution. That distinction is essential for identifying when foreign blame reflects actual international spillovers and when it instead reflects domestic political incentives.

Several limitations remain. The analysis is strongest for political communication and less informative for slower-moving institutional outcomes such as bill introduction. The exclusion restriction is also more demanding for the domestic instrument than for the transboundary instrument, because politicians may respond to broader regional domestic pollution episodes or shared media coverage rather than only to pollution realized within their own constituencies. Because the setting is a single country, caution is warranted in extrapolating the exact magnitude of the effects to other political systems. At the same time, these limitations point directly to future work. Similar designs could be used to study transboundary environmental politics in other countries, other pollutants, and other media environments, or to examine when foreign attribution affects public opinion rather than elite communication alone.

The broader implication is straightforward. When environmental harms cross borders, po-

litical narratives may do so as well. Understanding the domestic politics of transboundary pollution therefore requires not only measuring the physical movement of pollutants, but also tracing how political actors transform environmental shocks into claims about responsibility. This paper shows that those transformations are neither uniform nor apolitical. They are shaped by incentives, by institutions, and by the strategic uses of blame.

References

Anderson, Soren, Ioana Marinescu, and Boris Shor. 2023. "Can Pigou at the Polls Stop Us Melting the Poles?" *Journal of the Association of Environmental and Resource Economists*, 10(4): 903–945.

Bechtel, Michael M., and Jens Hainmueller. 2011. "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy." *American Journal of Political Science*, 55(4): 852–868.

Boomhower, Judson. 2024. "When Do Environmental Externalities Have Electoral Consequences? Evidence from Fracking." *Journal of the Association of Environmental and Resource Economists*, 11(4): 999–1029.

Buntaine, Mark T., Michael Greenstone, Guojun He, Mengdi Liu, Shaoda Wang, and Bing Zhang. 2024. "Does the Squeaky Wheel Get More Grease? The Direct and Indirect Effects of Citizen Participation on Environmental Governance in China." *American Economic Review*, 114(3): 815–850.

Cole, Shawn, Andrew Healy, and Eric Werker. 2012. "Do Voters Demand Responsive Governments? Evidence from Indian Disaster Relief." *Journal of Development Economics*, 97(2): 167–181.

Fredriksson, Per G., and Daniel L. Millimet. 2002. "Strategic Interaction and the Determination of Environmental Policy across US States." *Journal of Urban Economics*, 51(1): 101–122.

Gasper, John T., and Andrew Reeves. 2011. "Make It Rain? Retrospection and the Attentive Electorate in the Context of Natural Disasters." *American Journal of Political Science*, 55(2): 340–355.

Hazlett, Chad, and Matto Mildenberger. 2020. "Wildfire Exposure Increases Pro-Environment Voting within Democratic but not Republican Areas." *American Political Science Review*, 114(4): 1359–1365.

Healy, Andrew, and Neil Malhotra. 2009. "Myopic Voters and Natural Disaster Policy." *American Political Science Review*, 103(3): 387–406.

Helland, Eric, and Andrew B. Whitford. 2003. "Pollution Incidence and Political Jurisdiction: Evidence from the TRI." *Journal of Environmental Economics and Management*, 46(3): 403–424.

Heo, Seonmin Will, Koichiro Ito, and Rao Kotamarthi. forthcoming. "International Spillover Effects of Air Pollution: Evidence from Mortality and Health Data." *The Review of Economics and Statistics*.

Kahn, Matthew E., Pei Li, and Daxuan Zhao. 2015. "Water Pollution Progress at Borders: The Role of Changes in China's Political Promotion Incentives." *American Economic Journal: Economic Policy*, 7(4): 223–242.

Konisky, David M., and Neal D. Woods. 2010. "Exporting Air Pollution? Regulatory Enforcement and Environmental Free Riding in the United States." *Political Research Quarterly*, 63(4): 771–782.

Lee, Haillie, and Erik Voeten. 2026. "Transboundary Air Pollution and Hazy Accountability: Evidence from South Korea." *International Organization*, 80(1): 152–178.

Lipscomb, Molly, and Ahmed Mushfiq Mobarak. 2016. "Decentralization and Pollution Spillovers: Evidence from the Re-Drawing of County Borders in Brazil." *The Review of Economic Studies*, 84(1): 464–502.

Sheldon, Tamara L., and Chandini Sankaran. 2017. "The Impact of Indonesian Forest Fires on Singaporean Pollution and Health." *American Economic Review*, 107(5): 526–529.

Sigman, Hilary. 2002. "International Spillovers and Water Quality in Rivers: Do Countries Free Ride?" *American Economic Review*, 92(4): 1152–1159.

Sigman, Hilary. 2005. "Transboundary Spillovers and Decentralization of Environmental Policies." *Journal of Environmental Economics and Management*, 50(1): 82–101.

Song, Esther E. 2023. "Air Pollution Coverage, Anti-Chinese Sentiment, and Attitudes Towards Foreign Policy in South Korea." *Journal of Chinese Political Science*, 28: 571–592.

Stein, Ariel F., Roland R. Draxler, Glenn D. Rolph, Barbara J. B. Stunder, Mark D. Cohen, and Fong Ngan. 2015. "NOAA's HYSPLIT Atmospheric Transport and Dispersion Modeling System." *Bulletin of the American Meteorological Society*, 96(12): 2059–2077.

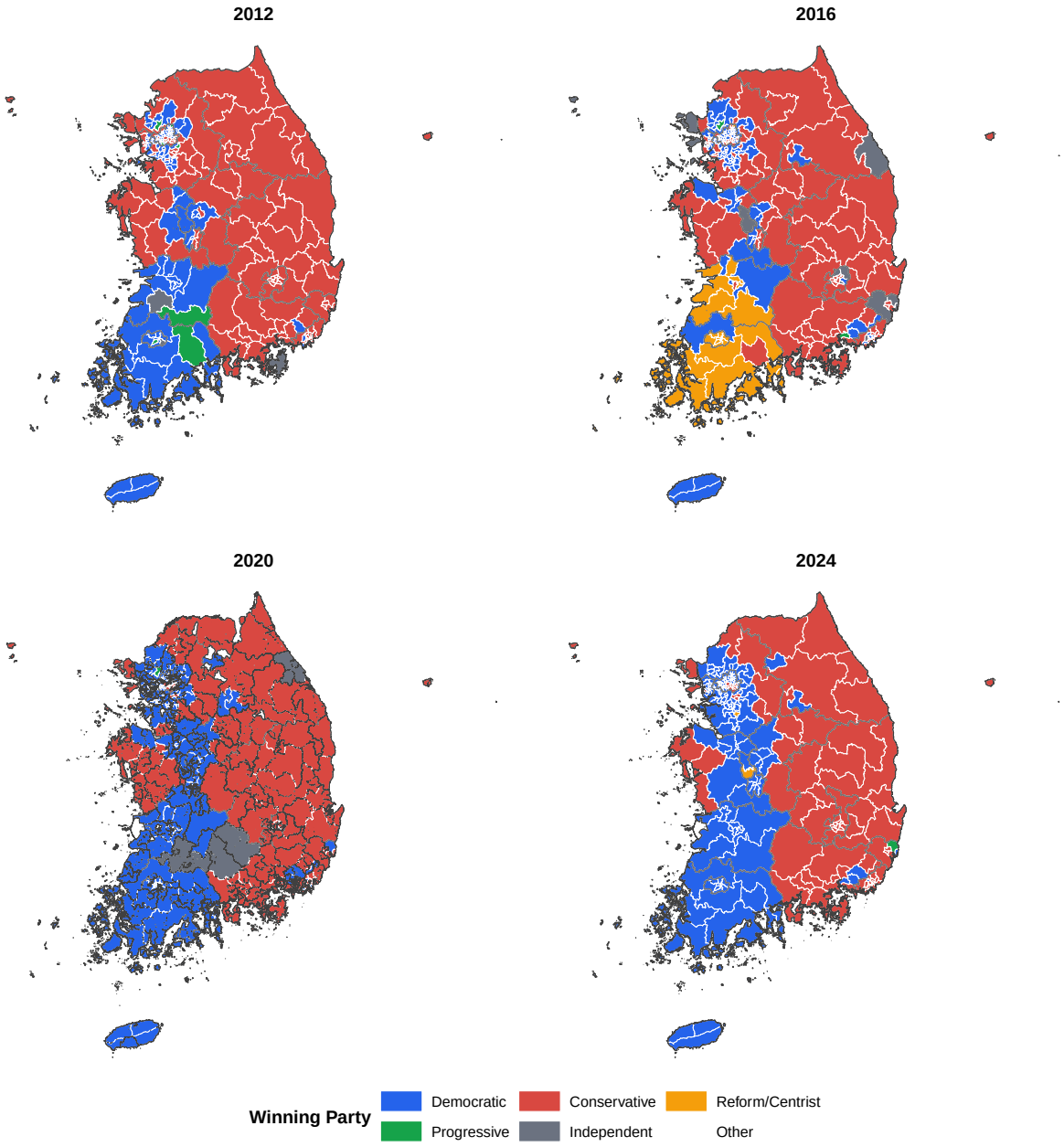
Wang, Shaoda, and Zenan Wang. 2020. "The Environmental and Economic Consequences of Internalizing Border Spillovers." University of Chicago: Chicago, IL, USA.

Yao, Yao, Xue Li, Russell Smyth, and Lin Zhang. 2022. "Air Pollution and Political Trust in Local Government: Evidence from China." *Journal of Environmental Economics and Management*, 115: 102724.

Zheng, Siqi, Jianghao Wang, Cong Sun, Xiaonan Zhang, and Matthew E. Kahn. 2019. "Air Pollution Lowers Chinese Urbanites' Expressed Happiness on Social Media." *Nature Human Behaviour*, 3(3): 237–243.

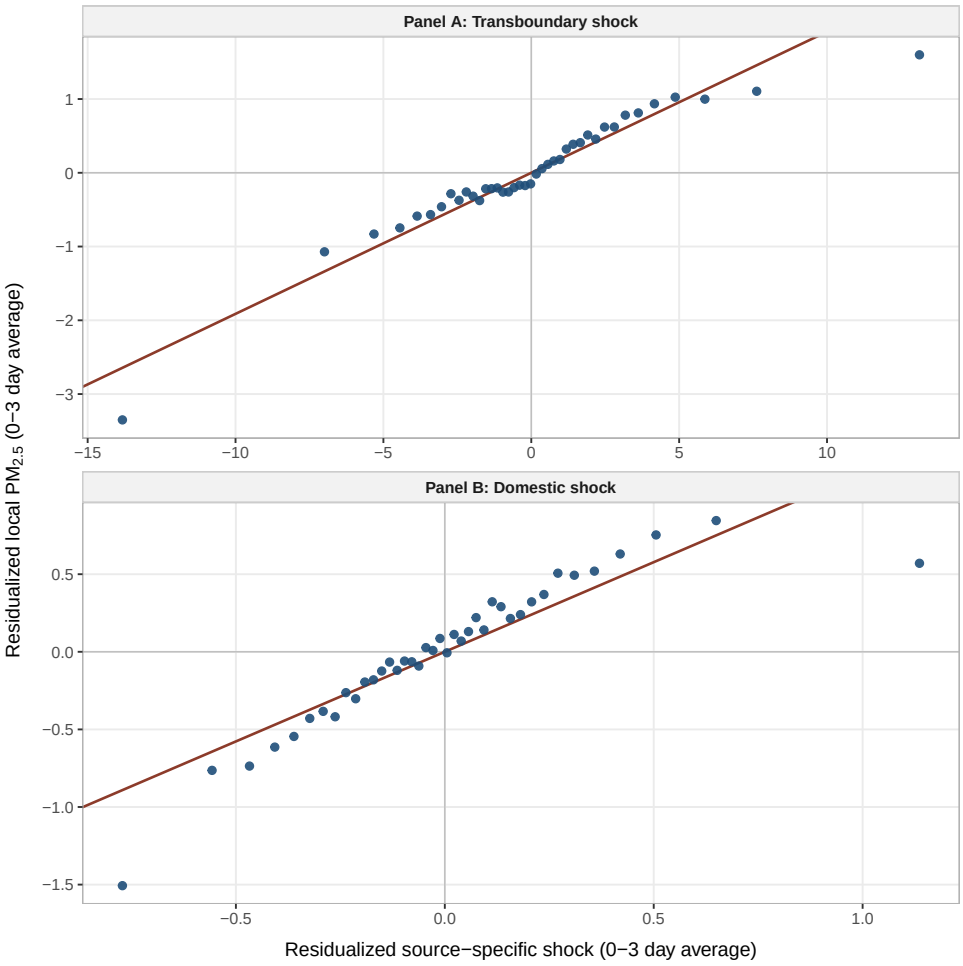
Main Figures

Figure 1: District Winners by Party Family, 2012–2024



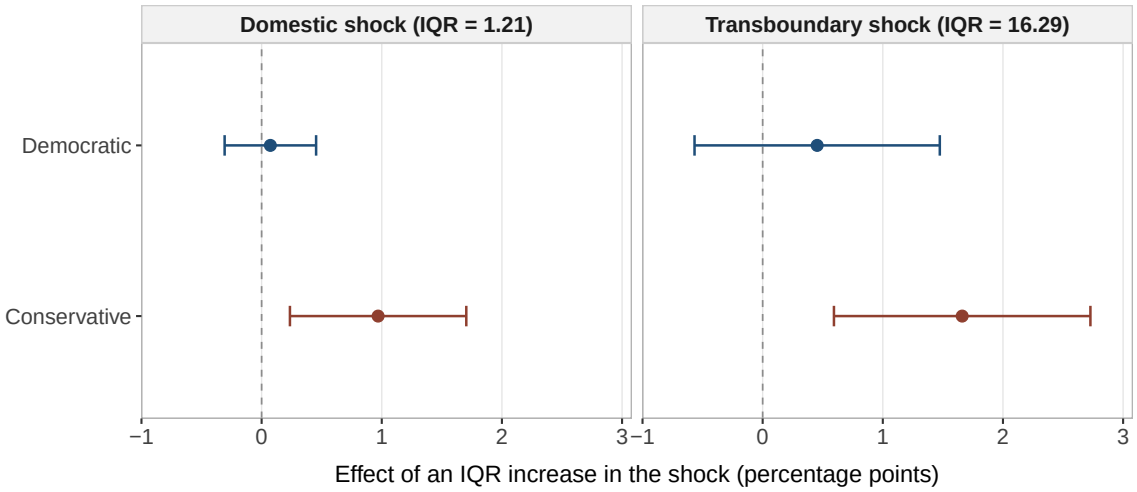
Notes: Each panel maps South Korea's district-seat National Assembly constituencies in the 2012, 2016, 2020, and 2024 elections. Colors group election winners into stable party families across years: Democratic, Conservative, Reform/Centrist, Progressive, and Independent. Proportional-representation seats are excluded.

Figure 2: Residualized First-Stage Relationships in the Preferred IV Specification



Notes: This figure visualizes the first-stage relationships in the preferred 0–3 day-average IV specification with wind speed, precipitation, and temperature controls. Each panel bins the partial relationship between local $PM_{2.5}$ and one source-specific shock after partialling out politician fixed effects, calendar-date fixed effects, weather controls, and the other source-specific shock. Because the date fixed effects absorb national shocks common to all politicians on a given day, the identifying variation is within-date cross-politician variation in exposure. The plotted points are 40 quantile bins, and the fitted line is the corresponding linear partial relationship in the estimation sample used for Table 2, columns (3)–(4).

Figure 3: Estimated Party-Specific Daily Effects on China Attribution, Conditional on Fine-Dust Posting



Notes: This figure visualizes the daily conditional estimates from Panel B of Table 4. The sample is restricted to politician-days with at least one classifier-identified fine-dust post, so the outcome captures attribution behavior once politicians are already discussing pollution. Table 4 reports coefficients on the original 10-unit scale; this figure mechanically rescales the same estimates and confidence intervals to show percentage-point changes in the probability of any China attribution for an interquartile-range increase in each source-specific shock. The panel titles report the interquartile ranges used for the rescaling. This scaling is useful because the transboundary shock is measured in China-trajectory $PM_{2.5}$ units, while the domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed. The main pattern is partisan heterogeneity: conservatives increase foreign attribution for both source-specific shocks, whereas Democratic estimates are small and statistically indistinguishable from zero. The domestic-shock response is especially relevant to strategic foreign attribution because it appears even when the measured shock is domestic. Horizontal lines denote 95 percent confidence intervals.

Main Tables

Table 1: Summary Statistics: Main Estimation Samples and News Corpus

	Mean	Std. Dev.	Minimum	Maximum	Observations
A. Politician Sample					
District politicians in main IV sample	655				
Age at election	55.19	6.54	32	73	655
Elections won	2.913	1.385	1	8	655
Female politicians	67				
Democratic politicians	343				
Conservative politicians	265				
B. Main Politician-Day Sample					
Local $PM_{2.5}$	22.08	14.26	0	155	714,321
Transboundary shock	12.30	17.92	0	262	714,321
Domestic shock	0.91	0.92	0	10	714,321
Any environmental post	0.028	0.166	0	1	714,321
Number of environmental posts	0.031	0.190	0	8	714,321
C. Attribution Sample					
Politicians in attribution sample	567				
Politicians in partisan attribution sample	523				
Fine-dust posts per day	1.062	0.288	1	7	12,222
China-blaming posts per day	0.012	0.111	0	2	12,222
Any China-blaming post	0.012	0.108	0	1	12,222
China-blame share	0.011	0.104	0	1	12,222
Transboundary shock	11.74	17.82	0	207	12,222
Domestic shock	0.94	0.96	0	10	12,222
D. Naver News Corpus					
Clean Naver news articles	26,728				
Press outlets represented	1,104				
Clean articles per day	5.06	6.51	1	151	5,287
Fine-dust-China articles per day	4.07	5.20	0	139	5,287
Explicit China-blame articles per day	0.91	2.06	0	69	5,287
Share of usable body text	0.828	0.232	0	1	5,287
China-blame share among fine-dust-China articles	0.181	0.279	0	1	5,101

Notes: This table focuses on the paper's main empirical objects rather than the full raw data inventory. Panel A summarizes the district politicians who enter the main IV sample. Age is measured at the election date corresponding to each legislator's cohort. Panel B summarizes the main politician-day sample used in the IV attention regressions; it is restricted to observations with non-missing local $PM_{2.5}$, source-specific shocks, and the full set of weather controls used in the preferred specification. Panel C summarizes the attribution sample used in the weak-classifier analysis, conditional on the politician making at least one fine-dust post on that day. The partisan attribution sample further restricts Panel C to politicians classified as democratic or conservative. Panel D summarizes the cleaned Naver news corpus used for phrase discovery, validation, and media-framing analysis. The final row in Panel D is defined only for days with at least one fine-dust-China article.

Table 2: Instrumental Variables Estimates: Local Pollution and Environmental Attention

	Same-day		0–3 day average	
	Any environmental post	Number of environmental posts	Any environmental post	Number of environmental posts
Instrumented local $PM_{2.5}$	-0.430*	-0.363	-0.555**	-0.515
	(0.231)	(0.274)	(0.266)	(0.314)
Weather controls	Yes	Yes	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
First-stage F	52.73	52.73	77.12	77.12
Mean dep. var.	2.82	3.08	2.80	3.07
Politicians	655	655	655	655
Dates	3,286	3,286	3,271	3,271
N	714,321	714,321	697,393	697,393

Notes: This table reports two-stage least squares estimates of the effect of local $PM_{2.5}$ on politicians' environmental Facebook posting. Local $PM_{2.5}$ is instrumented with the source-specific transboundary and domestic shocks. The transboundary shock combines China-crossing backward trajectories with Chinese monitor $PM_{2.5}$; the domestic shock equals an indicator that the backward trajectory does not reach China interacted with inverse local wind speed. Coefficients are reported for a 10 $\mu g/m^3$ increase in local $PM_{2.5}$. All dependent variables are multiplied by 100, so columns (1) and (3) are percentage-point effects and columns (2) and (4) are changes in the number of posts per 100 politician-days. The preferred specification uses the 0–3 day average exposure window and controls for local wind speed, temperature, and precipitation. All specifications include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. Means are reported in the same scaled units as the dependent variables. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Average Effects on China-Blaming Communication

	Panel A: Unconditional Margin		Panel B: Conditional on Fine-Dust Posting	
	Same-day	0–3 day average	Same-day	0–3 day average
Transboundary shock	0.008*	0.003	0.629*	0.775
	(0.005)	(0.008)	(0.332)	(0.550)
Domestic shock	0.043	0.023	3.383**	5.625**
	(0.027)	(0.043)	(1.602)	(2.861)
Conditional on fine-dust posting	No	No	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Mean dep. var.	0.020	0.020	1.193	1.212
Politicians	655	655	489	488
N	720,103	703,882	11,563	11,305

Notes: This table reports reduced-form estimates of source-specific shocks on explicit China-blaming communication in politicians' Facebook posts. In both panels, the outcome is an indicator for whether the politician made any classifier-identified China-blaming post on that day. Panel A uses the full politician-day panel and therefore measures the overall amount of visible China-blaming communication. Panel B restricts the sample to politician-days with at least one classifier-identified fine-dust post and therefore measures attribution behavior once politicians have already entered the fine-dust conversation. Coefficients are reported for a 10-unit increase in the relevant source-specific shock, and the coefficients and mean dependent variables are reported in percentage points. The transboundary shock is measured in China-trajectory $PM_{2.5}$ units, while the domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed. Because the domestic shock is not in $PM_{2.5}$ units, the main text also interprets domestic-shock magnitudes using interquartile-range changes shown in Appendix Figure A7. All specifications include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Partisan Heterogeneity in China Attribution

	Panel A: Unconditional Margin		Panel B: Conditional on Fine-Dust Posting	
	Same-day	0–3 day average	Same-day	0–3 day average
Democrat: transboundary shock	0.001	-0.002	0.278	0.659
	(0.004)	(0.009)	(0.320)	(0.588)
Democrat: domestic shock	0.000	-0.038	0.602	2.066
	(0.024)	(0.046)	(1.607)	(2.893)
Conservative diff. × transboundary shock	0.012*	0.019*	0.741*	1.243**
	(0.007)	(0.011)	(0.401)	(0.555)
Conservative diff. × domestic shock	0.095	0.115	7.422**	12.610***
	(0.061)	(0.103)	(3.356)	(4.787)
Conservative: total transboundary effect	0.013**	0.018*	1.019***	1.902***
	(0.006)	(0.009)	(0.334)	(0.638)
Conservative: total domestic effect	0.096*	0.077	8.024***	14.676***
	(0.056)	(0.087)	(3.099)	(4.678)
Conditional on fine-dust posting	No	No	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Mean dep. var.	0.017	0.018	1.047	1.062
Politicians	608	608	446	445
N	659,107	643,618	10,126	9,885

Notes: This table reports partisan heterogeneity in explicit China attribution. Party labels are assigned at the politician-cohort level using the observed cohort-specific party affiliation and a party-to-position crosswalk. "Democrat" denotes the baseline effect for politicians coded as Center-left. "Conservative diff." is the interaction between the relevant shock and an indicator for politicians coded as Center-right or Right-wing. "Conservative: total effect" is the sum of the baseline and differential coefficients. Politicians coded as Left-wing, Centrist, or unaffiliated are excluded from these regressions. Panel A uses the full politician-day panel, while Panel B restricts the sample to politician-days with at least one classifier-identified fine-dust post. Coefficients are reported for a 10-unit increase in the relevant source-specific shock, and the coefficients and mean dependent variables are reported in percentage points. The transboundary shock is measured in China-trajectory $PM_{2.5}$ units, while the domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed. Because the domestic shock is not in $PM_{2.5}$ units, the main text also interprets domestic-shock magnitudes using interquartile-range changes shown in Appendix Figure A7. In the underlying unconditional panel, conservatives make China-blaming posts on 0.027 percent of politician-days, compared with 0.010 percent for democrats. All specifications include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

A Source-Specific Pollution Measures

A.1 HYSPLIT Back-Trajectory Construction

The transboundary shock is constructed from hourly backward trajectories generated with the NOAA HYSPLIT model (Stein et al., 2015). For each hour and each Korean city receptor, we initialize a trajectory at the city centroid with an arrival height of 500 meters and trace the air mass backward for 200 hours. The simulations use HYSPLIT-format monthly reanalysis meteorological files for the previous, current, and following months, with the model top set to 10,000 meters. The raw HYSPLIT trajectory output is then converted into an endpoint panel containing the receptor city, arrival date-hour, endpoint date-hour, hours along the backward trajectory, latitude, longitude, height, and pressure.

We classify trajectory endpoints geographically using a China administrative boundary shapefile. After removing duplicate endpoint records, we split the trajectory points into those inside and outside China. The pollution signal uses only China-crossing endpoints below 1,000 meters, because low-altitude transport is more relevant for surface particulate exposure at the receptor. For each qualifying endpoint, we identify the nearest Chinese $PM_{2.5}$ monitor and merge the contemporaneous monitor reading for the endpoint's date and hour. The hourly transboundary signal for Korean city c at arrival hour h, denoted Z^F_{ch}, is the average matched Chinese $PM_{2.5}$ value across the qualifying low-altitude China endpoints on that backward trajectory. If the trajectory does not pass over China at qualifying altitude, the China-trajectory signal is set to zero.

This construction produces a transport-based exposure proxy rather than a direct measure of foreign emissions. A high value of Z^F_{ch} means that the air mass arriving in city c recently passed over polluted Chinese locations at low altitude. A value of zero means that the backward trajectory did not contain a qualifying China segment. The measure is therefore designed to capture plausibly exogenous variation in the delivery of China-origin pollution to Korean receptors, not to solve a full chemical transport or emissions-inventory source-apportionment model.

A.2 Domestic Stagnation Instrument

The domestic shock is constructed from the backward-trajectory classification and local wind speed. For city c and arrival hour h, let $\mathbf{1}\{\text{No China}_{ch}\}$ indicate that the low-altitude backward trajectory contains no qualifying China segment. The city-hour domestic signal is

$$Z_{ch}^{D} = \mathbf{1}\{\text{No China}_{ch}\} \times \frac{1}{\text{WindSpeed}_{ch}}, \tag{17}$$

where wind speed is measured in meters per second. Observations with missing or nonpositive wind speed are treated as missing for this variable.

The domestic signal is intended to capture low-wind conditions under which locally generated pollution can accumulate, while restricting that variation to hours when the arriving air mass is not classified as transboundary from China. As with the transboundary signal, the domestic measure is a source-specific shock used for identification and interpretation rather than a direct observation of emissions. Because its units combine a trajectory indicator with inverse wind speed, the substantive discussion interprets domestic-shock magnitudes using empirical movements in the politician-day distribution rather than treating one raw unit as a natural pollution increment.

A.3 Aggregation to Politician-Day Exposure

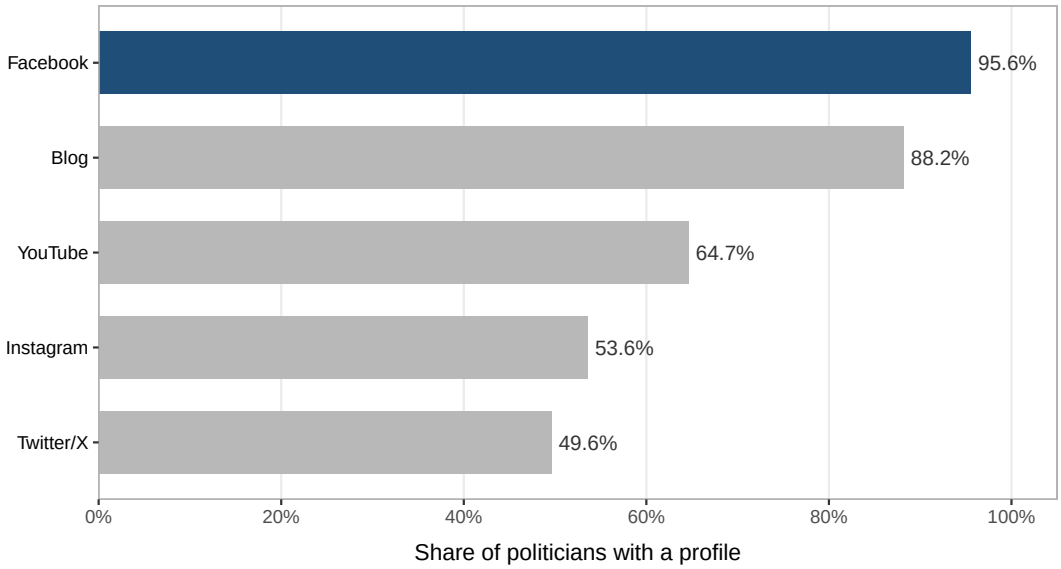
Both source-specific hourly signals are first collapsed to city-day measures by averaging over the hours in each city-date. Let Z_{ct}^{s} denote the resulting daily city-level shock for source $s \in \{F, D\}$. We then map cities to National Assembly constituencies using the constituency-city crosswalk described in Section 2. If politician i's constituency overlaps city c with area share ω_{ic}, the politician-day exposure is

$$Z_{it}^{s} = \sum_{c \in \mathcal{C}(i)} \omega_{ic} Z_{ct}^{s}, \qquad s \in \{F, D\}. \tag{18}$$

The same city-share weighting is used to aggregate local $PM_{2.5}$ and weather controls to the politician-day level. The resulting politician-day transboundary and domestic shocks are the excluded instruments in the main IV attention regressions and the source-specific regressors in the attribution reduced-form specifications.

B Additional Figures

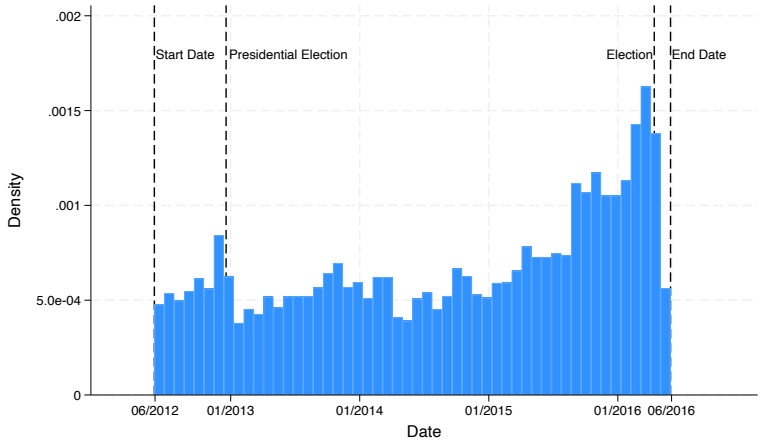
Figure A1: Social-Media Profile Availability among Politicians



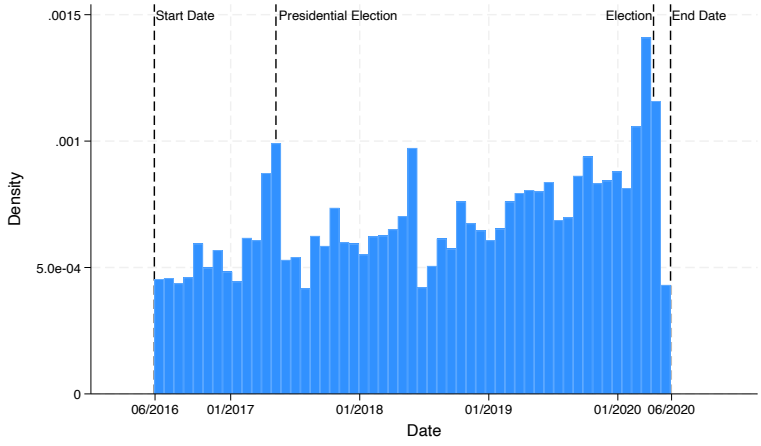
Notes: This figure reports the share of unique politicians who served in the 19th, 20th, or 21st National Assembly and have a nonmissing profile link in each platform field of the politician profile dataset. The sample contains 679 unique politicians. A profile is coded as available when the corresponding platform field is nonmissing and nonblank; the figure does not measure posting frequency or account activity. Twitter/X refers to the Twitter field in the source data. Homepages are omitted because the figure focuses on social-media and blog platforms.

Figure A2: Distribution of Facebook Posting Dates by Legislators

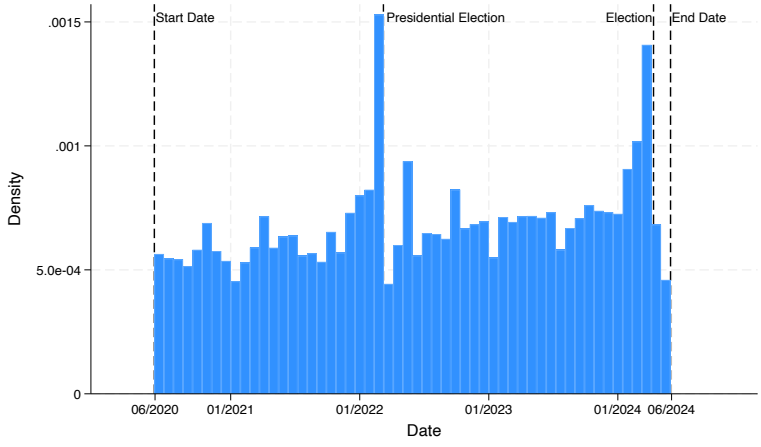
Panel A: The 19th Cohort



Panel B: The 20th Cohort

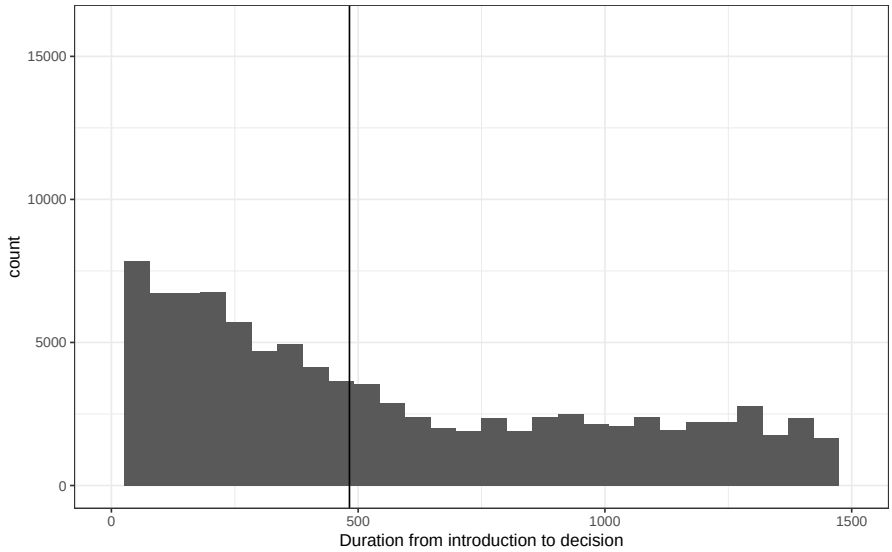


Panel C: The 21st Cohort



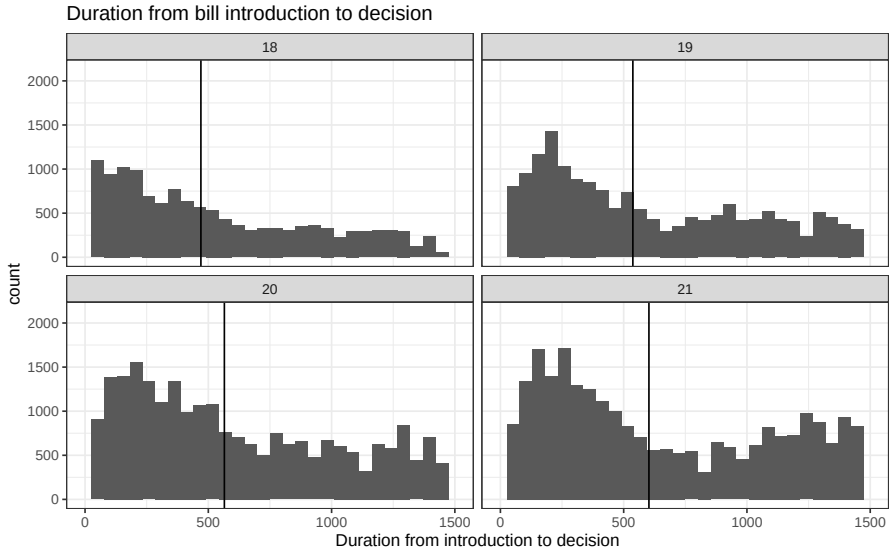
Notes: Panels A, B, and C show the distribution of Facebook posting dates for district-seat legislators in the 19th, 20th, and 21st National Assembly cohorts.

Figure A3: Average Bill Decision Time



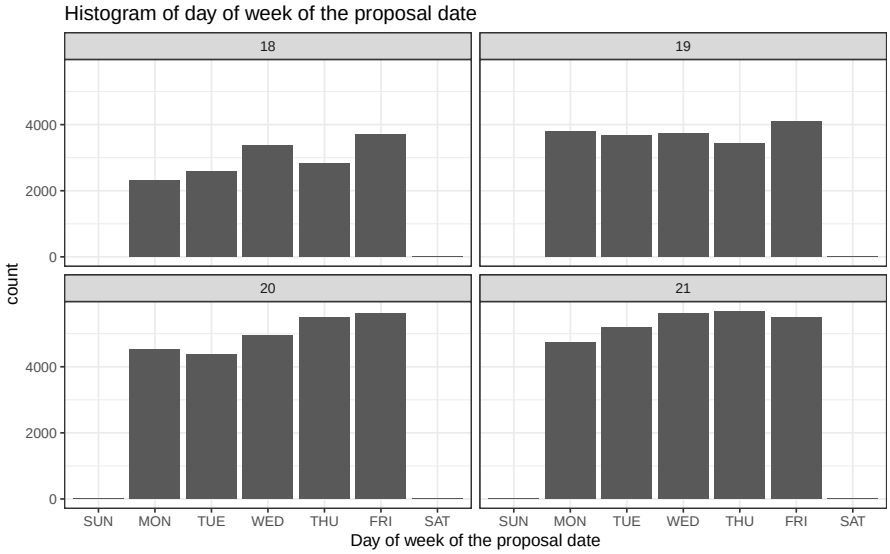
Notes: This histogram plots the number of days between bill introduction and final recorded decision for bills with nonmissing proposal and process dates. The vertical line marks the sample mean. The main pattern to notice is that legislative processing is slow and highly dispersed: many bills take well over a year to reach a decision, with a long right tail extending far beyond the mean.

Figure A4: Timeline from Bill Introduction to Decision by Cohort



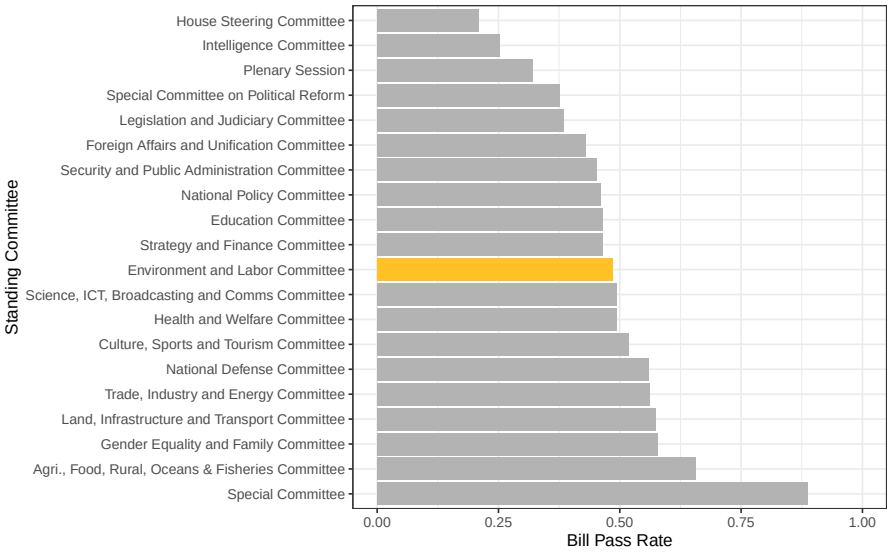
Notes: Each panel shows the distribution of bill decision times separately for the 18th through 21st National Assembly cohorts; the vertical line in each panel marks that cohort's mean. The figure is useful for seeing that long processing times are not confined to a single legislature. If anything, later cohorts appear to have somewhat longer average decision times, reinforcing the idea that bill production is a slow-moving institutional outcome.

Figure A5: Timing of Bill Proposal



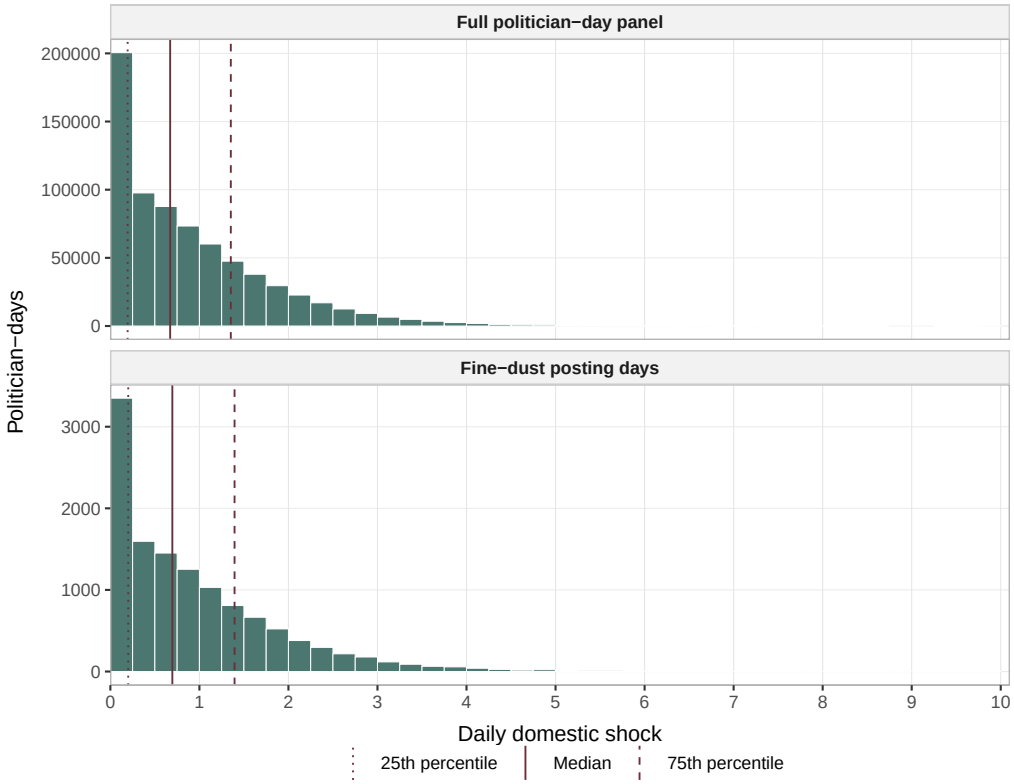
Notes: This figure reports the day-of-week distribution of bill proposal dates by Assembly cohort. Proposals are overwhelmingly introduced on weekdays, with almost no activity on weekends, and there is a modest tendency for introductions to cluster later in the work week. The key takeaway is that bill introduction follows an institutional calendar rather than responding flexibly on a day-to-day basis.

Figure A6: Bill Pass Rate by Committee



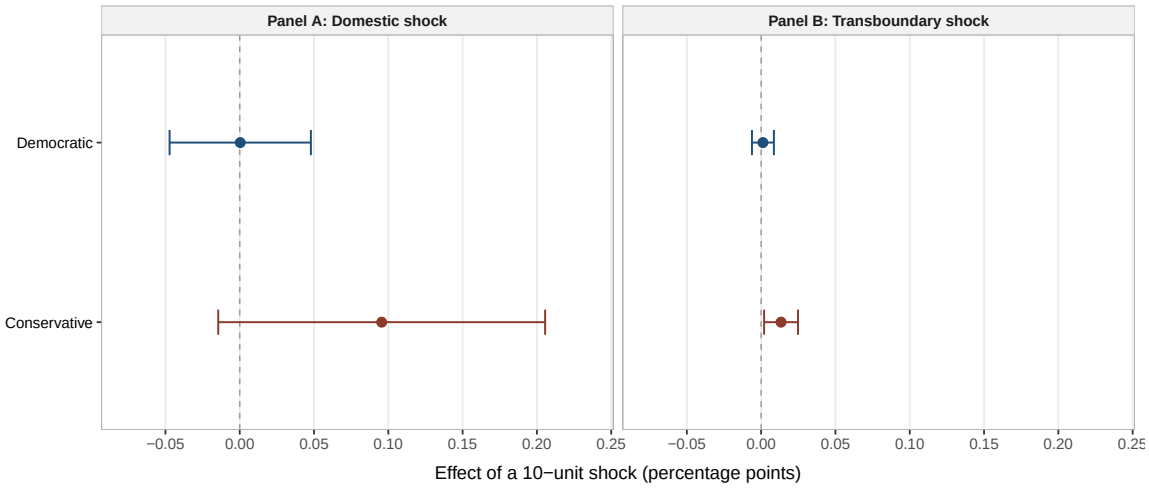
Notes: This figure plots bill pass rates across current standing committees. The Environment and Labor Committee is highlighted in gold. The main feature is substantial cross-committee variation in passage rates, while the Environment and Labor Committee sits near the middle of the distribution rather than standing out as an unusually high- or low-throughput venue.

Figure A7: Distribution of the Daily Domestic Shock



Notes: This figure plots the politician-day distribution of the daily domestic shock, defined as non-China trajectory exposure interacted with inverse local wind speed. The measure proxies for domestic pollution conditions by capturing low-wind accumulation when the arriving air mass is not classified as recently passing over China. The top panel uses the full daily attribution panel; the bottom panel restricts to politician-days with at least one classifier-identified fine-dust post. Vertical lines mark the 25th percentile, median, and 75th percentile in each sample. In the full panel, the 25th percentile, median, and 75th percentile are 0.19, 0.67, and 1.35. In the fine-dust posting sample, they are 0.20, 0.70, and 1.39.

Figure A8: Estimated Party-Specific Daily Effects on China Attribution, Unconditional Margin



Notes: This appendix figure visualizes the daily unconditional estimates from Panel A of Table 4. The outcome is whether a politician makes any explicit China-blaming post on a given day in the full politician-day panel. Effects are shown on the table's 10-unit shock scale; Appendix Figure A7 reports the empirical distribution used to interpret domestic-shock magnitudes. Because explicit attribution is rare on the full sample, the effects are much smaller in absolute magnitude than in Figure 3, although the same substantive asymmetry remains most visible for the domestic shock. Horizontal lines denote 95 percent confidence intervals.

C Additional Tables

Table A1: First-Stage Estimates for the Main Instrumental Variables Specification

	Same-day	0–3 day average
Transboundary shock	1.160***	1.913***
	(0.138)	(0.172)
Domestic shock	6.767***	11.550***
	(0.768)	(1.154)
First-stage F	52.73	77.12
Weather controls	Yes	Yes
Politician fixed effects	Yes	Yes
Date fixed effects	Yes	Yes
Politicians	655	655
Dates	3,286	3,271
N	714,321	697,393

Notes: This table reports the first-stage relationship between the excluded instruments and local $PM_{2.5}$ in the main IV specification. The dependent variable is same-day local $PM_{2.5}$ in column (1) and the 0–3 day average of local $PM_{2.5}$ in column (2). Coefficients are reported as changes in local $PM_{2.5}$ (in $\mu g/m^3$) for a 10-unit increase in the relevant source-specific shock. The transboundary shock is measured in China-trajectory $PM_{2.5}$ units, while the domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed. Appendix Figure A7 reports the empirical distribution of the daily domestic shock. The specification includes local wind speed, temperature, and precipitation, along with politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: OLS and IV Estimates for Broad Environmental Attention: Any Environmental Post

	Same-day		0–3 day average	
	OLS	IV	OLS	IV
Local $PM_{2.5}$	0.041	-0.430*	0.079	-0.555**
	(0.039)	(0.231)	(0.059)	(0.266)
Weather controls	Yes	Yes	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
First-stage F		52.73		77.12
Mean dep. var.	2.82	2.82	2.80	2.80
Politicians	655	655	655	655
Dates	3,286	3,286	3,271	3,271
N	714,321	714,321	697,393	697,393

Notes: This table compares OLS and instrumental variables estimates for the extensive-margin broad environmental attention outcome. The dependent variable is an indicator for whether the politician made any environmental Facebook post on a given day, multiplied by 100. OLS columns regress the outcome on observed local $PM_{2.5}$. IV columns instrument local $PM_{2.5}$ with the source-specific transboundary and domestic shocks. Coefficients are reported for a 10 $\mu g/m^3$ increase in local $PM_{2.5}$ and are therefore percentage-point effects. All specifications control for local wind speed, temperature, and precipitation and include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. First-stage F-statistics are reported only for the IV columns. Means are reported in the same scaled units as the dependent variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: OLS and IV Estimates for Broad Environmental Attention: Number of Environmental Posts

	Same-day		0–3 day average	
	OLS	IV	OLS	IV
Local $PM_{2.5}$	0.058	-0.363	0.101	-0.515
	(0.046)	(0.274)	(0.070)	(0.314)
Weather controls	Yes	Yes	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
First-stage F		52.73		77.12
Mean dep. var.	3.08	3.08	3.07	3.07
Politicians	655	655	655	655
Dates	3,286	3,286	3,271	3,271
N	714,321	714,321	697,393	697,393

Notes: This table compares OLS and instrumental variables estimates for the intensive-margin broad environmental attention outcome. The dependent variable is the number of environmental Facebook posts made by the politician on a given day, multiplied by 100. OLS columns regress the outcome on observed local $PM_{2.5}$. IV columns instrument local $PM_{2.5}$ with the source-specific transboundary and domestic shocks. Coefficients are reported for a 10 $\mu g/m^3$ increase in local $PM_{2.5}$ and are therefore changes in the number of environmental posts per 100 politician-days. All specifications control for local wind speed, temperature, and precipitation and include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. First-stage F-statistics are reported only for the IV columns. Means are reported in the same scaled units as the dependent variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Performance of the Weakly Supervised Text Classifier

	Fine-dust classifier	China-blame classifier
Classification threshold	0.5790	0.3825
ROC AUC	0.9941	0.9952
Average precision	0.9795	0.8606
F1 score	0.9436	0.8000
Precision	0.9631	0.7407
Recall	0.9248	0.8696
Evaluation observations	6,328	904
Positive observations	904	23

Notes: This table reports out-of-sample performance for the weakly supervised text classifier used to identify fine-dust posts and explicit China-blaming posts. The Korean news corpus is used to construct the attribution dictionary and seed phrases, while politician behavior is measured in the Facebook archive. The classifier is trained on silver labels derived from broad Facebook topic tags and the news-derived seed phrases, and then applied to the full Facebook archive. The fine-dust classifier is the first-stage topic screen; the China-blame classifier is estimated conditional on fine-dust relevance.

Table A5: Breadth of Explicit China Attribution in the Partisan Sample

	Overall	Conservative	Democratic
Politicians with any fine-dust posting day	523	220	303
Politicians with any China-attribution day	66	39	27
Share with any attribution day (%)	12.6	17.7	8.9
Fine-dust posting days	10,855	4,221	6,634
China-attribution days	114	75	39

Notes: This table summarizes the breadth of explicit China attribution in the daily partisan heterogeneity panel used in Table 4. The first row counts politicians with at least one day of classifier-identified fine-dust posting activity; the second row counts politicians with at least one day containing any classifier-identified China-blaming fine-dust post. The pattern is not driven by a single account: the most active politician accounts for 9 of 114 attribution days (7.9 percent), and the five most active politicians account for 29 attribution days (25.4 percent).

Table A6: Descriptive Engagement with China-Blaming Fine-Dust Facebook Posts

Sample	Post type	Posts	Politicians	Mean total engagement	Median total engagement
Overall	Other fine-dust posts	13,290	614	169.0	108
Overall	China-blaming fine-dust posts	136	76	134.9	107
Democratic	Other fine-dust posts	8,257	342	192.6	120
Democratic	China-blaming fine-dust posts	43	30	161.0	118
Conservative	Other fine-dust posts	5,033	272	130.4	96
Conservative	China-blaming fine-dust posts	93	46	122.8	105

Notes: This table compares descriptive engagement across classifier-identified fine-dust Facebook posts in the raw post archive. Total engagement equals likes plus comments plus shares. The sample is restricted to posts from politicians in the partisan sample with nonmissing engagement counts and post dates inside the relevant cohort term windows from 2015 through 2023. China-blaming fine-dust posts do not appear uniformly more popular than other fine-dust posts: mean total engagement is lower overall, while median engagement is very similar. At the component level, attribution posts also receive slightly fewer likes and comments on average, with shares modestly higher. These comparisons are descriptive and do not adjust for politician-specific audience size or posting-time composition.

Table A7: Alternative Attribution Measure: Dictionary-Based China Blame

	Same-day		0–3 day average	
	Any China-blaming post	China-blame share	Any China-blaming post	China-blame share
Transboundary shock	1.881	1.484	2.649	2.213
	(1.364)	(1.111)	(3.390)	(2.872)
Domestic shock	-0.446	-1.294	-2.185	-2.590
	(1.678)	(1.537)	(3.228)	(2.865)
Conditional on fine-dust posting	Yes	Yes	Yes	Yes
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Politicians	387	387	387	387
Dates	1,122	1,122	1,120	1,120
N	2,243	2,243	2,239	2,239

Notes: This table reports the attribution regressions using a conservative dictionary-based measure of explicit China blame rather than the weakly supervised classifier used in the main text. The dictionary measure is substantially sparser, which explains the much smaller sample size. Coefficients are reported for a 10-unit increase in the relevant source-specific shock. Outcomes are multiplied by 100, so all coefficients are in percentage points. The domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed, so the main text interprets its magnitudes using empirical changes in the daily shock distribution. The qualitative conclusion is unchanged: neither transboundary nor domestic shocks produce robust average increases in China-blaming behavior. Standard errors, reported in parentheses, are clustered two ways by politician and date. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Placebo Tests: Facebook Posts on Non-Environmental Topics

	Economy/labor	Education	Housing	Campaign
Panel A. Same-day shocks				
Transboundary shock	-0.086	-0.002	-0.021	-0.026
	(0.055)	(0.037)	(0.024)	(0.054)
Domestic shock	0.097	-0.034	-0.024	-0.209*
	(0.116)	(0.081)	(0.065)	(0.116)
Mean dep. var.	12.63	5.57	2.99	10.20
N	673,433	673,433	673,433	673,433
Panel B. 0–3 day average shocks				
Transboundary shock	-0.074	0.053	-0.029	-0.089
	(0.114)	(0.067)	(0.045)	(0.110)
Domestic shock	0.171	-0.024	-0.130	-0.386*
	(0.210)	(0.129)	(0.100)	(0.207)
Mean dep. var.	12.61	5.55	3.00	10.23
N	657,120	657,120	657,120	657,120
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes

Notes: This table reports placebo tests using politician Facebook posts on non-environmental topics. Outcomes are indicators for whether politician i made at least one post in the listed topic on day t, excluding posts classified as environmental by the upstream environmental-post screen. Coefficients are reported as percentage-point changes for a 10-unit increase in the relevant source-specific pollution shock. The specifications mirror the source-specific attribution regressions and include politician and calendar-date fixed effects. Standard errors, reported in parentheses, are clustered two ways by politician and date. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Environmental Bill Introduction Outcomes

	Same-day		0–3 day average	
	Any proposer bill	Number of proposer bills	Any proposer bill	Number of proposer bills
Transboundary shock	-0.024	-0.031	-0.071	-0.059
	(0.026)	(0.052)	(0.047)	(0.079)
Domestic shock	0.009	0.021	-0.040	-0.092
	(0.054)	(0.080)	(0.077)	(0.126)
Politician fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Mean dep. var.	1.44	1.84	1.45	1.86
Politicians	710	710	710	710
Dates	3,286	3,286	3,274	3,274
N	747,488	747,488	729,954	729,954

Notes: This table reports reduced-form estimates of source-specific pollution shocks on environmental bill introduction. The outcomes count bills for which the politician is the representative proposer. Coefficients are reported for a 10-unit increase in the relevant source-specific shock. All dependent variables are multiplied by 100, so column (1) and (3) effects are percentage points and columns (2) and (4) are changes in the number of bills per 100 politician-days. The domestic shock is measured as non-China trajectory exposure interacted with inverse wind speed. Legislative responses are much sparser than Facebook posting outcomes, and the estimates are correspondingly imprecise. Weekly and representative-sponsor specifications are similar and are omitted here for brevity. Standard errors, reported in parentheses, are clustered two ways by politician and date. Means are reported in the same scaled units as the dependent variables. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Next-Election Survival Outcomes

	Won any district seat in next election				Won same constituency in next election			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transboundary shock exposure (z)	-0.0309	-0.0324	-0.0173	-0.0167	-0.0211	-0.0225	-0.0145	-0.0139
	(0.0621)	(0.0628)	(0.0618)	(0.0617)	(0.0600)	(0.0607)	(0.0602)	(0.0601)
Domestic shock exposure (z)	-0.0102	-0.0049	-0.0332	-0.0304	-0.0268	-0.0218	-0.0452	-0.0422
	(0.0592)	(0.0603)	(0.0591)	(0.0595)	(0.0572)	(0.0582)	(0.0570)	(0.0573)
Environmental posting rate (z)		-0.0232				-0.0220		
		(0.0257)				(0.0258)		
Fine-dust posting rate (z)			0.0224	0.0230			0.0389	0.0395
			(0.0258)	(0.0259)			(0.0245)	(0.0246)
China-blame share among fine-dust posts (z)			-0.0448	-0.0501***			-0.0339	-0.0396***
			(0.0280)	(0.0167)			(0.0258)	(0.0142)
Any fine-dust post			0.104*	0.104*			0.0413	0.0412
			(0.0578)	(0.0578)			(0.0569)	(0.0570)
Domestic shock × blame share				-0.0261				-0.0281*
				(0.0198)				(0.0168)
Electoral vulnerability control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.475	0.475	0.475	0.475	0.415	0.415	0.415	0.415
N	383	383	383	383	383	383	383	383
R^2	0.105	0.107	0.126	0.130	0.110	0.111	0.123	0.128

Notes: This table reports exploratory next-election linear probability models using one observation per incumbent legislator. All regressors are aggregated over the 180 days before the next National Assembly election. Columns (1)–(4) use an indicator for whether the incumbent later wins any district seat; columns (5)–(8) use an indicator for whether the incumbent later wins again in the same constituency. All continuous regressors are standardized to mean zero and unit standard deviation, so the coefficients can be interpreted as changes in reelection probability associated with a one-standard-deviation increase in the relevant pre-election measure. The sample is restricted to incumbents with complete 180-day pre-election windows, which yields 383 politicians facing the 2016 and 2020 elections. Every specification controls for standardized electoral vulnerability based on the incumbent's previous vote share, cohort fixed effects, and politician-position fixed effects. Robust standard errors are reported in parentheses. Because the next-election outcomes are constructed from winner rosters, a zero combines incumbents who ran and lost with incumbents who did not run again. The estimates should therefore be interpreted as exploratory downstream evidence rather than as clean causal estimates of electoral punishment or reward. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Party Heterogeneity in Next-Election Survival Associations

	Won any district seat in next election		Won same constituency in next election	
	Democratic incumbents	Conservative incumbents	Democratic incumbents	Conservative incumbents
China-blame share among fine-dust posts (z)	-0.0848***	-0.0242	-0.0724***	-0.0150
	(0.0179)	(0.0406)	(0.0182)	(0.0371)
Transboundary shock exposure (z)	0.166	-0.103	0.191*	-0.0971
	(0.111)	(0.0753)	(0.112)	(0.0731)
Domestic shock exposure (z)	-0.152	0.0532	-0.146	0.0160
	(0.0941)	(0.0805)	(0.0933)	(0.0786)
Fine-dust posting rate (z)	0.000684	0.0567	0.0243	0.0692*
	(0.0417)	(0.0399)	(0.0402)	(0.0367)
Any fine-dust post	0.121	0.0642	0.0385	0.0213
	(0.0880)	(0.0844)	(0.0889)	(0.0827)
Electoral vulnerability control	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Mean dep. var.	0.619	0.406	0.571	0.343
N	168	175	168	175
R^2	0.054	0.088	0.052	0.052

Notes: This table reports split-sample versions of the richest specification from Appendix Table A10. Each column uses one observation per incumbent legislator and aggregates all regressors over the 180 days before the next National Assembly election. The dependent variables are indicators for whether the incumbent later wins any district seat or later wins again in the same constituency. All continuous regressors are standardized to mean zero and unit standard deviation within the pooled democratic-plus-conservative sample before the split-sample estimation. Every column controls for standardized electoral vulnerability based on the incumbent's previous vote share and cohort fixed effects. Robust standard errors are reported in parentheses. These split-sample estimates are descriptive complements to the pooled interaction models, not separate causal designs. In the corresponding pooled interaction regressions, the conservative differential on the blame-share coefficient is positive but imprecisely estimated ($p = 0.223$ for any-seat survival and $p = 0.197$ for same-constituency survival). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.