Bias Against Mobile Capital: Evidence from U.S. Environmental Enforcement*

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Abstract

Do governments discriminate against foreign-owned firms in enforcing regulations? Politicians may favor domestic-owned firms through relaxed enforcement. Yet foreign-owned firms can face discrimination even under politicians who are not protectionist and value stringent regulation: an information-driven bias by bureaucratic regulators. Pro-regulatory politicians task bureaucrats with enforcement, which requires detecting violations that are not perfectly observable. External ownership—foreign or domestic non-local—may signal less familiarity with local regulations and greater violation risk, especially where politicians have promoted stringent rules. Regulators therefore target externally owned firms for additional scrutiny, such as inspections. I examine U.S. environmental regulation, where states can adopt stricter standards and conduct most enforcement. I link administrative records of scrutiny and penalties to ownership data covering 2.4 million plants since 1989. Scrutiny rises after plants are taken over by foreign owners while remaining unchanged after in-state takeovers. This reaction reflects information-driven discrimination rather than protectionism. It extends to domestic out-of-state takeovers. It is concentrated in Democratic strongholds, where pro-regulatory politicians have promoted stringent state-specific regulations. Penalty decisions show no comparable ownership bias but favor campaign donors. Information-driven bias within the bureaucracy, not protectionism, drives discrimination against foreign investors.

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1 Introduction

Do multinational companies (MNCs) receive fair, or even lenient, regulation from host governments? Foreign investors are entitled to "fair and equitable treatment" and "national treatment," core principles in international investment treaties (Simmons, 2014). They can also bargain for lenient regulation by threatening exit and leveraging host governments' demand for investment (Vernon, 1971; Hirschman, 1978). Competition to attract mobile capital could thus undermine regulatory stringency (Rodrik, 1998). This concern has motivated popular opposition to globalization as well as scholarly studies evaluating whether capital mobility weakens formal regulatory standards in environmental, labor, and tax regulation across countries and U.S. states.[1]

Even if foreign MNCs weaken regulatory standards, they may still face discrimination in enforcement. Real-world examples suggest that such discriminatory enforcement may exist: South Korean leaders condemned the 2025 U.S. immigration enforcement raid at Hyundai's plant in Georgia as discriminatory.[2] U.S. politicians recently alleged that the E.U. targeted U.S. MNCs through antitrust enforcement in retaliation for U.S. tariffs.[3] Investors have also increasingly initiated international legal disputes accusing host governments, developed and developing alike, of violating the "fair and equitable treatment" or "national treatment" principles (Moehlecke and Wellhausen, 2022). Do host governments, including those in developed democracies, in fact discriminate against the foreign investors they seek to attract? If so, why?

I argue that foreign-owned firms can face two forms of discrimination: favoritism by politicians and information-driven discrimination by bureaucrats. Politicians may have incentives to favor domestic-owned firms because of domestic firms' demands for protection and greater political influence, citizens' nationalism, or international competition. Favoring domestic-owned firms in enforcing domestic regulations can serve as a behind-the-border or non-tariff measure of protectionism. Yet regulatory enforcement is unlikely to become a venue for protectionism when

[1] For literature reviews, see Drezner (2001), Konisky (2007), and Carruthers and Lamoreaux (2016).

[2] Kell Ng and Ho Lee, "Firms will hesitate to invest in US after raid – S Korea president," *BBC*, Sep. 11, 2025.

[3] U.S. House Judiciary Subcommittee hearing, *Anti-American Antitrust: How Foreign Governments Target U.S. Businesses* (Dec. 16, 2025).

politicians value stringent enforcement and delegate it to autonomous bureaucratic regulators.

However, foreign MNCs can face discriminatory enforcement even under politicians who are not protectionist and value stringent regulation: bureaucratic regulators may exhibit information-driven, or statistical, discrimination based on foreign and non-local ownership. I develop a model in which politicians task regulators with strengthening enforcement to maximize compliance. Regulators therefore seek to detect as many violations as possible but lack perfect information about each firm's ongoing violations. They may use ownership origin as a signal of violation risk. In countries where politicians have established more stringent regulations, regulators may believe that foreign-owned firms, on average, have greater difficulty understanding and adapting to domestic regulations. When regulatory standards and enforcement are delegated to subnational jurisdictions, regulators in high-standard jurisdictions may extend this belief to all externally owned firms, both foreign and domestic non-local.

This belief motivates regulators to use ownership origin as an input in selecting targets for *scrutiny*, subjecting externally owned firms to more frequent scrutiny actions. Scrutiny refers to proactive enforcement attention directed at specific firms and is labeled as "audits," "inspections," or "investigations" in different contexts. The same belief does not generate bias in the later stage of enforcement—penalties—because regulators do not seek to harm externally owned firms. Moreover, information-driven bias against a specific firm can attenuate over time if the firm repeatedly demonstrates compliance and regulators update their beliefs. By contrast, political favoritism can appear in both stages and need not attenuate over time.

I apply my theory to U.S. environmental regulation, where states can adopt regulations more stringent than federal standards and state environmental agencies conduct most enforcement. State politics shapes enforcement patterns. Democratic states are more likely than Republican states to exhibit information-driven bias. Democratic politicians are more pro-regulation and adopt more stringent state regulations. Regulators in Democratic states are therefore more likely to view external ownership as signaling less familiarity with state regulations and to use it in scrutiny targeting. While this informational mechanism should not bias penalty decisions, penalties could reflect political favoritism toward domestic-owned firms, especially campaign donors. Prior work shows that campaign contributions to members of Congress reduce environmental penalties (Heitz et al., 2023). Donations to state politicians may have similar effects, especially in Republican states, where politicians are more supportive of corporate political spending (Gilens et al., 2021).

I draw on administrative and commercial data with exceptional scale and granularity. I use administrative records covering all regulated facilities, scrutiny actions—such as on-site inspections and off-site audits—violation citations, and penalties nationwide. I link these records to comprehensive plant-level business registration data to identify foreign and out-of-state domestic ownership. Together, these datasets cover 2.4 million regulated facilities,[4] 4.5 million scrutiny actions, and 1.5 million citations and penalties between 1989 and 2024. I also examine comprehensive records of state-level campaign donations nationwide. I supplement these data with 27 interviews with 31 leaders and senior managers of state environmental agencies across 21 states.

I first examine bias in selecting scrutiny targets and then bias in penalty size. I implement difference-in-differences (DiD) designs to compare how scrutiny responds to three types of ownership takeovers: from one in-state owner to another, from in-state to out-of-state domestic ownership, and from in-state to foreign ownership. Comparing the effect of the first takeover type with those of the latter two is necessary for identifying bias toward external ownership, with the first takeover type serving as a placebo test for whether scrutiny increases after any ownership change. Comparing the effects of the latter two is necessary for identifying additional bias against foreign ownership beyond the general bias toward non-local ownership. I then conduct mechanism tests to assess whether the differential scrutiny responses reflect proactive targeting of external ownership rather than responses to observed violations. I also compare scrutiny of new greenfield facilities with different ownership types using matching and weighting methods that balance covariates.

I find that, while in-state takeovers do not increase scrutiny actions, foreign takeovers lead to immediate increases, and out-of-state takeovers have similar but weaker effects. These differential responses represent proactive targeting of external ownership: they appear only in high-standard

[4] Following the U.S. Environmental Protection Agency's (USEPA) terminology, I use "facility" to refer to regulated business establishments: firms' physical sites in distinct locations, such as separate plants of multi-plant firms.

Democratic strongholds, apply to takeovers by companies from Western countries, diminish after roughly a decade, and are not explained by increases in facility size or violations following foreign or out-of-state takeovers. I similarly find scrutiny bias against greenfield facilities established by foreign owners and weaker bias against those established by out-of-state owners in Democratic strongholds; both diminish after roughly a decade. In Republican strongholds, neither takeovers nor new greenfield facilities established by foreign or out-of-state investors trigger greater scrutiny. My interviews corroborate these findings and provide direct evidence that regulators in high-standard states view external ownership as a signal of greater violation risk.

I then examine potential bias in penalties—whether violations involving externally owned facilities receive larger penalties—using covariate-balancing matching and weighting to compare violations of similar nature and severity. I find no consistent evidence that external ownership predicts larger penalties, confirming that information-driven bias does not extend to penalties. The absence of comparable ownership bias in penalty decisions also suggests that the observed scrutiny bias does not reflect political favoritism. Political bias does exist in penalty decisions, however, in the form of favoritism toward campaign donors. Such favoritism is more apparent in Republican strongholds, where politicians are more supportive of corporate influence. Some interviewees from these states report being instructed by their governors to reduce penalties for donors. Moreover, this favoritism is driven by domestic rather than foreign firms, indicating a more nuanced form of political favoritism toward domestic firms conditional on their political influence.

The study contributes to debates over whether foreign direct investment (FDI) and mobile capital undermine host-jurisdiction regulation. Numerous studies examine whether foreign and non-local investors weaken environmental, labor, and tax standards across countries and U.S. states. Interstate competition for mobile capital in the U.S. has made race-to-the-bottom theory "a fixture of the state environmental politics literature" (Konisky, 2007, 854). Yet cross-country and U.S.-focused studies find mixed evidence (Drezner, 2001; Konisky, 2007).[5] I shift the focus from

[5] Environmental and labor standards sometimes show evidence of influence by mobile capital. For example, Bayer (2023) shows that, in the E.U., foreign MNCs' mobility helps them obtain more lenient firm-level carbon emission standards. Yet comprehensive reviews by Drezner (2001) and Carruthers and Lamoreaux (2016) conclude that regulatory standards across countries and U.S. states have not clearly co-evolved in a race to the bottom.

standards to enforcement, asking whether externally owned firms receive differential enforcement. Race-to-the-bottom theory predicts that politicians respond to exit threats by offering relaxed enforcement to external investors, especially foreign MNCs (Bayer, 2023). Such leniency is possible because many developed and developing countries have strict standards but weak enforcement, with regulators subject to firm and political influence (Duflo et al., 2018). I provide a novel firm- and plant-level test of this prediction and show that, rather than receiving leniency, externally owned firms can face discrimination in enforcement. Autonomous bureaucrats play an important role in buffering the negative effects of mobile capital on regulatory stringency.

I offer novel evidence of government bias against foreign ownership in the Global North. Given foreign investors' entitlement to "fair and equitable treatment" and their bargaining power, they should receive fair or even lenient treatment, especially in the Global North, where legal institutions protect property rights against expropriation and contract breaches. Recent studies of MNCs in the Global North emphasize their economic and political advantages in shaping regulations and policies rather than their disadvantages (Kim, 2017; Gulotty, 2020; Perlman, 2023; Lee, 2023). Yet anecdotal reports of foreign investors alleging discriminatory enforcement by host governments, including developed ones, have become increasingly common and politicized in trade negotiations. Management scholars likewise emphasize MNCs' operational disadvantages relative to domestic firms in both developing and developed host countries, known as the "liabilities of foreignness" (Zaheer, 1995; Lu et al., 2022). They suggest host-government discrimination as a potential source of such liabilities but have neither theorized nor tested it.[6] Bridging these perspectives, this paper presents a more balanced picture of investor-state relations in the Global North.

Foreign MNCs can face discrimination without encountering the conventional political risks emphasized in the literature on FDI and investor-state relations. Existing studies focus on adverse host-government treatment of foreign investors, especially the confiscation of property rights through outright expropriation or contract breach (Wellhausen, 2015). Contract breaches involve

[6]The closest example in the liabilities of foreignness literature is Wu and Salomon (2017), which compares 189 foreign-owned banks in the U.S. with domestic banks and finds that foreign-owned banks receive more corrective actions. Yet the authors attribute this disparity to regulatory "discrimination" without directly testing the mechanism.

governments confiscating investors' property rights by breaking or renegotiating contracts and are also called indirect expropriations (Esberg and Perlman, 2023). Rather than adverse treatment of foreign investors per se, I analyze discrimination—differential treatment of foreign-owned and domestic-owned firms. Such discrimination requires neither expropriation nor contract breach: politicians may selectively favor domestic-owned firms, or bureaucrats may exhibit information-driven bias against foreign-owned firms.

Bureaucratic information-driven bias is a novel source of discrimination against, and risk to, foreign investors. Its distinctiveness lies in the role of bureaucrats: existing studies of FDI and investor-state relations examine foreign investors' interactions with a unitary host government (Jensen, 2003; Li and Resnick, 2003; Wellhausen, 2015; Johns and Wellhausen, 2016; Beazer and Blake, 2018; Wright and Zhu, 2018; Johns and Wellhausen, 2021; Gao and Chen, 2025), whereas I distinguish bias generated by bureaucratic regulators from bias driven by politicians. Foreign-owned firms can face discrimination even when governments eschew expropriation and contract breaches—and even absent favoritism or protectionism toward domestic firms.

Differentiating bureaucratic from political bias has implications for understanding global disputes over host governments' domestic regulatory decisions: legal disputes brought by foreign investors alleging violations of the "fair and equitable treatment" and "national treatment" principles (Kerner and Pelc, 2022; Moehlecke and Wellhausen, 2022), and interstate trade disputes questioning whether domestic regulations constitute protectionist non-tariff measures (NTMs). While existing studies examine how politicians design regulatory standards to discriminate against imports (Kono, 2006; Gulotty, 2020), I consider when enforcement of domestic regulations discriminates against affiliates of foreign MNCs producing in the host country. Such discrimination depends not only on politicians' incentives to protect domestic firms but also on their relationship with the bureaucratic agents responsible for enforcement. Politically motivated protectionism, expropriation, and contract breaches can explain violations of the two principles and the use of NTMs, but bureaucratic incentives may better explain cases involving host governments, especially rich democracies, that are neither protectionist nor weak in their legal protection of property rights.

2 Theory: Politicians, Bureaucrats, and Enforcement Bias

I theorize political favoritism and bureaucratic information-driven discrimination within a shared principal-agent framework. Politicians are the principals who set regulatory standards, appoint agency leadership, and allocate budgets. They delegate enforcement decisions to bureaucratic regulators, whose goal is to ensure compliance by detecting and correcting violations. Politicians may influence bureaucrats to pursue goals other than compliance.

I consider a common enforcement setting with two features. First, enforcement has two key stages: scrutiny actions and penalties. Regulators lack perfect information about individual firms' ongoing violations and must conduct scrutiny actions, such as on-site inspections or off-site audits of submitted reports, to uncover violations.[7] Scrutiny is selective because capacity constraints prevent regulators from constantly scrutinizing all regulated firms.[8] If regulators document violations, they then determine whether to impose financial penalties. Second, both stages involve discretionary decisions: scrutiny is not conducted through randomization,[9] and penalties are not mechanically fixed by legal rules but involve human judgment. Bureaucratic regulators decide scrutiny targets and penalty size, subject to potential political influence.

Politicians have varying preferences over three goals: favoring domestic-owned firms over foreign-owned ones, attracting investment, and strengthening regulation. Favoritism for domestic-owned firms may reflect firms' demands, public hostility toward foreign investors, or international competition, as I elaborate below. Politicians also differ in how much they value attracting external investment, domestic or foreign, to generate jobs. They also differ in how much they value stringent regulatory standards and enforcement, namely the strength of their pro-regulation ideology. Below, I discuss when politicians have strong incentives to favor domestic-owned firms and, given those incentives, when they choose to shape regulatory enforcement to do so.

[7] Citizen complaints are usually noisy, so scrutiny is still needed to verify violations (Majumder et al., 2025).

[8] Exceptions are high-stakes programs that intensively oversee a small number of firms. For instance, inspectors of the U.S. Nuclear Regulatory Commission are permanently stationed at nuclear plants (Gordon and Hafer, 2005).

[9] For instance, the U.S. Occupational Safety and Health Administration (OSHA) has experimented with randomized inspections, but most OSHA inspections follow neither randomization nor fixed schedules (Johnson et al., 2023).

I then develop a model of optimal enforcement in which regulators seek to maximize compliance and use ownership origin as a signal to detect more violations. Such information-driven targeting is more likely when politicians value regulatory stringency and support regulators' efforts to maximize compliance. While bureaucrats may instead be influenced by firms through corruption, I abstract from such alternative incentives to focus on the principal-agent dynamics.[10] I offer a formalized version of the model, with simulation results, in Appendix A.

2.1 Political Favoritism

Favoring domestic-owned firms over foreign-owned ones in enforcing domestic regulations can serve as a veiled form of protectionism. Specifically, it can constitute a non-tariff measure or behind-the-border restriction on FDI.[11] Existing studies show that governments use domestic regulatory standards and enforcement as non-tariff barriers against imports, advantaging domestically located producers over foreign producers (Kono and Rickard, 2014; Kim, 2018; Kim et al., 2024).[12] Yet barriers against imports do not imply comparable barriers against foreign-owned firms, as governments often pursue distinct policies toward trade and FDI.[13] Given the economic benefits foreign-owned firms bring, such as jobs and skills, when do politicians have incentives to discriminate against them? And when does regulatory enforcement become the venue through which they pursue such favoritism and protectionism?

Favoritism toward domestic-owned firms may reflect firms' demands, citizens' hostility toward foreign investors,[14] or international competition. Although politicians may value the jobs and growth generated by foreign MNCs, domestic firms may exert greater political influence through lobbying and campaign contributions. While foreign MNCs tend to have greater exit mobility,

[10]Economists have developed foundational models of optimal enforcement in which regulators maximize compliance and have extended them to incorporate political and corporate influence. See Shimshack (2014) for a review.

[11]The International Classification of Non-Tariff Measures (ICNTM) (UNCTAD, 2019) includes "trade-related investment measures" as a category of non-tariff measures. Regarding FDI restrictions, see the OECD FDI Regulatory Restrictiveness Index (OECD, 2024) and UNCTAD's Investment Policy Monitor (UNCTAD, 2024).

[12]For example, the Buy American Act prioritizes U.S.-based producers regardless of ownership (Cho et al., 2025).

[13]Some governments restrict imports while remaining open to FDI, as seen in Latin American import substitution and, more recently, the Trump administration's combination of tariffs with efforts to attract manufacturing FDI.

[14]Public opinion studies find less support for government incentives to foreign investors than to domestic ones in the U.S. (Jensen and Lindstadt, 2023; Tingley et al., 2015; Zeng and Li, 2019; Feng et al., 2021; Chilton et al., 2020; Jud, 2023; Chen and Chu, 2025) and elsewhere (Li and Zeng, 2017; Tanaka et al., 2023; Moehlecke et al., 2024, 2025).

politicians may find their exit threats less credible at the enforcement stage because they have already sunk their investments.

When politicians place limited value on attracting investment, they are more likely to manipulate enforcement to favor domestic-owned firms. If investment attraction instead outweighs concerns about firm nationality, they may commit to fair treatment. When politicians have both a moderate interest in attracting investment and incentives to favor domestic ownership, they may attract foreign investment while favoring domestic-owned firms in regulatory enforcement.

Given these incentives, I consider how and when politicians can manipulate enforcement to favor domestic-owned firms. They can pressure bureaucratic regulators either to relax scrutiny and penalties for domestic-owned firms or to impose excessive scrutiny and penalties on foreign-owned firms. Politicians may pursue both strategies simultaneously, but I treat them as separate choices for analytical clarity.

To favor domestic-owned firms and grant them a competitive advantage, providing more lenient scrutiny and penalties is more effective than relying solely on excessive enforcement against foreign-owned firms. Foreign-owned firms would adapt to avoid violations and thus harsher penalties, making excessive penalties harder to impose over time. When excessive enforcement against foreign-owned firms imposes short-term investment costs of adaptation but limited marginal costs of continued compliance, it is unlikely to grant domestic-owned firms a large and durable competitive advantage. Unless politicians relax enforcement for domestic-owned firms, those committing violations would still pay baseline penalties. By contrast, relaxed scrutiny and penalties allow domestic-owned firms to reduce compliance costs and gain a meaningful advantage.

Relaxed enforcement, however, undermines compliance and requires both that politicians place little value on stringent enforcement and that they can control their bureaucratic agents. It would provoke resistance from regulators whose career incentives center on ensuring compliance. When bureaucratic regulators have greater discretion or autonomy, principal-agent problems make politically driven discrimination less feasible.

Principal-agent problems are more severe in regulatory programs with broad coverage and decentralized bureaucracies—such as environmental, labor, and tax regulation—and less severe in programs with narrower coverage and more centralized enforcement, such as antitrust regulation. Every industrial firm is subject to environmental, labor, and tax regulations, so enforcement requires rank-and-file bureaucrats to monitor large numbers of regulated firms. These regulators have career incentives tied to effective enforcement and compliance. By contrast, some regulatory programs have narrower scope, more centralized enforcement, and regulators whose careers depend more on politicians.[15] Antitrust regulation is one example: only firms large enough to become dominant market players are likely targets, and enforcement actions are more likely to be pursued by politically appointed senior regulators (Wood and Anderson, 1993; Allen et al., 2026).

Principal-agent problems are also more severe for routine, decentralized scrutiny than for rarer, more centralized penalty decisions, especially in broad-based environmental and labor regulatory programs. Routine scrutiny is more likely to be delegated to rank-and-file bureaucrats, whereas penalty decisions are rarer and higher-stakes and thus more likely to be centralized among senior regulators appointed by politicians.[16] Hence, politicians may struggle to control routine scrutiny while still pursuing discrimination by influencing penalties. Although political favoritism can in principle bias both scrutiny and penalties, it may appear only in penalties when politicians cannot effectively control bureaucrats' scrutiny decisions.

Another feature of broad-based regulatory programs deepens the principal-agent problem: the small share of foreign-owned firms in the regulated population. For instance, foreign-owned entities comprise around 6% of all entities regulated by U.S. environmental agencies in 2024.[17] By contrast, under narrower programs such as antitrust regulation, which target large market actors, the share of foreign-owned firms can be much larger because foreign firms tend to be large and

[15]Politically appointed bureaucrats may internalize politicians' preferences and pursue such favoritism without direct instructions to advance their careers (Gordon and Hafer, 2005). Bureaucrats seeking politically appointed posts tend to be senior regulators, whereas rank-and-file regulators are evaluated primarily on enforcement performance.

[16]For example, in U.S. environmental and occupational safety and health regulation, rank-and-file inspectors may propose initial penalties for violations identified during inspections. These penalties are then negotiated with violators, while senior regulators approve the final settlement amounts. If initial negotiations fail, violators negotiate directly with senior agency officials, and unresolved cases may ultimately proceed to litigation.

[17]Based on data used in this paper, introduced in Section 3.

productive enough to bear the costs of FDI.[18] Relaxing enforcement for domestic-owned firms is therefore costlier to bureaucratic regulators under broad-based programs, where most regulated firms are domestically owned.

In sum, while politicians may face incentives to favor domestic-owned firms over foreign-owned ones, they are not always willing or able to manipulate regulatory enforcement to do so. Political favoritism drives enforcement when politicians moderately value investment attraction, place limited value on stringent regulation, and can effectively control bureaucratic regulators.

2.2 Bureaucratic Information-Driven Discrimination

I now consider a different scenario in which bureaucrats face no political pressure to favor any group of firms and instead pursue their professional enforcement goals with substantial autonomy. This requires politicians to place a high value on regulatory stringency while attaching limited importance to both investment attraction and favoritism toward domestic-owned firms. Such politicians support stringent regulations and allow regulators to use their professional judgment to maximize compliance. If politicians instead prioritize investment attraction, they would keep regulatory standards close to, or even below, those of other jurisdictions and may pressure regulators to avoid scrutinizing new investors. Likewise, if they prioritize domestic-owned firms, they would pressure regulators to relax enforcement for them.

Under these conditions, I consider how regulators select scrutiny targets and determine penalties. Regulators lack perfect information about individual firms' ongoing compliance and may therefore use external ownership to infer violation risk. I argue that, in jurisdictions with relatively stringent regulatory standards, regulators may believe that foreign-owned and domestic non-local firms are more likely to have violations.[19]

[18] For example, observers have argued that the E.U.'s antitrust enforcement against U.S. MNCs is not discriminatory because U.S. firms are disproportionately large in the E.U. market and more likely to become enforcement targets. That said, some argue that the timing of enforcement, following the Trump tariff threats, indicates strategic intent (Lester, 2026).

[19] Future studies could consider contexts where regulators instead associate external ownership with fewer violations. In developing countries with very low environmental or labor standards, regulators may associate foreign ownership with fewer violations, leading foreign-owned firms to receive relaxed enforcement regardless of true compliance.

First, regulators may believe that externally owned firms are, on average, less familiar with local regulations because regulatory standards vary across jurisdictions. They may infer that foreign-owned firms are accustomed to different national standards and less familiar with those in the host country. Similarly, when rules are decentralized across subnational jurisdictions, regulators may believe that domestic non-local firms are accustomed to different subnational standards and less familiar with local ones. Such beliefs are more likely in jurisdictions with relatively stringent or complex regulatory standards.

While some firms may be known to originate from weaker regulatory regimes, regulators may apply this belief to externally owned firms more broadly, including those from jurisdictions with relatively stringent standards. Regulators are concerned about differences in regulations, not merely stringency. For instance, some environmental abatement technologies approved in Europe may not be approved in the U.S., so their use is legitimate in the E.U. but could result in violations in the U.S.[20] Many U.S. states regulate unique pollutants or activities because of their local ecological systems, even when most other states—including those with highly stringent regulations—do not.[21] Moreover, regulators may lack precise knowledge of firms' home-jurisdiction regulations and prefer to err on the side of caution. Hence, regulators may believe that externally owned firms generally have greater difficulty understanding and adapting to local regulations.

Second, regulators may believe that externally owned firms' multi-layered management structures make it harder for them to adapt to host-jurisdiction regulations. In MNCs, headquarters managers may lack knowledge of local regulations and pressure local managers to prioritize operational goals that conflict with compliance goals.[22] Within host-country subsidiaries, compliance is often handled by local staff while key decisions are made by expatriate managers, creating communication barriers. Expatriate managers often serve on temporary international assignments before

[20]Interviews A1, A3, A12, A14, A19, A27. Countries and states may have the same formal standards but different interpretations and enforcement practices, which also create difficulties for compliance (Interviews A9, A17, A21).

[21]Interviews A1, A3, A4, A6, A7, A9, A10, A16, A17, A19. For instance, the Great Lakes states have unique stormwater regulations to protect wetlands, but their overall regulatory stringency is intermediate among the 50 states.

[22]For instance, one environmental regulator interviewed for this project said that local employees "pretty much run things" when operations run smoothly, but foreign investors monitor closely and may intervene during enforcement; in several cases, local staff understood what needed to be done, but company headquarters said no (Interview A10). I corroborate this claim with more interviews in Section 6.

returning to headquarters or moving across locations, limiting their accumulation of local knowledge.[23] Domestic non-local firms may face similar organizational barriers when key decisions are made by managers outside the jurisdiction with limited knowledge of local rules.

If regulators believe that externally owned firms, as a group, are more likely to violate regulations, they expect greater returns from targeting scrutiny toward them. They therefore proactively scrutinize externally owned firms without first observing firm-specific evidence of noncompliance, increasing each firm's probability of scrutiny. Without such proactive targeting, regulators would simply react to observed noncompliance. My formal model in Appendix A shows that targeting based on this belief leads to more detected violations and greater compliance, especially when compliance information is poorer.

The bias should attenuate rather than persist into penalties. Targeted scrutiny is motivated by the belief that externally owned firms lack familiarity with host regulations; it does not imply that they have inherently lower ability or intention to become compliant. Because they are subsidiaries of companies large and productive enough to operate across borders, experienced regulators should understand that externally owned firms generally have equal or greater capacity to comply. Thus, insofar as regulators are not prejudiced against them or seeking to favor locally owned firms, the same belief should not lead to excessive penalties.

Information-driven bias may also decline over time for externally owned firms that demonstrate good compliance through repeated interactions with regulators. A firm repeatedly found compliant provides evidence that overrides origin-based priors. Through these interactions, regulators accumulate firm-specific information and update their beliefs. This process unfolds gradually, however, because scrutiny and other enforcement actions may be infrequent for each firm. Regulators also retain their origin-based priors for new investors and firms that fail to adapt. Appendix A.4 reports simulations showing how this updating occurs over time.

[23] Interview B1: One senior American manager at a foreign-owned manufacturing firm in the Midwest reported that home-country managers rotated back to headquarters every two years, limiting their accumulation of local knowledge and creating persistent information barriers between them and local compliance managers, as well as regulators. For evidence from the management literature, see Feldman and Tompson (1993) and Takeuchi et al. (2019).

2.3 Differentiating the Two Types of Bias

Political favoritism and bureaucratic information-driven discrimination have distinct observable implications across enforcement stages, temporal dynamics, targets, and political contexts. Information-driven bias appears in scrutiny targeting but not penalty severity, whereas favoritism may appear in scrutiny, penalties, or both, depending on where politicians can effectively influence bureaucratic decisions. Information-driven bias diminishes as firms repeatedly demonstrate compliance and regulators update their beliefs, whereas favoritism need not diminish with firm age. Information-driven bias may extend to all externally owned firms, including domestic non-local ones, whereas political favoritism divides firms by nationality, privileging domestic over foreign ownership. Empirically, identifying nationality-based favoritism and protectionism therefore requires benchmarking foreign-owned firms against domestic non-local firms.

The two forms of bias also arise under different political contexts. Political favoritism is most likely when politicians place a high value on favoring domestic-owned firms and less value on investment attraction and regulatory stringency. Bureaucratic information-driven bias is most likely when politicians place a high value on regulatory stringency and little on investment attraction or favoritism toward domestic-owned firms. Neither bias is likely when politicians emphasize investment attraction and place little value on regulatory stringency or favoring domestic-owned firms.

These variations in politicians' preferences do not always map onto the left-right ideological spectrum. Left-leaning politicians generally place greater value on regulatory stringency, but whether they also prioritize investment attraction or favor domestic-owned firms depends on the context. In many countries, left-leaning politicians are more supportive of investment attraction to generate jobs for working-class supporters (Pinto, 2013). State-level politics in the U.S., however, offers a counterexample: Republicans are less pro-regulation and more pro-business than Democrats; moreover, Republican states tend to depend more on external investment for jobs, and their politicians appear more likely to limit labor and environmental regulations to attract investors.[24] I elaborate on the role of partisanship when introducing the U.S. context below.

[24]For anti-union regulations, see Anzia and Moe (2016). For environmental "no-more-stringent" laws, which

3 The Case of U.S. Environmental Enforcement

I examine three major regulatory programs: the Clean Air Act (CAA), governing air emissions; the Clean Water Act (CWA), governing discharges into surface waters; and the Resource Conservation and Recovery Act (RCRA), governing hazardous and solid waste. Together, these programs form the core of environmental regulation covering the private-sector economy.[25]

Under all three statutes, the U.S. Environmental Protection Agency (USEPA) delegates standard-setting and enforcement to states. States may adopt their own standards, provided they are no less stringent than the USEPA's minimum standards. State environmental agencies, such as the Utah Department of Environmental Quality, have "primacy" in enforcement and conduct the vast majority of enforcement actions. These agencies are part of state governments rather than branches of the USEPA. State politics and partisanship are thus central to my theoretical expectations.

3.1 Expectations for Bureaucratic Bias

I use *scrutiny actions* to refer collectively to proactive compliance evaluations directed at specific regulated facilities. These actions may be conducted on-site through physical inspections or off-site through record audits. In the USEPA's terminology, they are called "compliance evaluations" under the CAA, "inspections" under the CWA, and "evaluations" under the RCRA.[26]

Information-driven bias in scrutiny targeting is possible because regulators follow guiding procedures but retain substantial flexibility. No statute specifies how frequently facilities must receive on-site inspections under these programs, nor are inspections randomly assigned. The USEPA recommends minimum inspection frequencies based on facility class: under the CAA, Major facilities should be inspected at least once every two years and Synthetic Minor facilities at least once every five years, while Minor facilities have no recommended minimum frequency. These frequencies are not binding, however, and additional inspections are common.[27] Scrutiny deci-

prohibit state standards from exceeding federal requirements, see Woods (2021).

[25]Other environmental regulatory programs not examined here, such as the Safe Drinking Water Act, primarily cover government-owned facilities in sectors such as water supply and waste management.

[26]CWA "inspections" include both physical inspections and off-site audits of records and documents.

[27]Interviews A4, A8, A9, A12, A13, A16, A21, A22, A24, A27. One interviewee described deliberately with-

sions typically consider facility characteristics, such as size and industry; prior violations; and citizen complaints.[28]

Information-driven bias is likely to appear in Democratic strongholds but not in Republican strongholds. Democratic state leaders and legislators have a more pro-regulatory ideology and adopt more stringent state-specific regulations. This makes regulators more likely to believe that external investors have insufficient understanding of state regulations and face greater violation risk. By contrast, Republican state governments are right-leaning, less pro-regulation, and more pro-business. To promote business growth and attract investment, they tend to keep regulations closer to the federal baseline. They are also more likely to adopt "no-more-stringent" laws that prohibit state environmental regulations from exceeding federal requirements (Woods, 2021). Regulators in Republican states should therefore be less likely to believe that externally owned firms have difficulty understanding state regulations. Furthermore, Republican state politicians might instruct regulators to avoid intensive scrutiny of new investors.

The three programs—air (CAA), water (CWA), and waste (RCRA)—differ along two dimensions that shape the likelihood of information-driven discrimination: the quality of compliance information available to regulators and cross-state heterogeneity in regulatory standards (Table 1). On both dimensions, the water and waste programs leave less room for information-driven bias than the air program.

Table 1: **Key differences across the three regulatory programs examined in this paper.**

	Air (CAA)	**Water (CWA)**	**Waste (RCRA)**
Regulators' uncertainty about ongoing compliance	Higher	Lower	Lower
Cross-state heterogeneity in standards	Higher	Lower	Lower
Likelihood of information-driven bias	**Higher**	**Lower**	**Lower**

First, when pollution is continuously monitored at each emission point, regulators can use

holding inspection schedules from businesses and using the possibility of discretionary inspections as a deterrent. Regulators conduct inspections beyond the recommended schedule when they suspect problems, emphasizing that facilities should understand that they "could come back next week" (Interview A21).

[28]Interviews A8, A9, A12, A13, A16, A17, A22, A24, A27.

monitoring data rather than ownership origin to select scrutiny targets. Yet precisely monitoring fugitive air emissions at each emission point is difficult and costly, and most facilities are not required to monitor and report regularly.[29] Regulators have better information under the water program, where discharge permits generate universal facility-level monitoring data,[30] and under the waste program, where the standardized manifest system tracks hazardous waste.[31]

Second, cross-state regulatory heterogeneity is greatest under the air program, where State Implementation Plans allow states to adapt federal technical standards. By comparison, water permits are anchored in federal guidelines, while waste regulation follows more prescriptive federal standards.[32] Foreign and out-of-state investors are therefore more likely to have difficulty adapting to state-specific regulations under the air program. Taken together, information-driven bias should be more likely under the air program than under the other two programs.

3.2 Expectations for Political Bias

Political favoritism toward domestic-owned firms is especially likely to take the form of favoritism toward campaign donors in Republican strongholds. Heitz et al. (2023) show that firms donating to members of the U.S. Congress receive reduced penalties when found in violation of environmental regulations. At the state level, such favoritism is particularly likely in Republican states because Republican politicians are more supportive of corporate donors' influence in politics (Gilens et al., 2021). Although foreign-owned firms can make campaign donations, domestic firms may have greater expertise and effectiveness in using donations to secure beneficial exchanges. Additionally, because conservative ideology is closely tied to nationalism in the U.S., Republican voters hold less favorable attitudes toward foreign investors (Zeng and Li, 2019). Republican state politicians may therefore face greater political costs for favoring foreign investors than for shielding domestic ones.

Political influence on penalties is possible for two reasons. First, financial penalties are not

[29] Interviews A7, A13, A17, A21, A26.

[30] Interviews A4, A12, A17, A26.

[31] Interviews A9, A17, A21, A26.

[32] Interviews A1, A4, A7, A14, A17, A21, A26. Multiple interviewees also noted that countries and states may apply the same standards or rules differently, with important implications for compliance and enforcement (Interviews A9, A17, A21).

fixed but determined through bureaucratic assessment and negotiation with violators. Each state has its own penalty guidelines, which set upper limits based on pollutant and violation type, violation severity, and whether the facility is a repeat violator. After proposing initial penalties, regulators negotiate with violators to determine final amounts. Violators can often negotiate lower penalties because the guidelines set upper limits and regulators consider managers' displayed goodwill, including whether violations were unintentional and whether managers are eager to correct their practices.[33] Second, state environmental agencies are part of state governments and depend on funding from state legislatures, exposing them to oversight and influence from governors and legislators. According to a senior manager leading one of the three regulatory programs in their state, their department receives hundreds of inquiries from state politicians about penalties each year.[34] Agency leaders are appointed by governors and often review and approve final penalties and sometimes participate in negotiations with companies.[35]

Moreover, political favoritism toward domestic-owned firms may appear more readily in penalty decisions than in scrutiny targeting. Each state has an agreement with the USEPA specifying recommended minimum inspection frequencies. Although these frequencies are not binding at the facility level, inspectors in Republican strongholds must broadly adhere to them or risk USEPA intervention, making it infeasible to forgo minimum inspections for large numbers of domestic-owned facilities. State politicians therefore face greater principal-agent problems in influencing scrutiny targeting. By contrast, penalty decisions provide greater room for reducing enforcement intensity, allowing politicians to more easily seek reduced penalties for firms they wish to protect.

3.3 Combining Administrative Records and Business Registration Data

I use administrative records of regulated facilities and enforcement actions gathered by the USEPA. The USEPA's facility database provides identifying information on all facilities regulated under the three programs. My sample includes 2,385,260 facilities, excluding government-owned facilities such as municipal wastewater treatment plants. The enforcement records cover all scrutiny actions

[33] Interviews A4, A7, A8, A9, A10, A11, A12, A17, A19, A26, A27.

[34] Interview A16.

[35] Interviews A4, A9, A10, A11, A12, A13, A19, A24.

conducted, violations cited, and penalties imposed by the USEPA and state agencies since each program's enactment in the 1970s. Appendix Table C1 provides details on these data.

The USEPA's facility database does not record ownership origin, so I link it to a proprietary business registration database—Dun & Bradstreet (D&B)[36]—for ownership and additional facility characteristics. Although the U.S. Census Bureau maintains plant-level census data on MNC investment, federal law protects the data's confidentiality and prohibits merging them with data from other government agencies, including the USEPA. D&B is the largest private database of U.S. business establishments. Its unit of observation is the establishment-year, and its definition of an "establishment" matches the USEPA's definition of a facility, with both referring to distinct business locations such as plants. The USEPA also relies on D&B to identify facilities and build its facility database, facilitating consistent linkage. D&B tracks each establishment's ownership over time and, for each establishment-year, reports its headquarters, immediate parent, and global ultimate owner.[37] I classify an establishment as foreign-owned if any of these entities is located outside the U.S.

I link the USEPA facility data to D&B using the USEPA's official linkages for selected facilities and original linkages for the remainder. I begin with the USEPA's linkages for facilities in the Toxics Release Inventory (TRI). Launched in 1989, TRI includes 62,864 high-priority facilities that exceed statutory thresholds for chemical emissions and must submit detailed annual reports. As part of this reporting, facilities provide D&B identifiers, which the USEPA manually verifies. I directly adopt these identifiers for TRI facilities. For all remaining facilities, I construct original linkages using a multi-stage matching procedure that combines exact string matching with machine-learning-based fuzzy matching (Appendix C.3). Because TRI reporting data are available for 1989–2024, my sample covers the same period. Table 2 provides key summary statistics for the final sample, while Appendix C.4 provides additional descriptive statistics.

[36]It is also branded as the National Establishment Time-Series (NETS) database.
[37]See Appendix C.2 for definitions and an example.

Table 2: **Key summary statistics of the final sample (1989–2024).**

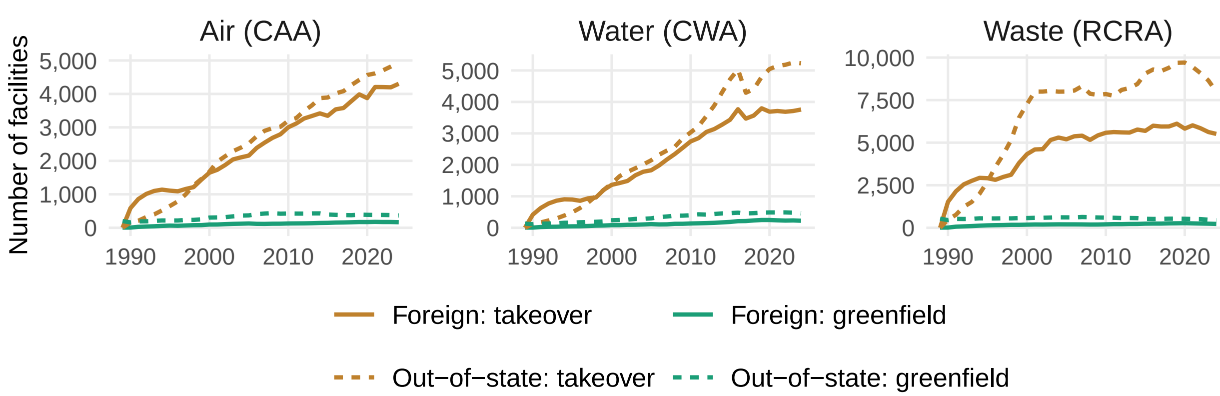
	Toxics Release Inventory (TRI) [official linkage to D&B available]			Non-TRI		
	CAA	CWA	RCRA	CAA	CWA	RCRA
Facilities	27,218	22,500	43,137	222,920	777,815	1,246,890
Manufacturing	85%	83%	86%	35%	24%	23%
Foreign-owned, 2024	23%	24%	22%	7%	4%	5%
Out-of-state-owned, 2024	34%	36%	34%	17%	14%	32%
Had any scrutiny action in 2024	30%	17%	16%	8%	8%	5%
Had any on-site inspection in 2024	26%	16%	13%	7%	7%	4%

Notes: The sample includes 2,385,260 facilities, including 61,929 TRI facilities and 2,323,331 non-TRI ones.

4 Bias in Selecting Scrutiny Targets

A key feature of the sample guides my research design for identifying ownership bias in scrutiny targeting. Among facilities that have ever had external ownership, far more acquired it through a takeover (i.e., merger or acquisition, M&A) than were established as greenfield facilities by external owners. Figure 1 shows, for each year, how many facilities are foreign- or out-of-state-owned, separating those that acquired that ownership through a takeover from those created under it. Takeovers dominate throughout: over the sample period, the air program sample contains 8,206 facilities that became foreign-owned through a takeover versus 366 created as foreign-owned, and 9,928 that became out-of-state-owned versus 1,007 created as out-of-state-owned, with comparable ratios under the water and waste programs.

Figure 1: **Facilities come under external ownership far more often through takeover than through greenfield entry.**



Notes: M&A facilities are first observed in-state and later become foreign- or out-of-state-owned; *greenfield* facilities are those that D&B records as externally owned at creation, including pre-1989 builds, counted until their first ownership change. Facilities whose ownership at creation cannot be verified by D&B are excluded.

Therefore, I begin by examining scrutiny targeting following ownership takeovers, which not only has greater substantive importance but also allows more rigorous causal inference by exploiting within-facility changes in ownership. I then examine bias toward the smaller set of new greenfield facilities. Despite the smaller sample and less causal leverage, greenfield facilities warrant separate treatment because they are more likely to feature practices and technologies that investors bring from their home jurisdictions, potentially posing distinct challenges for regulators.

4.1 Scrutiny Responses to Ownership Takeovers

I identify bias in scrutiny targeting toward foreign and out-of-state ownership by examining whether facilities previously owned by in-state firms experience increased scrutiny after being acquired by foreign or out-of-state owners. I estimate and compare three Average Treatment Effects on the Treated (ATTs): (1) transitions from one in-state owner to another, (2) transitions from in-state to out-of-state ownership, and (3) transitions from in-state to foreign ownership. Comparing the effects of the latter two ATTs with the first is necessary for identifying bias toward external ownership. Comparing the last two ATTs is necessary for identifying the additional bias specific to foreign ownership, beyond bias toward non-local ownership in general.

The first ATT (in-state takeover effect) serves as a placebo test for whether scrutiny increases after any ownership change. It should not, because no federal or state rules require scrutiny actions or other administrative procedures involving regulators following ownership changes.[38] Any scrutiny response to takeovers should therefore reflect regulators' discretionary decisions rather than mechanical, routine reactions. The first ATT tests this claim empirically: if it is correct, in-state takeovers should have no effect on scrutiny.

Identification then rests on a parallel trends assumption. For each ATT, it requires that, absent the ownership change, treated facilities would have followed the same scrutiny trajectory as their matched in-state controls. That is, acquirers do not systematically select facilities already experiencing declining compliance and rising scrutiny intensity prior to acquisition. Because my aim is the *differential* scrutiny response across takeover types rather than the magnitude of any

[38] All interview respondents with inspection experience confirmed this.

single ATT, the design does not hinge on this assumption holding exactly for each transition: any selection or scrutiny dynamic common to ownership changes in general is captured by the in-state-to-in-state ATT and nets out when I compare it against the foreign and out-of-state ATTs. What remains is the weaker requirement that pre-acquisition selection not differ systematically across the three takeover types: foreign and out-of-state acquirers do not differ from in-state acquirers in their tendency to select facilities with deteriorating compliance and rising scrutiny. No systematic evidence directly addresses this selection concern.[39] I examine it empirically: the event-study estimates below include pre-treatment placebo periods, and differential selection across takeover types would appear as differential pre-trends in scrutiny before acquisition.

Control units are facilities owned by in-state firms at the time of each treated unit's transition and must satisfy three criteria.[40] First, treated units are matched only to control units identical in (1) state, (2) two-digit SIC industry, (3) whether the facility has ever been subject to TRI reporting, and (4) designated facility class.[41] Second, I exclude facilities that have ever been under a third ownership type irrelevant to the ATT being estimated. For instance, when estimating the ATT of foreign ownership, I exclude facilities that have ever been out-of-state-owned, which may differ systematically from always-in-state facilities.[42] Finally, when estimating the ATT of foreign or out-of-state ownership, I restrict control units to in-state facilities with separate in-state headquarters or in-state parent firms, excluding single-site independent firms. This restriction accounts for the fact that foreign and out-of-state takeovers involve firms large and productive enough to own facilities in multiple locations, whereas many in-state firms are smaller.

This DiD design can show whether regulators respond to new external owners with greater scrutiny, and I supplement it with mechanism tests to assess whether such differential responses represent bias against external ownership. My theory holds that regulators use external ownership

[39] To my knowledge, the only study linking EPA enforcement data to business registration records to examine foreign-owned facilities is Holladay and Roush (2025), which relies on a 10 percent sample of the TRI database.

[40] These include both facilities that remain in-state-owned throughout the sample period (never-treated) and those that are in-state-owned at time t but later transition to another ownership type (not-yet-treated).

[41] Under the air program, this refers to Major vs. Synthetic Minor vs. Minor; under the water program, Major vs. Minor; and under the waste program, Large, Small, and Very Small Generators, and Nongenerators.

[42] Analogously, estimation of the ATT for out-of-state takeovers excludes facilities that have ever been foreign-owned.

itself as a signal of violation risk, subjecting externally owned facilities to greater scrutiny on a group basis. It is necessary to show that the differential scrutiny responses do not simply reflect reactions to observed violations or other signals, such as facility size. Foreign and out-of-state takeovers could expand facility size and production, increase emissions, and generate more self-reported violations, all of which regulators normally consider when selecting scrutiny targets. In particular, the USEPA recommends that state agencies set minimum inspection frequencies by designated facility class.[43] If foreign and out-of-state takeovers move facilities from a lower to a higher class, their effects on scrutiny could be partly mechanical. Thus, I examine whether foreign and out-of-state takeovers lead to higher facility classifications, greater emissions, or more self-reported violations, and whether these changes account for the observed scrutiny responses.

I implement this DiD design using recent estimators that accommodate staggered treatment timing and heterogeneous treatment effects in an event-study framework. Treatment timing is staggered because ownership changes occur in different years across facilities,[44] and standard two-way fixed-effects estimators can be biased in such settings. The primary estimation method for results presented below is the DiD with matching estimator `PanelMatch` (Imai et al., 2023); I use a doubly robust DiD estimator following Sant'Anna and Zhao (2020), and the fixed-effects counterfactual estimator implemented in `fect` (Liu et al., 2022) as robustness checks. I choose `PanelMatch` as the primary estimator because it integrates the DiD design with covariate-balancing matching and weighting methods to minimize differences between treated and control units on confounding covariates. I use covariates that capture the observable information regulators normally use in scrutiny decisions: recent scrutiny actions, previous citations and penalties, self-reported violations, and facility size (employees and sales). For each comparison in each sample, I try all matching and weighting methods available in `PanelMatch` and adopt the one that achieves the best covariate balance. Imbalance rarely exceeds 0.1 standard deviation. Appendix D reports the distribution of treatment years, model specification, covariates, and covariate balance.

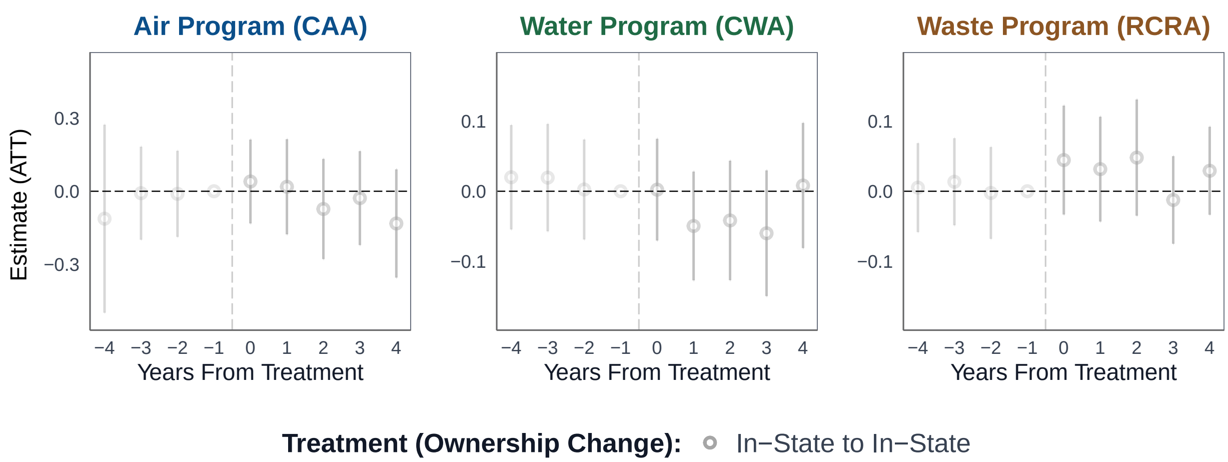
[43] As noted previously, the USEPA recommends that, under the CAA, Major facilities should be inspected at least once every two years and Synthetic Minor facilities at least once every five years, while Minor facilities have no recommended minimum frequency. Yet these frequencies are not binding, and additional inspections are common.

[44] See Appendix D.1 for the distribution of treatment timing.

Baseline Results

I first report the effects of in-state takeovers in Figure 2 to validate the assumption that ownership changes in general should not trigger routine scrutiny responses. The three panels report estimates for the air, water, and waste program samples, respectively. The dependent variable is the number of scrutiny actions conducted under each program per facility-year.[45] Hollow gray dots denote effect estimates for in-state takeovers. Each estimation includes four pre-treatment periods as placebo tests for pre-trends, with effects reported from the treatment year through the fourth post-treatment year.[46] I use the subsample of TRI facilities linked to D&B through the USEPA's official linkages to ensure data quality. Appendix D.7.1 reports results from the full sample.

Figure 2: **Effects of in-state takeovers on scrutiny actions per year, by regulatory program.**



Notes: Dependent variable: number of scrutiny actions (i.e., on-site inspections and off-site audits) per facility-year. 95% confidence intervals (CIs) are shown. Standard errors are calculated using the conditional method. The placebo periods ($t-4$ to $t-1$) are shaded in gray. The period $t-1$ is used as the reference period.

Figure 2 shows that in-state takeovers have no statistically significant effects on scrutiny actions in any of the three programs. The estimates remain close to zero throughout under the air program. They are negative in some post-treatment years under the water program and positive in some years under the waste program, but never statistically distinguishable from zero. These null effects across all three programs confirm that ownership changes in general do not trigger scrutiny responses.

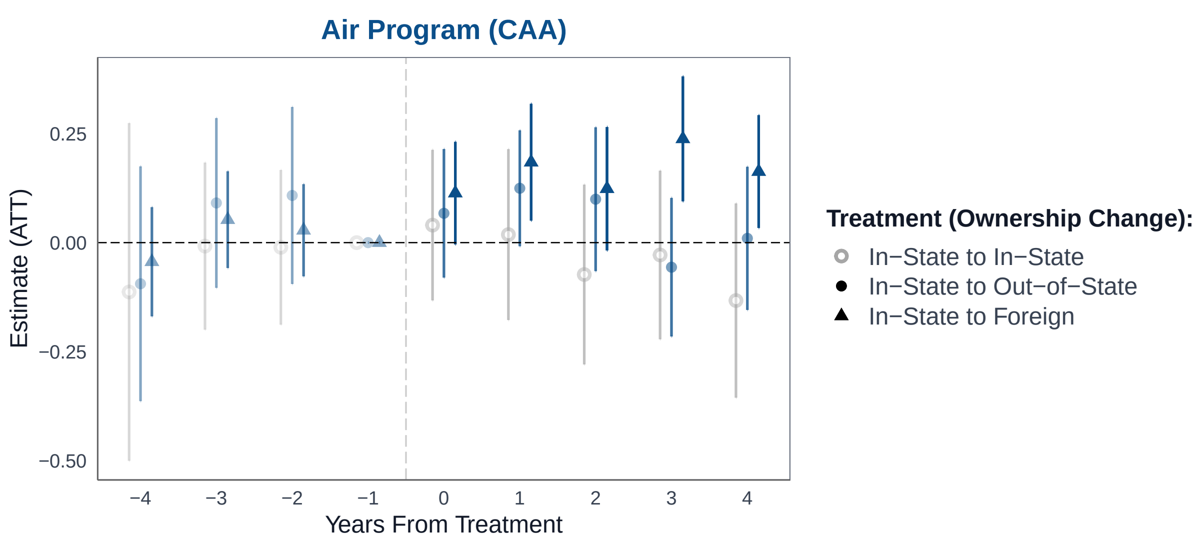
[45] I use a binary indicator for any scrutiny action as the outcome in a robustness check (Figure D31).

[46] I choose four pre-treatment years because it is a reasonable period over which to complete a plant acquisition. I restrict the post-treatment period to the same length to guard against violations of the temporary carryover assumption, extrapolation, and changes in sample composition in later years. I vary both choices in robustness checks.

Additionally, the pre-treatment placebo estimates are flat and close to zero, suggesting that new owners do not select facilities based on preexisting scrutiny trends.

Figure 3 presents the full results for the air program, where information-driven bias should be most likely because of poorer compliance information and greater interstate heterogeneity in standards (Table 1). Hollow gray dots continue to denote estimates for in-state takeovers, solid dots denote estimates for out-of-state takeovers, and darker triangles denote estimates for foreign takeovers. The estimates are reported in raw units and should be interpreted relative to the sample mean of 0.79. Foreign takeovers immediately increase scrutiny actions, and the effect persists throughout the period. Point estimates range from 0.11 to 0.24 additional scrutiny actions per facility-year, corresponding to sizable increases of roughly 14 to 30 percent over the sample mean. Estimates for out-of-state takeovers are positive and larger than those for in-state takeovers in the first three years, reach statistical significance at the 90% confidence level in the second year, and move closer to zero thereafter. Figure D13 plots the differences between the foreign and out-of-state takeover effects and the in-state takeover effects, with 95% CIs.

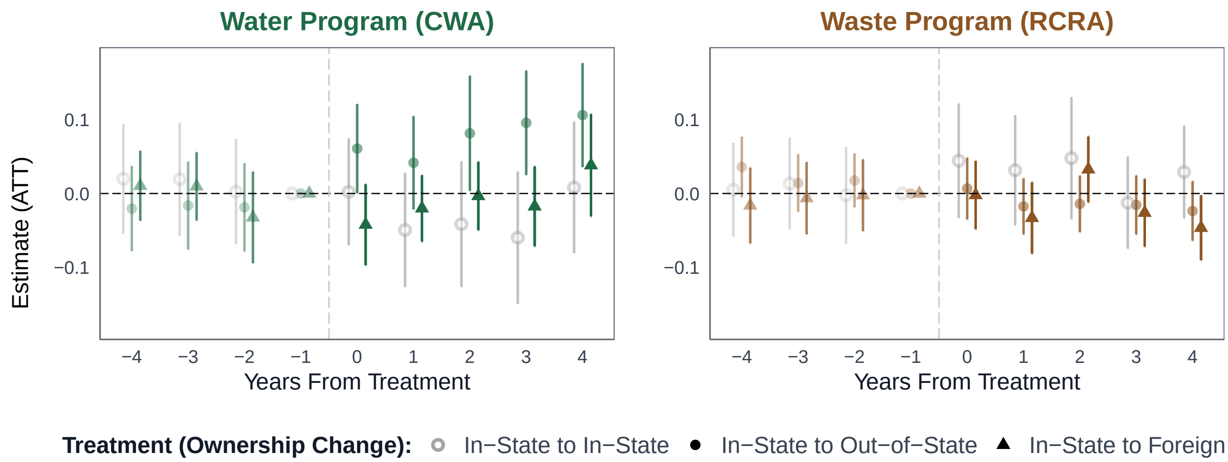
Figure 3: **Effects of ownership takeovers on annual scrutiny actions: air program.**



Notes: The dependent variable is the number of scrutiny actions (i.e., on-site inspections and off-site audits) conducted under the air program (CAA) per facility-year. The sample mean is 0.79. Model details follow Figure 2.

Figure 4 reports the results for the water and waste programs. Under the water program, out-of-state rather than foreign takeovers have positive and statistically significant effects, although estimates for foreign takeovers tend to exceed those for in-state takeovers. Under the waste program, both foreign and out-of-state takeovers generally have null effects. These patterns are consistent with my expectation that information-driven bias is less likely under these programs because of better compliance information and less interstate heterogeneity in standards (Table 1).

Figure 4: **Effects of ownership takeovers on scrutiny actions: water and waste programs.**



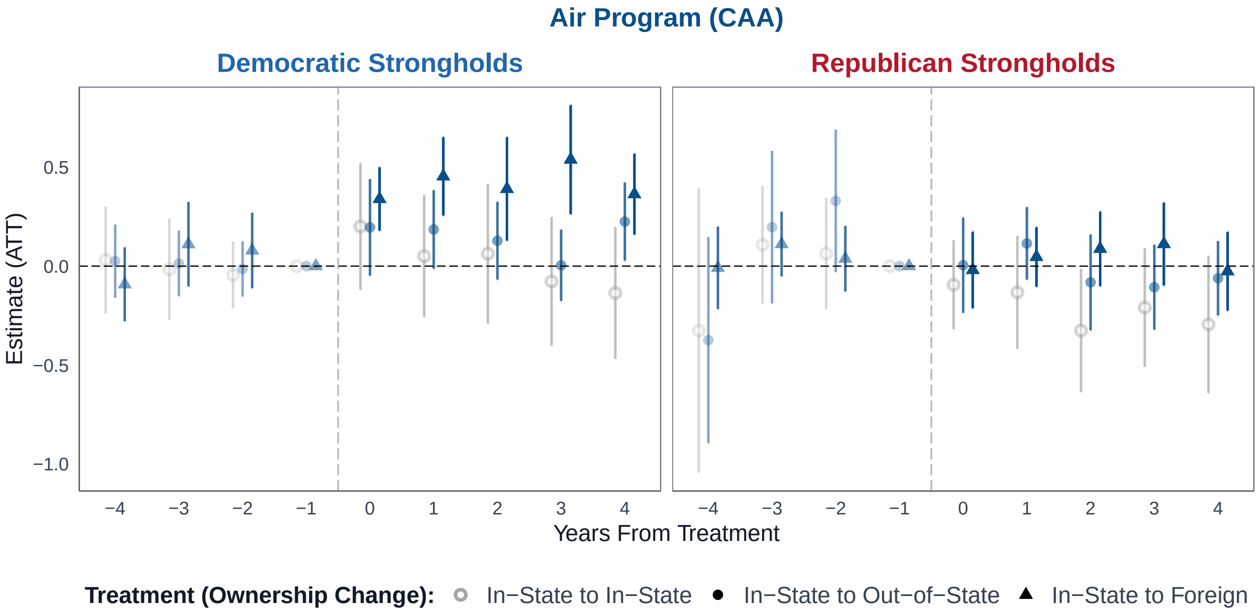
Notes: The dependent variable is the number of scrutiny actions conducted under each program per facility-year. The sample means are 0.24 and 0.26, respectively. Model details follow Figure 2.

State Heterogeneity

From here on, I focus on the air program and provide additional evidence to interpret the observed differential scrutiny responses. I first examine heterogeneity across states to test my expectation that information-driven bias is more likely in Democratic strongholds, where politicians tend to support more stringent environmental standards and enforcement. Figure 5 reports results for two subsamples: Democratic and Republican stronghold states. Because the sample spans 1989–2024, I classify states based on presidential election history from 1988 to 2024.[47] In the Appendix, I also report results using an alternative classification based on the stringency of state environmental standards (Figure D15).

[47] See Figure D8 for a map and Appendix D.6 for the coding rule. I do not use time-varying measures of governors' partisanship for two reasons. First, a time-varying classification would move some states across groups over time and break many units' treatment histories. Second, presidential voting better captures state-level partisan tendencies because many Democratic strongholds have recently had moderate Republican governors who win on policy appeals rather than ideology. For example, Massachusetts had Republican governors for 24 of the 36 years between 1989 and 2024, and New Jersey for 17 of those 36 years.

Figure 5: **Effects of ownership takeovers on air scrutiny actions by state partisanship.**



Notes: Here, I present the results using the subsample where TRI facilities are linked to the D&B dataset by the USEPA's official linkages built for TRI reporting. Results for all three types of states, including swing states, are available in Figure D9. Figure D12 reports the results from the full sample. Other details follow Figure 2. Figure D14 reports the differences between the foreign and out-of-state takeover effects and the in-state takeover effects.

Two findings are important. First, the baseline positive effect of foreign takeovers is driven by Democratic strongholds, where point estimates range from 0.34 to 0.54 additional scrutiny actions per facility-year. In Republican strongholds, estimates for foreign takeovers are indistinguishable from zero throughout the estimation window. Facilities in Democratic strongholds average 0.55 scrutiny actions per year, so the effects are substantively large, amounting to increases of roughly 60 to 100 percent. These large relative effects do not reflect heavier baseline scrutiny in Democratic strongholds: average scrutiny rates there are actually lower than in Republican strongholds, largely because Democratic strongholds regulate relatively more minor facilities, which receive far less scrutiny than major ones, and their facilities have had lower violation rates in recent years.

Second, in Democratic strongholds, the effects of out-of-state takeovers are usually positive and reach statistical significance at the 90% confidence level in the second and fifth years. They tend to be larger than the estimates for in-state takeovers but much smaller than those for foreign takeovers. This suggests that information-driven bias likely extends to out-of-state firms rather than only foreign-owned firms, while still leaving a specific bias against foreign ownership.

I make two points about the temporal patterns of the effects in Democratic strongholds. First, the effects grow during the first few post-treatment years. This may reflect the time regulators need to identify external ownership, since each oversees many facilities, scrutiny actions are infrequent, and facilities need not report ownership origin. Second, I estimate effects within five years of treatment because later estimates are less reliable due to temporal carryover and changes in sample composition. With this caution, I extend the analysis to 10 post-treatment years using both the original estimator (Figure D37) and two alternative estimators (Figures D38 and D39). The results suggest that the effects of foreign takeovers grow for roughly the first decade before declining, consistent with my theory's implication of gradual belief updating.

The findings hold for more intensive scrutiny actions, across different national origins of foreign parents, and in several robustness checks. Foreign takeovers increase relatively intensive scrutiny actions—on-site inspections and "Full Compliance Evaluations"—only in Democratic strongholds (Figure D16). The results for Democratic strongholds hold when restricting treated units to takeovers by firms from Western countries, suggesting that they are not driven by geopolitical or nationalistic bias (Figure D17). Appendix D.9 reports additional robustness checks.

Further Mechanism Tests

I extend the design in several ways to further delineate the nature of the observed scrutiny response. I summarize the findings here and detail the tests below. The response is rational and effective, as regulators in Democratic strongholds cite more violations following external takeovers. Still, it is not merely driven by cited violations or observed signals of violation risk from self-reporting, facility expansion, or citizen complaints, suggesting proactive targeting of external ownership. Moreover, this targeting appears aimed at detecting violations rather than harming external firms through political favoritism, as there is no comparable increase in penalties.

I first apply the same DiD design to estimate the effects of takeovers on violations that regulators discover and cite through scrutiny actions. Figure D18 shows that both foreign and out-of-state takeovers immediately increase violation citations in Democratic strongholds. Yet these citations are unlikely to drive the scrutiny response. Regulators do not cite more violations before takeovers, while scrutiny rises immediately in the takeover year. Moreover, the increase in violation citations after foreign takeovers does not persist beyond the first post-treatment year, so it can hardly explain the more persistent increase in scrutiny. In Republican strongholds, foreign takeovers also produce small increases in violation citations despite no corresponding increase in scrutiny.

I next assess whether the observed scrutiny response could be driven by signals of violation risk other than external ownership, beginning with self-reporting. If new external owners immediately self-report more violations after takeovers, this could trigger greater scrutiny in the same year. Figures D19 and D20 report the effects of ownership changes on self-reported violations. Foreign and out-of-state takeovers increase self-reported violations in some years, but these effects appear only later, are inconsistent across years, and are similar, if not larger, in Republican strongholds.

I then assess whether external takeovers, especially foreign ones, expand production and emissions. I first estimate their effects on air program facility class: Major, Synthetic Minor, or Minor (Figures D21 and D22). Neither foreign nor out-of-state takeovers make facilities more likely to upgrade to Major status from Minor or Synthetic Minor. For upgrades from Minor to Synthetic Minor, out-of-state takeovers have a small effect in the first year, while foreign takeovers have a similar effect by the fifth post-treatment year. Figure D23 also shows that foreign takeovers do not increase air emissions. Overall, these findings suggest that the immediate scrutiny effects of foreign and out-of-state takeovers are not driven by mechanical movement to more frequent minimum inspection schedules. I further address this concern with a causal mediation analysis estimating the controlled direct effect of takeovers on scrutiny while holding facility class fixed (Appendix D.8.4).

A related concern is that foreign takeovers could lead to greater expansion than out-of-state takeovers because MNCs tend to be larger and more productive than non-MNCs. Yet takeovers by out-of-state U.S. MNCs still have smaller effects than foreign takeovers in the baseline estimates (Figure D25). Moreover, scrutiny does not increase when out-of-state-owned facilities are acquired by foreign MNCs, as would be expected if foreign MNCs signaled qualitatively higher violation risk than out-of-state U.S. companies (Figure D26).

I also find little evidence that citizen complaints drive the results. Complaints inform but do not automatically trigger scrutiny. My analysis of citizen complaint records in Appendix D.8.6 suggests that citizens may need time to perceive foreign ownership, observe potential violations, and file complaints: complaints decline initially after takeovers and increase only later.

Finally, the scrutiny bias does not reflect an intent to harm externally owned firms or political favoritism. Figures D28 and D29 show no increase in the number or severity of penalties following foreign or out-of-state takeovers in Democratic strongholds. Section 5 further examines the relationship between ownership origin and penalties to identify potential political bias.

4.2 Scrutiny of New Greenfield Facilities

I now use covariate-balancing matching and weighting to compare facilities that were foreign- or out-of-state-owned from their creation with those that were in-state-owned from their creation. I compare facilities that differ in ownership but are similar in observable characteristics relevant to scrutiny decisions, including enforcement history, violation history, and facility attributes such as emissions, size, and industry. These methods restrict or reweight in-state facilities (control units) so that they more closely resemble externally owned facilities (treated units).

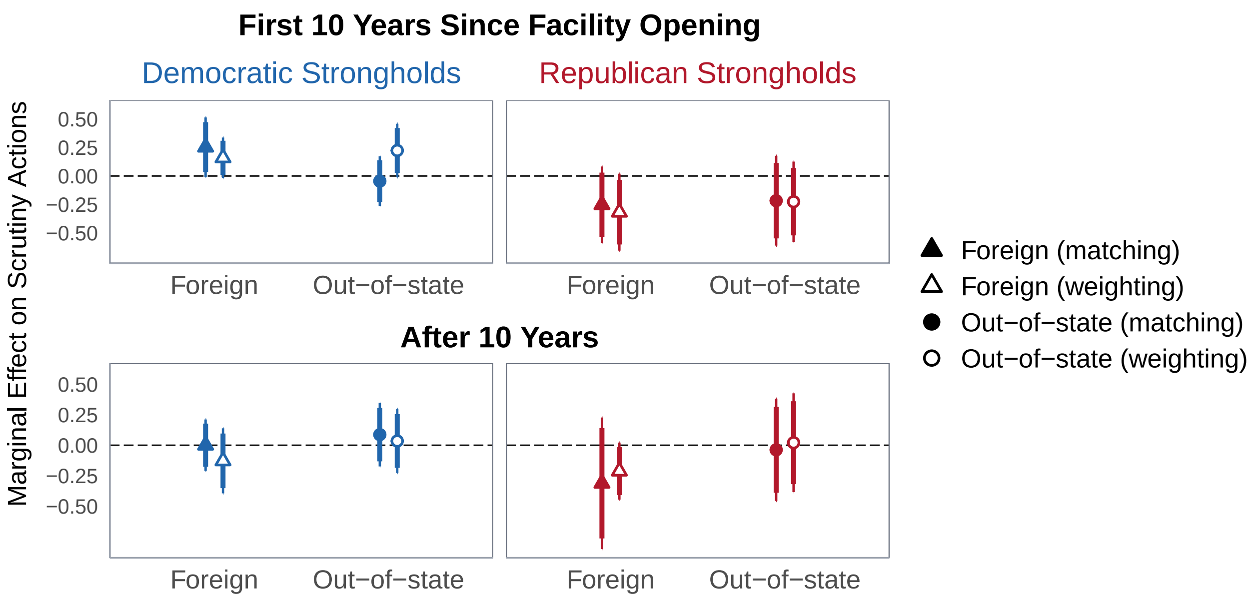
The design has three steps; Appendix E.1 provides details. First, I select eligible control units from in-state facilities in the same state, with the same TRI status and in the same five-year age bracket.[48] Second, I use matching and weighting methods to respectively restrict and reweight eligible control units so that they more closely resemble treated units in industry, facility class, and the other balancing covariates used in the takeover design above. Finally, when estimating ownership effects within these refined samples, the outcome regressions include state-by-year and industry-by-year fixed effects to absorb time-varying state- and industry-level shocks and control for any covariates that remain imbalanced after preprocessing.

I report estimates from both the best-performing matching method and the best-performing

[48] If a facility could be compared with one built a decade earlier, the pair's age gap would enter the estimate at every age, and matched-pair fixed effects cannot absorb a difference that varies within the pair. Appendix E.2 further explains why a five-year age bracket is appropriate: finer brackets are more costly for estimation, while wider ones are too coarse.

weighting method. Choosing only one of the two approaches would involve a genuine trade-off. Weighting retains every control unit, making it more efficient and yielding more precise estimates. However, this design requires exact matches on state, TRI status, and age bracket rather than mere adjustment for these characteristics. Matching can impose this at the pairing stage; weighting has no pairing step and must instead estimate separately within each combination of the required strata, which can be unstable in thin cells. Therefore, I give equal attention to both methods, since agreement between methods that fail in different ways is more informative than either alone.

Figure 6: **Effects of foreign and out-of-state ownership on scrutiny among greenfield facilities.**



Notes: The dependent variable is the number of scrutiny actions per facility-year under the air program (CAA). Filled and hollow markers denote estimates from the preferred matching and weighting estimators, respectively. Thick and thin bars represent 90% and 95% CIs. Appendix E.2 reports the preferred estimators and specifications for each subsample and describes how they are selected based on diagnostics. Table E5 reports the numeric estimates, and Table E6 reports the numeric estimates within each five-year age bracket.

To examine whether scrutiny bias diminishes as facilities age, I estimate ownership effects separately for younger and older facilities. Figure 6 reports matching and weighting estimates using a ten-year cutoff for the air program in Democratic and Republican strongholds. In Democratic strongholds, foreign-owned facilities in their first decade receive roughly 0.25 more scrutiny actions per year than otherwise similar in-state facilities of the same vintage—close to half the base rate—with similar estimates across both methods. Among facilities ten years or older, the effect disappears. Out-of-state ownership shows a positive early effect under weighting, though it falls short of conventional significance and is not reproduced by matching. In Republican strongholds, neither ownership type shows an effect. If anything, foreign-owned greenfield facilities receive less scrutiny during their early years. Table E8 shows no comparable ownership effects on penalties.

The findings so far provide evidence of bureaucratic information-driven bias in scrutiny targeting: strong bias against foreign investors in Democratic strongholds under the air program, extending more weakly to out-of-state investors. The political context in which this bias arises and the mechanism tests support the interpretation that it reflects bureaucratic information-driven discrimination.

Both designs above also suggest that scrutiny bias does not persist into the penalty stage, as foreign ownership has no effect on the annual number or amount of penalties in either type of state. Below, I further examine penalty size for each violation case. Finding no bias in penalty size against externally owned facilities in Democratic strongholds would further support the interpretation that the preceding findings reflect bureaucratic information-driven bias rather than political favoritism. In Republican strongholds, although I find no ownership bias in scrutiny actions, political favoritism may still appear in penalty size because penalties are more likely to be handled by politically appointed senior regulators and subject to political intervention.

5 Bias in Penalties

I conduct two analyses to examine direct and indirect bias against externally owned firms in penalties. I first examine whether violations by foreign- and out-of-state-owned facilities receive larger penalties than similar violations by in-state-owned facilities. I then examine a specific form of political favoritism toward campaign donors: whether firms that donated to state candidates receive lower penalties for similar violations than non-donors, and whether foreign-owned firms are less able to use donations to secure reduced penalties. If so, this would indicate indirect political bias against foreign investors through more limited political access. To my knowledge, no existing study examines state-level political influence by foreign-owned firms in the U.S.; prior work focuses on publicly listed foreign firms' donations to federal politicians (Lee, 2023, 2024).

Identifying bias in penalties requires conditioning on violation severity. I leverage the USEPA's CAA Pipeline dataset, the first dataset that explicitly links violation citations to penalties.[49] The dataset covers the universe of 55,655 High Priority Violations (HPVs) and Federally Reportable Violations (FRVs) since 2015. HPVs are the most serious violations under the CAA; FRVs include HPVs as well as other serious violations that are less severe than HPVs. Much discretion remains in imposing monetary penalties for these serious violations: only about half received one.

5.1 Comparison of Violation Cases

The analysis operates at the violation-case level and extends the covariate-balancing matching and weighting design used above for facilities with time-invariant ownership. I match each case to the facility panel by the year in which the violation was identified, so each case carries its facility's characteristics in that year. I therefore do not distinguish between facilities that were foreign-owned from the outset and those that became foreign-owned through takeovers. Matching/weighting preprocessing then compares violation cases with similar observable violation and facility characteristics but different ownership types: (1) violations by foreign-owned versus in-state facilities, and (2) violations by out-of-state-owned versus in-state facilities.

The design framework follows the preceding section but uses different strata and balancing covariates. Control units must come from the same state; I impose no additional strata given the smaller sample size. The balancing covariates include the same facility-level covariates as in the previous design, along with violation-level variables from the Pipeline dataset capturing the nature and severity of the violation.[50] I select the preferred matching and weighting specifications for each comparison based on covariate balance and overlap (Appendix F.3). Finally, I estimate OLS outcome regressions with industry-by-year and state-by-year fixed effects.

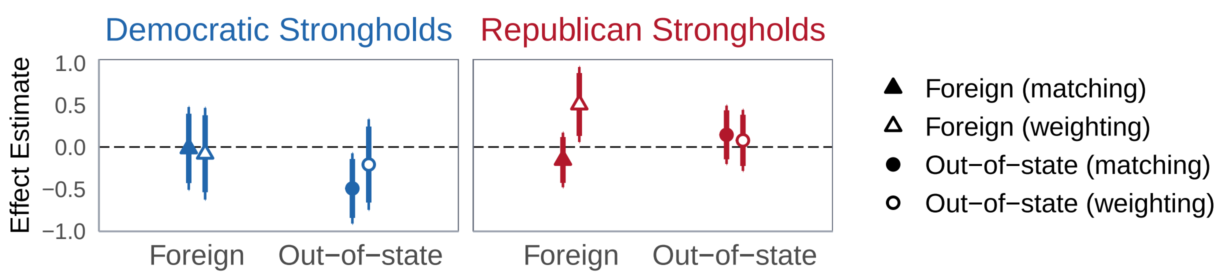
Figure 7 reports the effects of foreign and out-of-state ownership on the log value of monetary penalties in Democratic and Republican strongholds. In Democratic strongholds, foreign ownership has no effect on penalty size under either matching or weighting; out-of-state owner-

[49] In the USEPA data, violations and enforcement actions are not linked, making it infeasible to use the full USEPA records of violations and enforcement actions. See Appendix F.1 for details on the Pipeline dataset.

[50] Appendix F.1 describes the Pipeline variables and sample construction.

ship predicts smaller penalties under matching, though the weighting estimate is indistinguishable from zero. In Republican strongholds, the two methods diverge for foreign ownership—null under matching but positive and significant under weighting—while out-of-state ownership has no effect under either. Taken together, the two methods provide no consistent evidence that externally owned facilities receive larger penalties in either group of states.

Figure 7: **Effect of external ownership on log value of penalty by violation case.**



Notes: Outcome is the log monetary penalty per violation case under the air program (CAA). Filled and hollow markers denote estimates from the preferred matching and weighting estimators, respectively. Appendix F.3.1 describes how the preferred estimators and specifications are selected based on diagnostics. Table F5 reports numeric estimates. Figure F5 uses a binary outcome of receiving any monetary penalty. Thick and thin bars represent 90% and 95% CIs.

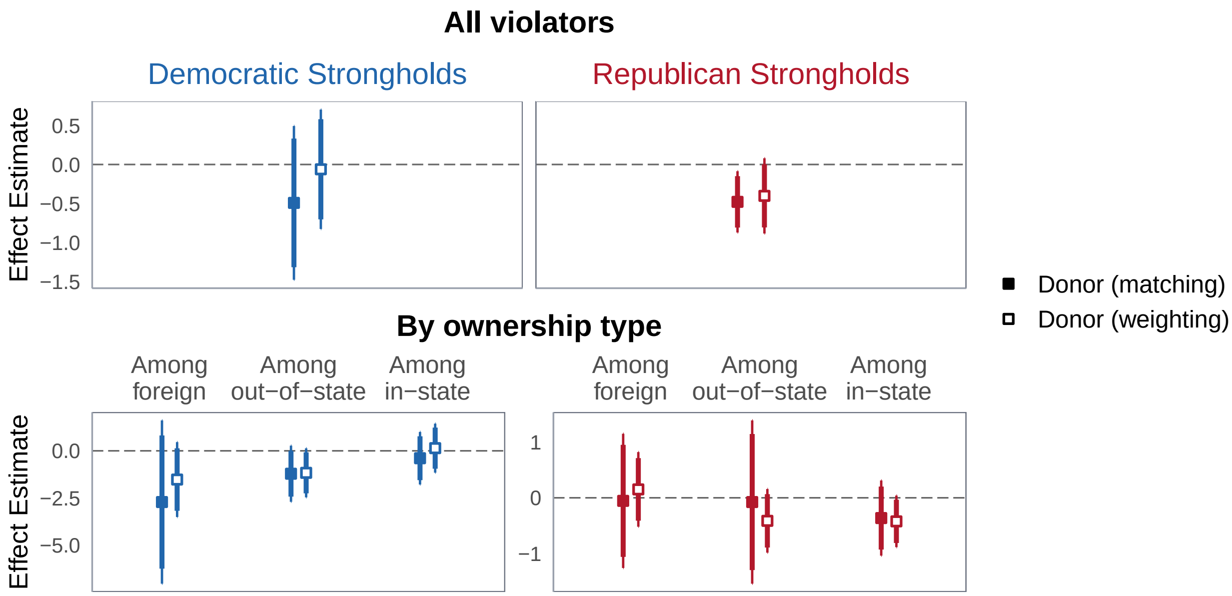
5.2 Securing Protection via Campaign Donations

I use the Database on Ideology, Money in Politics, and Elections (DIME) for data on campaign contributions to state-level candidates (Bonica, 2016). Appendix F.2 provides details on the data. I again apply the covariate-balancing matching and weighting procedures described above. The treatment is whether the parent firm owning the violating facility made any donation to candidates of the facility's state in the five years before the violation was identified.

Figure 8 reports the main estimates for log monetary penalties. The first row pools all violators within each group of states. A pre-violation donation predicts lower penalties in Republican strongholds but not in Democratic strongholds, where the estimates are smaller and indistinguishable from zero. The presence of political bias in penalty decisions is itself informative because it shows that the null results in Figure 7 cannot be attributed to a test too weak to detect such bias.

The lower row splits each sample by the violating facility's ownership type. In Democratic strongholds, the disaggregated pattern differs from the pooled null: donations have sizable negative estimates for foreign- and out-of-state-owned plants, with the latter more precisely estimated, but none for in-state firms. External firms therefore appear to have a greater donor advantage than in-state firms in Democratic strongholds. If this pattern reflected the same mechanism as the scrutiny bias, the ordering would run in the opposite direction. In Republican strongholds, the donation discount is concentrated among in-state firms and, more weakly, out-of-state firms, while foreign-owned firms receive no discount. This suggests that foreign-owned firms may face a disadvantage in obtaining political access and favors.

Figure 8: **Effect of pre-violation campaign donation on log value of penalty by violation case.**



Notes: Treatment: whether the owner firm donated to state candidates in the five years before the violation. Outcome: log monetary penalty per violation case under the air program (CAA). Filled and hollow markers denote estimates from the preferred matching and weighting estimators, respectively. Appendix F.3.1 describes how the preferred estimators and specifications are selected based on diagnostics. Table F6 reports numeric estimates. Figure F7 uses a binary outcome of receiving any monetary penalty. Thick and thin bars represent 90% and 95% CIs.

To summarize, I find strong bias in scrutiny targeting against foreign investors and, to a lesser extent, out-of-state investors in Democratic strongholds under the air program. The political context, mechanism tests, and absence of similar penalty bias in Democratic strongholds suggest that this pattern reflects bureaucratic information-driven discrimination rather than political favoritism or other dynamics. In Republican strongholds, scrutiny does not discriminate by ownership origin, but political favoritism appears in penalties toward domestic firms, especially in-state firms that have donated to state politicians.

6 Insights from Regulator Interviews

I conduct interviews with state environmental regulators to corroborate these findings and provide direct evidence on regulators' beliefs and other mechanisms. The interviews offer first-hand accounts of whether regulators believe externally owned facilities face greater violation risk, whether they face pressure from state politicians to avoid scrutinizing new investors or reduce penalties for campaign donors, and whether they can resist these pressures.

I completed 27 semi-structured interviews with 31 current and former leaders and senior managers at state environmental agencies across 21 states; participants outnumber interviews because several officials chose to be interviewed jointly. Respondents include nine agency heads and 22 program or district directors, identified through public agency directories and recruited by direct outreach, with voluntary participation (Appendix B). These interviews are not a probability sample of state regulators, and I do not use them to estimate how widespread any practice is. Their role is validation: they examine whether the mechanisms inferred from the administrative data correspond to regulators' own accounts of how they select scrutiny targets and assess penalties. Because officials who agree to discuss enforcement may differ from those who decline, and because respondents may underreport practices they consider improper, I place the most weight on statements against the speaker's apparent interest—such as admissions of political pressure on penalties—and on the convergence between interview accounts and the quantitative patterns, rather than on any single account.

The belief that external ownership signals greater violation risk is shared by regulators in Democratic strongholds, as well as some in swing states and Republican strongholds. Of the 31 participants, 18 began their careers as staff directly responsible for inspections and other scrutiny actions,[51] while the remaining participants have held only policy or leadership roles and could answer questions about penalties and political influence, but not about the details of scrutiny actions. Among the 18 regulators, four are from Democratic strongholds, five from swing states, and nine

[51] Some of these 18 participants have continued to conduct inspections as program or district leaders.

from Republican strongholds. All four from Democratic strongholds, three of the five from swing states, and three of the nine from Republican strongholds agreed that external ownership signals greater violation risk. These regulators also agreed that their states have more stringent regulations, either across all three programs or in specific areas.

Regulators appear to have developed this belief over time through observing new external investors' compliance challenges. They generally attribute these difficulties to a lack of understanding: external managers often assume that rules and abatement technologies established in one jurisdiction also apply in another. They also note that the multi-layered management structure of multinational and multi-state companies creates conflicts between headquarters' demands and local compliance requirements.[52]

Would hiring local environmental consultants or managers solve these difficulties for external investors? When encountering new external investors, regulators routinely recommend that they hire local environmental consultants and lawyers, but many investors do not follow this advice.[53] Many foreign companies operating in the Midwest, South, and West instead rely on consulting and law firms based in major cities on the East or West Coast, partly because local firms are often smaller and less well known.[54] Yet consultants and lawyers from other states may likewise lack familiarity with state-specific regulations.[55] Moreover, although multinational and multi-state companies are often large overall, subsidiaries in a given host jurisdiction may be too small to justify hiring external consultants or lawyers.[56]

Regulators recognize that many firms eventually adapt and improve, but that adaptation takes

[52]One regulator noted that local employees generally manage routine operations, but foreign headquarters may intervene during enforcement matters; in several cases, local staff understood the appropriate compliance response, yet headquarters refused to approve it (Interview A10). Another regulator from a different state noted that a firm's senior leadership and training systems were built around federal rules and overlooked state-specific requirements, and that resolving enforcement cases required communication between local managers and headquarters (Interview A14).

[53]Interviews A1, A3, A4, A9, A11, A17, A19, A26, A27.

[54]The local environmental consulting firms are often private practices run by former regulators.

[55]Interviews A3, A4, A9, A10, A26. As an example, one regulator from a Northeastern state noted that foreign companies often relied on consultants based in Boston who nonetheless had limited understanding of the state's unique regulations (Interview A3). Similarly, a regulator in a Midwestern state reported that international companies there usually relied on consultants from Chicago who had limited understanding of the state's regulations (Interview A26).

[56]Interviews A11, A27. As an extreme example, one regulator mentioned a small Japanese-owned plant in their district that had only three employees.

time and some firms deliberately choose not to comply.[57] When asked whether external investors could adapt over time, one regulator immediately pointed to a major Japanese automaker's facility in their district: *They came in shortly before I started this job, and they had a lot of trouble in those early years; now they have been here for forty years, so they are* "so Americanized" *and no different from nearby American companies* (Interview A1). Another regulator described a counterexample in which an out-of-state company refused to adapt, insisting on applying its home-state practices across all of its locations (Interview A14).

Whether the belief that external ownership signals greater violation risk translates into proactive targeting depends on state politics. Although the belief is found across all three types of states, it leads to proactive scrutiny only in Democratic strongholds and a few swing states. Each of the four respondents with inspection experience in Democratic strongholds described proactive scrutiny, including on-site inspections, after foreign takeovers and for greenfield facilities established by external investors. Respondents from two swing states also report such practices.[58] By contrast, all nine inspectors from Republican strongholds and most of the five from swing states report that ownership origin does not enter scrutiny decisions. Several explicitly reject using ownership origin as a factor, saying that it would constitute "bias," "targeting," or "unfair" treatment.[59]

Pro-business Republican governors encourage agencies to build friendly relationships with new external investors by avoiding extensive scrutiny and/or providing additional assistance. In one Republican stronghold, interviewees report that their agency supports the governor's pro-business agenda by providing services, rather than enforcement, to new investors. They recognize that new external investors tend to have difficulty understanding and adapting to state regulations. Rather than conducting inspections or other scrutiny activities that could lead to enforcement, they offer various forms of assistance to help these investors identify and address violations.[60] Respondents from three other Republican strongholds report similar practices of providing additional assistance

[57] Interviews A9, A10, A12, A14, A19, A26, A27.

[58] Interviews A3 and A16. According to A16, additional scrutiny in their state is limited to the first year of operation.

[59] Interviews A8, A13, A15.

[60] Interviews A13, A19.

rather than scrutiny.[61] In another Republican stronghold, an interviewee explains that whenever an external company has been actively recruited, the agency is aware of the consequences of pursuing enforcement aggressively and that the Governor's Office has sometimes told it to "back off."[62]

Interviewees suggest that politicians' intervention on behalf of campaign donors is plausible, particularly in Republican strongholds. Politicians often express interest in specific penalty cases through inquiries on behalf of businesses in their constituencies.[63] Two senior agency managers in Republican states report that state leadership has explicitly instructed them to reduce penalties for companies that are important campaign donors, all of which are domestic-owned companies.[64]

Yet bureaucratic discretion can curb such political favoritism in two ways. First, inspectors still largely adhere to the minimum inspection frequencies recommended under states' agreements with the USEPA.[65] This is probably why I find no scrutiny bias in favor of domestic investors in Republican strongholds. Second, bureaucratic discretion in penalty decisions can also help. All regulators report that they never consider ownership origin or campaign contributions when proposing initial penalty assessments under state guidelines. Some states with larger environmental agencies, including Republican strongholds, require collective deliberation by senior regulators, making political interference less likely.[66] Several interviewees, including those from Republican states, are confident that even when politicians inquire about penalty cases, regulators either decline to respond or ensure that penalty decisions do not reflect political pressure.[67]

7 Conclusion

This paper advances bureaucratic information-driven bias as a novel source of discrimination against, and risk to, foreign investors. This bias is distinct from politicians' favoritism toward

[61] Interviews A5, A7, A9, A11.

[62] Interview A22.

[63] Interviews A16, A25.

[64] Interviews A15, A22.

[65] Interview A22. The regulator says that, although state leadership tells them not to pursue enforcement aggressively against external investors, they still conduct the minimum inspections; they avoid additional inspections and offer reduced penalties. Politicians know that forgoing the minimum inspections would invite USEPA intervention.

[66] Interviews A2, A7, A8, A12, A17, A19, A26.

[67] Interviews A7, A12, A16, A17, A19, A24, A27. Multiple interviewees note that directly elected board members are intentionally kept out of the penalty decision-making process (Interviews A17, A24).

domestic-owned firms and from the two conventional forms of political risk through which investor-state relations have been analyzed: expropriation and contract breaches. This theoretical innovation reveals bias against foreign MNCs in Global North host countries and helps explain rising global disputes over host governments' domestic regulation, including alleged protectionist non-tariff measures and violations of the "fair and equitable treatment" and "national treatment" principles.

This paper suggests new approaches to analyzing investor-state relations and, more broadly, identifying and explaining biased government treatment of different firms. First, bureaucrats deserve greater attention as key actors. Existing studies of investor-state relations tend to treat host governments as unitary actors. Yet bureaucrats' incentives differ from politicians' and matter especially where bureaucratic autonomy is greater.

Second, it is important to compare foreign-owned firms with domestic-owned ones—treating discrimination as the dependent variable—and to make the appropriate comparison. Studies of investor-state relations have long focused on host governments' adverse treatment of foreign investors' property rights in isolation (Wellhausen, 2021). Analyzing discrimination reveals aspects of investor-state relations that this traditional approach cannot explain. Moreover, I benchmark foreign-owned firms against domestic non-local firms to identify nationality-based favoritism or protectionism. Because subnational government actors may categorize foreign-owned and domestic non-local firms as a single group, identifying protectionism in subnational settings requires assessing not simply whether governments treat foreign-owned and domestic-owned firms differently, but whether they treat foreign-owned firms differently from domestic non-local ones. Several studies examine protectionism in subnational government behavior (Kim, 2018; Chen and Xu, 2023) but have yet to make this comparison.

Finally, in regulatory enforcement, the intent behind biased treatment can be inferred from patterns of bias across enforcement stages and over time. A central challenge in research on discrimination, in regulatory and non-regulatory settings alike, is inferring the nature of discrimination from observed disparities (Bohren et al., 2019; Carpenter, 2004). I distinguish statistical from animus-based discrimination by exploiting differences between the scrutiny and penalty stages and the evolution of scrutiny over time—an approach that may prove useful in other settings. Taken together, this study offers an analytical framework for identifying and explaining biased government treatment of different firms.

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Appendix for *Bias Against Mobile Capital*

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A Appendix A: A Model of Bureaucratic Information-Driven Discrimination

High pollution persists in many developing and middle-income countries despite strict environmental standards. A central reason, documented by Duflo et al. (2018) in a field experiment with the Gujarat Pollution Control Board (GPCB), is that regulatory enforcement is both resource-constrained and informationally limited. Regulators cannot inspect every plant; they must use imperfect signals to target their inspection budget toward likely violators. The key finding is that discretion is valuable: inspections chosen by the regulator induce roughly three times more abatement than the same number of randomly assigned inspections, precisely because the regulator's signal—noisy as it is—concentrates effort on dirtier plants.

This appendix asks what happens when a new foreign-owned plant enters the jurisdiction. At the moment of acquisition or greenfield entry, the inspector has no plant-specific compliance history. In the absence of firm-specific information, the inspector may rely on ownership origin as an unofficial group-level proxy for violation risk. My goal is to understand, within the Duflo et al. (2018) framework, (i) when and how strongly the regulator should optimally use ownership origin as a targeting input, and (ii) how the equilibrium targeting rule and plant abatement rule co-evolve over time as the firm builds a compliance record.

I obtain two main theoretical results:

Proposition 1 (Information substitution). I extend the static targeting stage to allow the regulator to use both the observed pollution shock u_{1j} and ownership origin $F_j \in \{0, 1\}$. The optimal weight on F_j in the targeting rule equals $(1 - \rho_I)\mu_F$, where $\rho_I = \sigma_1^2/(\sigma_1^2 + \sigma_2^2) \in (0, 1)$ is the information quality—the share of pollution variance the regulator observes—and μ_F is the mean pollution differential between foreign and domestic plants. The weight is strictly decreasing in ρ_I: poor information raises the value of the foreignness proxy as a substitute signal. With perfect information ($\rho_I \to 1$) the optimal weight is zero; with no usable pollution signal ($\rho_I \to 0$) foreignness becomes the sole available input.

Proposition 2 (Equilibrium path convergence). In the dynamic extension, the regulator maintains a Bayesian belief π_{jt} about plant j's latent type and updates it after every inspection. The plant's abatement decision follows a threshold rule: run equipment if and only if $\pi_{jt} \geq \pi^*$. For an adapting low-risk foreign firm ($\kappa_j = A$, $\theta_j = L$), the belief path $\{\pi_{jt}\}$ is a supermartingale and converges almost surely to zero (full identification as a low type), at a geometric rate determined by the type separability $q_H - q_L$ and the belief-to-targeting sensitivity γ_π. For a non-adapting high-risk firm ($\kappa_j = N$, $\theta_j = H$), the belief path is a submartingale converging almost surely to full identification ($\pi^\infty = 1$). Inspection intensity I_{jt} and abatement effort Run^*_{jt} co-evolve monotonically along both paths.

The remainder of this appendix is organized as follows. Appendix A.1 reviews the Duflo et al. (2018) model. Appendix A.2 develops the static model and proves Proposition 1. Appendix A.3 develops the dynamic model—types, beliefs, and the plant's compliance decision. Appendix A.4 presents the Monte Carlo simulations. The proof of Proposition 2 and the equilibrium characterization are not included here due to limited space and are available upon request.

A.1 Baseline Model: Duflo et al. (2018)

I briefly present the elements of Duflo et al. (2018) that I build on, using their notation throughout.

A.1.1 Pollution equation and abatement

Plant j's latent log pollution is:

$$\log \tilde{P}_j = \phi_0 + \phi_1 X_j + u_{1j} + u_{2j} \tag{1}$$

where X_j are observable plant characteristics, $u_{1j} \sim N(0, \sigma_1^2)$ is a pollution shock known to both the plant and the regulator, and $u_{2j} \sim N(0, \sigma_2^2)$ is a plant-only shock. Running abatement equipment reduces pollution proportionally:

$$\log P_j = \log \tilde{P}_j + \phi_2 \cdot \mathrm{Run}_j, \qquad \phi_2 < 0. \tag{2}$$

Plants run equipment when the reduction in expected regulatory penalties exceeds the maintenance cost c_j, where $\log c_j \sim N(\mu_c, \sigma_c^2)$.

A.1.2 Static targeting rule

The regulator sets a targeting rule before observing u_{1j}, then assigns inspections after observing the signal. Its probit link form is:

$$I(u_{1j} \mid X_j, T_j, \lambda, \beta, \rho) = \lambda_2 \, \Phi\left(\frac{\lambda_1 + X_j'\beta_1 + T_j'\beta_2 + u_{1j}}{\rho}\right) \tag{3}$$

The regulator maximizes total abatement subject to an inspection budget $\sum_j \mathbb{E}[I(u_{1j})] = N\bar{I}$. The key finding of Duflo et al. (2018) is that $\sigma_1^2/(\sigma_1^2 + \sigma_2^2) \approx 0.004$: the observed signal captures less than one percent of pollution variance, yet aggressive targeting on this noisy signal still dominates random assignment because the regulator concentrates inspections on the extreme tail.

A.1.3 Penalty stage

After an initial inspection finds pollution reading p_{j1}, the plant and a *regulatory machine* alternate moves: the machine chooses among Inspect, Warn, Punish, and Accept; the plant chooses between Comply (pay capital cost k) and Ignore. The machine's action probabilities are estimated as a policy function and held fixed. Backward induction over this finite-horizon game yields $V_0(p)$: the expected discounted regulatory cost to the plant of an initial inspection finding pollution p. This function is taken as given in both the static and dynamic targeting stages below.

A.2 Extension: Ownership Origin as Signal

The extension has two parts, which express the same idea—foreign plants are riskier on average—in two complementary ways, and which are used independently. This section develops the *static* part in the continuous-shock representation of (1): the foreign premium μ_F shifts mean total pollution, information quality ρ_I is the share of pollution variance the regulator observes, and ownership origin is valuable at the targeting stage precisely because it proxies for the *unobserved* component u_{2j} (Proposition 1). The *dynamic* part—in which a plant's risk is a discrete latent type that the regulator learns about through repeated inspections—is developed in Appendix A.3.

A.2.1 Information quality

I keep the Duflo et al. observation structure exactly: the regulator observes the pollution shock u_{1j} directly but not the plant-only shock u_{2j}. The *information quality* is the observed share of total pollution variance,

$$\rho_I \equiv \frac{\sigma_1^2}{\sigma_1^2 + \sigma_2^2} \in (0, 1), \tag{4}$$

the fraction of the latent pollution shock the regulator can see. In Duflo et al. this is tiny ($\rho_I \approx 0.004$): the observed component explains under one percent of pollution variance, and u_{2j} is the large unobserved remainder. As $\rho_I \to 1$ the regulator effectively observes all pollution; as $\rho_I \to 0$ it observes almost none. For comparative statics I vary ρ_I holding the total shock variance $\sigma_1^2 + \sigma_2^2$ fixed, so that changing ρ_I shifts only the observed *share* of pollution, not its overall distribution.

A.2.2 Ownership origin as a group-level signal

Ownership origin $F_j \in \{0, 1\}$ (foreign) is fully observable to the regulator. Foreign plants are dirtier on average:

$$\mu_F \equiv \mathbb{E}[u_{1j} + u_{2j} \mid F_j = 1] - \mathbb{E}[u_{1j} + u_{2j} \mid F_j = 0] > 0 \tag{5}$$

is the foreign premium in *total* latent pollution. I assume the premium loads on the observed and unobserved shocks in proportion to their variances:

$$\mathbb{E}[u_{1j} \mid F_j] = \rho_I\, \mu_F F_j, \qquad \mathbb{E}[u_{2j} \mid F_j] = (1-\rho_I)\, \mu_F F_j. \tag{6}$$

The key point is that foreignness carries information the regulator's routine signal does *not*. The regulator already sees the observed share $\rho_I \mu_F$ through u_{1j}; the unobserved share $(1-\rho_I)\mu_F$—the part of the premium hidden in u_{2j}—is visible only through F_j. The premium μ_F is treated as known (estimated from accumulated inspection data).

A.2.3 Extended targeting rule

The regulator targets on the observed shock u_{1j} and ownership origin F_j:

$$I(u_{1j}, F_j \mid X_j, \lambda, \beta, \gamma, \rho) = \lambda_2\, \Phi\!\left(\frac{\lambda_1 + X_j'\beta + \gamma F_j + u_{1j}}{\rho}\right) \tag{7}$$

where $\gamma \geq 0$ is the weight on ownership origin and ρ is the probit scale. The Duflo et al. baseline (3) is nested at $\gamma = 0$.

A.2.4 Proposition 1: information substitution

I work in the static targeting stage, suppressing the time subscript. The regulator observes (u_{1j}, F_j, X_j) and chooses the weight γ on ownership origin to maximize total expected abatement subject to the inspection budget.

Proposition 1 (Information substitution)**.** *Let $\mu_F > 0$ and $\rho_I \in (0,1)$. The optimal weight on ownership origin in the targeting rule is:*

$$\gamma^*(\rho_I) = (1-\rho_I)\, \mu_F \tag{8}$$

In particular:

(i) γ^* is strictly decreasing in ρ_I: $d\gamma^*/d\rho_I = -\mu_F < 0$.

(ii) $\lim_{\rho_I \to 1} \gamma^* = 0$: with perfect information quality, ownership origin carries zero targeting weight.

(iii) $\lim_{\rho_I \to 0} \gamma^* = \mu_F$: with no usable signal, foreignness becomes the sole available proxy.

(iv) γ^* is strictly increasing in μ_F: a larger mean pollution differential raises the value of the foreignness signal at every information level.

Corollary 1 (Value of the foreignness signal)**.** *The increment in abatement from adding F_j to a targeting rule that uses u_{1j} alone is strictly positive for all $\rho_I < 1$ and $\mu_F > 0$, and equals zero if and only if $\rho_I = 1$.*

A.2.5 Proof of Proposition 1

Proof. **Step 1: The regulator's pollution estimate.** The regulator observes u_{1j} and F_j but not u_{2j}. Violation, and hence abatement, depends on total latent pollution through $\omega_j \equiv u_{1j} + u_{2j}$, so the regulator targets on its conditional expectation. Because $u_{2j} \perp u_{1j}$ given F_j, the split (6) gives

$$\mathbb{E}[\omega_j \mid u_{1j}, F_j] = u_{1j} + \mathbb{E}[u_{2j} \mid F_j] = u_{1j} + (1-\rho_I)\, \mu_F F_j \;\equiv\; m_j. \tag{9}$$

The regulator already sees the observed share of the premium inside u_{1j} (whose mean is $\rho_I \mu_F F_j$); F_j supplies the remaining, unobserved share $(1-\rho_I)\mu_F$.

Step 2: Sufficient statistic for targeting. The regulator maximizes expected abatement $\mathbb{E}[I_j\,\Delta_j]$ subject to the budget $\mathbb{E}[I_j] = \bar{I}$, where $\Delta_j \geq 0$ is the abatement achieved if plant j is inspected and runs. Given (u_{1j}, F_j), the distribution of ω_j (and hence of Δ_j) depends on the observables only through its conditional mean m_j in (9): the residual $\omega_j - m_j = u_{2j} - \mathbb{E}[u_{2j} \mid F_j]$ has fixed variance σ_2^2. So m_j is the sufficient statistic for expected abatement, $\mathbb{E}[\Delta_j \mid u_{1j}, F_j] = \tilde{\Delta}(m_j, X_j)$, which is strictly increasing in m_j.

Step 3: Optimal targeting rule. The regulator therefore targets on m_j. Writing the probit rule (7) with argument m_j,

$$I^* = \lambda_2\,\Phi\left(\frac{\lambda_1 + X_j'\beta + u_{1j} + (1-\rho_I)\mu_F F_j}{\rho}\right), \tag{10}$$

and comparing with the general form (7), the optimal weight on F_j is

$$\gamma^* = (1-\rho_I)\,\mu_F. \tag{11}$$

The observed shock u_{1j} enters with coefficient one—it is seen exactly—so the only quantity the regulator chooses is the weight on the proxy F_j.

Step 4: Parts (i)–(iv). Differentiating, $d\gamma^*/d\rho_I = -\mu_F < 0$ (part (i)); parts (ii) and (iii) are the limits of (11) as $\rho_I \to 1$ and $\rho_I \to 0$; and $\partial\gamma^*/\partial\mu_F = 1-\rho_I > 0$ for $\rho_I < 1$ (part (iv)). □

Proof of Corollary 1. At $\gamma = 0$ the regulator targets on u_{1j} alone, ignoring the unobserved share of the premium. Setting $\gamma = \gamma^* = (1-\rho_I)\mu_F$ moves the targeting argument to the sufficient statistic m_j of (9), the best available predictor of ω_j; expected abatement is then weakly higher, and strictly higher whenever the omitted term $(1-\rho_I)\mu_F$ is nonzero, i.e. for $\rho_I < 1$ and $\mu_F > 0$. At $\rho_I = 1$ the unobserved channel vanishes, $\gamma^* = 0$, and the increment is exactly zero. □

Remark 1 (Connection to Duflo et al. Appendix B). Duflo et al. (2018) show that the optimal targeting function is *steeper* (more aggressive) when the regulator's information quality is lower (their Figure B2, Panel A vs. Panels B–C). Proposition 1 provides the complementary result for group-based signals: the optimal weight on a categorical group proxy F_j is also increasing in information limitation. Both results reflect the same principle: scarcer information makes every available signal, whether continuous or discrete, more valuable as a targeting input.

A.3 Extension: Temporal Dynamics

I now turn to the dynamic part of the extension. A plant's risk is collapsed into a discrete latent *type* with type-specific violation rates; the regulator does not observe the type but learns about it from inspection outcomes, and foreignness enters only through the regulator's prior belief. This section sets out the ingredients—types, violation probabilities, beliefs—and derives the plant's compliance decision; the resulting equilibrium paths (Proposition 2) are illustrated in the simulations below, with the formal characterization omitted for space. The simulations follow the same split: Simulations 1 and 3 use the static model, Simulation 2 the dynamic one. The penalty stage is unchanged from Duflo et al. (2018): it produces $V_0(p)$, the expected discounted regulatory cost to the plant of an inspection finding pollution reading p, increasing in p; I take $V_0(\cdot)$ as a primitive.

A.3.1 Latent types

Each plant j has two fixed latent characteristics, known to the plant but not the regulator.

- *Violation type* $\theta_j \in \{H, L\}$. High-type plants draw u_{1j} from a distribution with higher mean $\mu_H > \mu_L = 0$, so they are more likely to violate the standard.
- *Adaptability type* $\kappa_j \in \{A, N\}$. Adapting (A) plants have finite maintenance cost $c_j < \infty$ and choose optimally whether to run equipment. Non-adapting (N) plants face effectively infinite cost at the margin and always set $\text{Run}_{jt} = 0$.

A.3.2 Violation probability

When plant j is inspected, the violation indicator is $y_{jt} = \mathbf{1}\{P_{jt} > \bar{P}\}$. From (1)–(2):

$$p(\text{Run}, u_{1j}) = \mathbb{P}(y_{jt} = 1 \mid \text{Run}, u_{1j}) = 1 - \Phi\left(\frac{\log \bar{P} - \phi_0 - \phi_1 X_j - u_{1j} - \phi_2 \cdot \text{Run}}{\sigma_2}\right) \quad (12)$$

Since $\phi_2 < 0$, running equipment raises the threshold and lowers the violation probability: $p(1, u_{1j}) < p(0, u_{1j})$ for all u_{1j}. For non-adapting firms $p(0, u_{1j})$ is the violation probability regardless of scrutiny.

A.3.3 Regulator's belief and Bayesian updating

The regulator maintains a belief $\pi_{jt} = \mathbb{P}(\theta_j = H \mid \mathcal{H}_{jt}) \in [0, 1]$ about plant j's type, where $\mathcal{H}_{jt}$ is the history of inspection outcomes up to t. For a new plant with no inspection history, the prior is:

$$\pi_{j0} = \mathbb{P}(\theta_j = H \mid X_j, F_j) = \frac{\exp(\alpha_0 + \alpha_1 X_j + \alpha_F F_j)}{1 + \exp(\alpha_0 + \alpha_1 X_j + \alpha_F F_j)}, \qquad \alpha_F > 0 \quad (13)$$

This is the standard logistic (logit) link: it posits that the *log-odds* of being a high type are linear in observables, $\log[\pi_{j0}/(1 - \pi_{j0})] = \alpha_0 + \alpha_1 X_j + \alpha_F F_j$, and (13) simply inverts that linear index back into a probability in $(0, 1)$ (the single exponential $\exp(\cdot)$ appears in both the numerator and the denominator). Thus α_0 is the baseline log-odds of a domestic plant and $\alpha_F > 0$ is the foreign premium in log-odds, so foreign plants start with a higher prior, $\pi_{j0} \mid_{F=1} > \pi^D \equiv \pi_{j0} \mid_{F=0}$.

After each inspection yielding outcome $y_{jt} \in \{0, 1\}$, the belief updates by Bayes' rule:

$$\pi_{j,t+1} = B(\pi_{jt}, y_{jt}) = \frac{\pi_{jt}\, q_H^{y_{jt}} (1 - q_H)^{1 - y_{jt}}}{\pi_{jt}\, q_H^{y_{jt}} (1 - q_H)^{1 - y_{jt}} + (1 - \pi_{jt})\, q_L^{y_{jt}} (1 - q_L)^{1 - y_{jt}}} \quad (14)$$

where $q_H = \mathbb{P}(y = 1 \mid \theta = H) > q_L = \mathbb{P}(y = 1 \mid \theta = L)$ are the type-specific population violation rates. The compact factor $q^y (1 - q)^{1-y}$ is the Bernoulli likelihood of the outcome—it equals q when $y = 1$ (a violation) and $1 - q$ when $y = 0$ (a clean inspection). The operator B is just Bayes' rule: the numerator is the prior π_{jt} times the likelihood of y under $\theta = H$, and the denominator is the total probability of y. Spelling out the two cases,

$$B(\pi, 1) = \frac{\pi\, q_H}{\pi\, q_H + (1 - \pi)\, q_L} \qquad \text{(violation)},$$

$$B(\pi, 0) = \frac{\pi\, (1 - q_H)}{\pi\, (1 - q_H) + (1 - \pi)\, (1 - q_L)} \qquad \text{(clean inspection)}.$$

Since $q_H > q_L$, a violation raises the belief ($B(\pi, 1) > \pi$) and a clean inspection lowers it ($B(\pi, 0) < \pi$). When no inspection occurs, $\pi_{j,t+1} = \pi_{jt}$.

Log-odds representation. Let $\ell_{jt} = \log[\pi_{jt}/(1 - \pi_{jt})]$. Update (14) is additive in log-odds:

$$\ell_{j,t+1} = \ell_{jt} + y_{jt} \underbrace{\log \frac{q_H}{q_L}}_{\mathrm{LR}_1 > 0} + (1 - y_{jt}) \underbrace{\log \frac{1 - q_H}{1 - q_L}}_{\mathrm{LR}_0 < 0} \tag{15}$$

Each violation adds $\mathrm{LR}_1 > 0$; each clean inspection adds $\mathrm{LR}_0 < 0$. The *type separability* $q_H - q_L$ governs the magnitude of each update.

Dynamic targeting rule. In the dynamic extension the targeting rule replaces the static signal with the log-odds belief:

$$I(\pi_{jt}, u_{1j} \mid X_j, \lambda, \beta, \gamma_\pi, \rho) = \lambda_2 \, \Phi\left(\frac{\lambda_1 + X_j'\beta + \gamma_\pi \, \ell_{jt} + u_{1j}}{\rho} \right) \tag{16}$$

where $\gamma_\pi > 0$ is the belief-to-targeting sensitivity (distinct from the static foreignness weight γ of (7)). This reduces to the Duflo et al. baseline at $\gamma_\pi = 0$.

Two points clarify the role of foreignness. First, the regulator now targets on the *belief* π_{jt} (through its log-odds ℓ_{jt}), not on F_j directly; foreignness enters through the prior (13), which gives foreign plants a higher initial belief and hence more initial scrutiny. Second, γ_π is the dynamic *counterpart of*—not the same parameter as—the static foreignness weight γ in (7). At $t = 0$, $\ell_{j0} = \alpha_0 + \alpha_F F_j$, so $\gamma_\pi \, \ell_{j0}$ contributes an effective weight $\gamma_\pi \alpha_F$ on F_j; the dynamic rule thus coincides with the static rule at entry when $\gamma_\pi \alpha_F = (1 - \rho_I)\mu_F$. As inspections accumulate, ℓ_{jt} updates away from the prior toward the plant's true type, so the foreignness component of scrutiny fades—this transience is precisely Proposition 2.

A.3.4 State and per-period cost

The payoff-relevant state is the regulator's current belief π_{jt} (which the plant infers from the common inspection history $\mathcal{H}_{jt}$) together with the plant's fixed pollution shock u_{1j}. In a period in which the plant chooses $\mathrm{Run} \in \{0, 1\}$, its expected cost is the maintenance cost plus the expected regulatory penalty,

$$c_j \cdot \mathrm{Run} + I(\pi) \, V_0\big(p(\mathrm{Run}, u_{1j})\big), \tag{17}$$

where $I(\pi) = \lambda_2 \Phi\big([\lambda_1 + X_j'\beta + \gamma_\pi \ell(\pi) + u_{1j}]/\rho\big)$ is the inspection intensity (16), *strictly increasing in* π (because $\gamma_\pi > 0$ and ℓ, Φ are increasing), and $p(\mathrm{Run}, u_{1j})$ is the violation probability (12).

A.3.5 Belief transition

An inspection occurs with probability $I(\pi_{jt})/\lambda_2$. Given inspection, the violation outcome is $y_{jt} \sim \mathrm{Bernoulli}(p(\mathrm{Run}_{jt}, u_{1j}))$ and the belief updates via (14). The full one-period transition is:

$$\pi_{j,t+1} = \begin{cases} B(\pi_{jt}, 1) & \text{with prob. } \dfrac{I(\pi_{jt})}{\lambda_2} \, p(\mathrm{Run}_{jt}) \\ B(\pi_{jt}, 0) & \text{with prob. } \dfrac{I(\pi_{jt})}{\lambda_2} \, (1 - p(\mathrm{Run}_{jt})) \\ \pi_{jt} & \text{with prob. } 1 - \dfrac{I(\pi_{jt})}{\lambda_2} \end{cases} \tag{18}$$

Note $B(\pi, 1) > \pi > B(\pi, 0)$ for all $\pi \in (0, 1)$: a violation raises the belief and a clean inspection lowers it, with magnitudes given by (15).

A.3.6 Compliance benefit

An adapting plant ($\kappa_j = A$, finite c_j) runs equipment when the benefit exceeds the maintenance cost c_j. Running yields two benefits, both larger the more heavily the plant is inspected:

1. *Current deterrence* (the Duflo et al. (2018) channel): running lowers the violation probability from $p(0, u_{1j})$ to $p(1, u_{1j})$, cutting the penalty expected from this period's inspection by $I(\pi)\big[V_0(p(0, u_{1j})) - V_0(p(1, u_{1j}))\big]$.

2. *Reputation* (the new channel): a lower violation probability tilts the inspection outcome toward the clean update $B(\pi, 0)$ and away from the violation update $B(\pi, 1)$, lowering future beliefs and hence future scrutiny. I summarize this forward-looking gain in reduced form as

$$\Delta^{\text{rep}}(\pi) = \frac{I(\pi)}{\lambda_2}\left[p(0, u_{1j}) - p(1, u_{1j})\right]\nu_j, \qquad \nu_j > 0, \tag{19}$$

   where $\nu_j > 0$ is the marginal value to the plant of a lower belief (the discount factor is applied separately in (20)). Linearizing the continuation cost in the belief gives $\nu_j = \tau\,|\phi_2|\,\mathbb{E}[\tilde{P}_j]$, the form used in the simulations; only $\nu_j > 0$ matters for the results below.

The total benefit of running is

$$G(\pi) = \underbrace{I(\pi)\big[V_0(p(0, u_{1j})) - V_0(p(1, u_{1j}))\big]}_{\text{current deterrence}} + \underbrace{\delta\,\Delta^{\text{rep}}(\pi)}_{\text{reputation}}. \tag{20}$$

This reduced-form benefit replaces a full dynamic program: the only structure I use is that *both* channels scale with the inspection intensity $I(\pi)$, which is increasing in the belief π.

A.3.7 The threshold rule

The benefit (20) delivers the single result I need for the dynamics: the plant complies exactly when the regulator's belief—and hence scrutiny—is high enough.

Lemma 1 (Threshold compliance rule)**.** *$G(\pi)$ is strictly increasing in π. Hence the adapting plant's optimal policy is a threshold rule,*

$$\text{Run}^*(\pi) = \mathbf{1}\{\pi \geq \pi^*\}, \tag{21}$$

where π^* is the unique solution of $G(\pi^*) = c_j$ when c_j lies in the range of G (plants with very high c_j never run; those with very low c_j always run).

Proof. Running strictly lowers pollution ($\phi_2 < 0$ in (2)), so $p(0, u_{1j}) > p(1, u_{1j})$ and, since V_0 is increasing, $V_0(p(0, u_{1j})) - V_0(p(1, u_{1j})) > 0$. Both this difference and the gap $p(0, u_{1j}) - p(1, u_{1j}) > 0$ are positive constants in π. Collecting terms in (20),

$$G(\pi) = \underbrace{\Big[V_0(p(0, u_{1j})) - V_0(p(1, u_{1j})) + \tfrac{\delta\nu_j}{\lambda_2}\big(p(0, u_{1j}) - p(1, u_{1j})\big)\Big]}_{>0,\ \text{constant in } \pi} I(\pi),$$

a positive constant times the inspection intensity. Since $I(\pi)$ is strictly increasing in π (because $\gamma_\pi > 0$ and ℓ, Φ are increasing) and $I(\pi) \to 0$ as $\pi \to 0$, G rises strictly from 0 to its maximum at $\pi = 1$. The threshold rule and the unique interior π^* solving $G(\pi^*) = c_j$ then follow from the intermediate value theorem. □

Non-adapting plants ($\kappa_j = N$) face effectively infinite marginal maintenance cost and set $\mathrm{Run}_{jt} = 0$ in every period, independently of π.

A.4 Monte Carlo Simulations

Following the Monte Carlo strategy of Appendix B in Duflo et al. (2018), three simulation exercises illustrate the propositions quantitatively, using a calibrated parameter vector chosen to match the broad features of the Duflo et al. estimates. Two are reported as figures—Figure A1 (Proposition 1) and Figure A2 (Proposition 2)—and the third as a numerical summary.

A.4.1 Simulation 1: Optimal γ^* as information quality varies (Proposition 1)

This simulation illustrates Proposition 1. I draw a population in which the foreign premium μ_F splits across the observed and unobserved shocks as in (6), and vary the information quality ρ_I on a grid from 0.01 to 0.99 holding the total shock variance $\sigma_1^2 + \sigma_2^2$ fixed (so the pollution distribution is the same at every ρ_I and only the observed share moves). At each ρ_I the regulator observes u_{1j} and F_j, and I compute total expected abatement $\mathbb{E}[I_j\,\Delta_j]$ under a common inspection budget for three regimes:

(i) *Uniform*: every plant inspected equally (no targeting).

(ii) *Signal only*: target on the observed u_{1j} ($\gamma = 0$), the Duflo et al. baseline.

(iii) *Signal + foreignness*: target on $m_j = u_{1j} + (1-\rho_I)\mu_F F_j$ from (9), which adds the regulator's estimate of the unobserved premium (Proposition 1).

A.4.2 Simulation 2: Belief paths and compliance trajectories by firm type (Proposition 2)

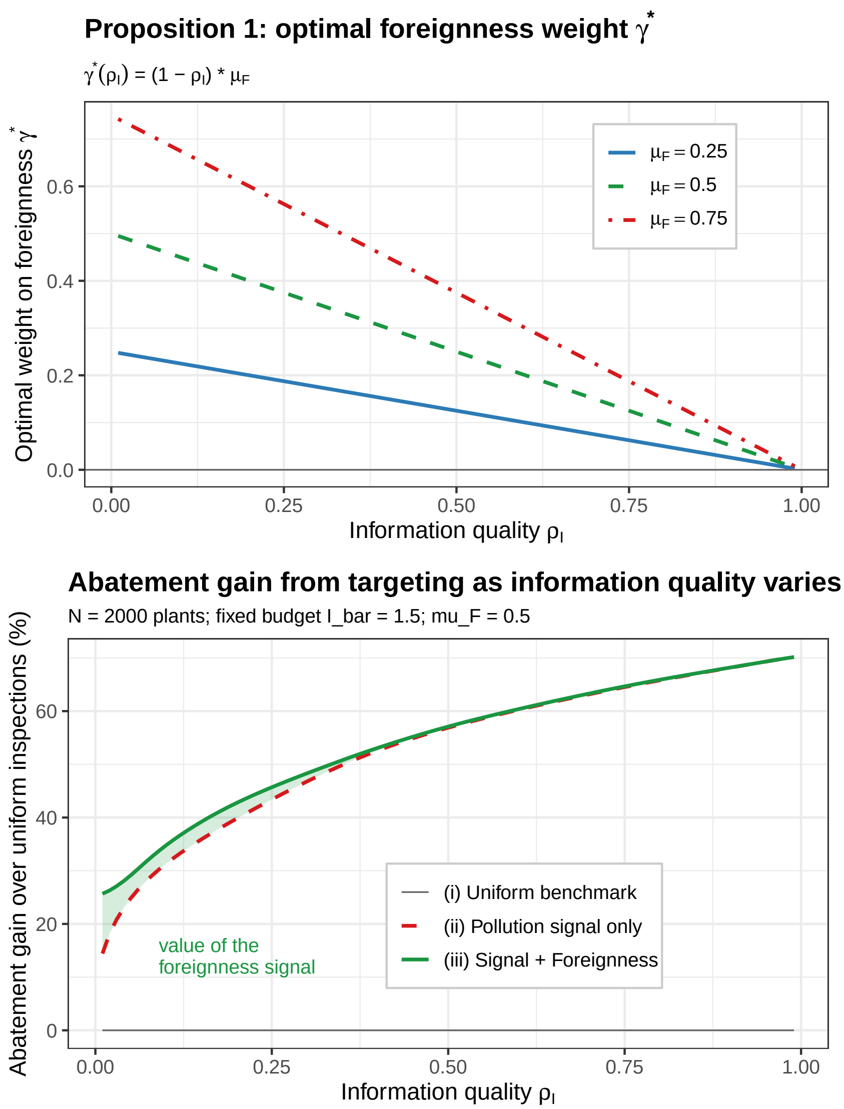
I focus on the two archetypes that drive Proposition 2—the adapting low-risk type (L, A) (transitory premium) and the non-adapting high-risk type (H, N) (persistent premium)—and simulate each as both foreign and domestic, $N_{\text{sim}} = 500$ plants per cell over $T = 60$ periods. For each plant:

1. Initialize belief at $\pi_{j0} = \bar{\pi}^F$ (foreign) or π^D (domestic).

2. At each t: set $\mathrm{Run}^*_{jt} = \mathbf{1}\{\pi_{jt} \geq \pi^*\}$ for adapting plants and $\mathrm{Run}^*_{jt} = 0$ for non-adapting plants; draw $D_{jt} \sim \mathrm{Bernoulli}(I(\pi_{jt})/\lambda_2)$, draw $y_{jt} \sim \mathrm{Bernoulli}(p(\mathrm{Run}^*_{jt}, u_{1j}))$ if $D_{jt} = 1$, and update π_{jt} via (14).

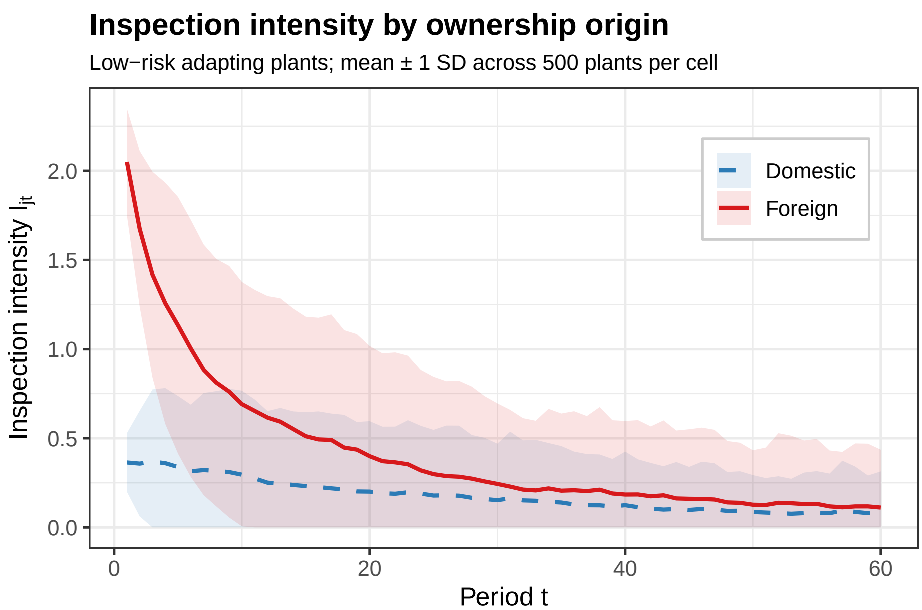
3. Record $(\pi_{jt}, I_{jt}, \mathrm{Run}^*_{jt})$ at each t.

Figure A1: **Proposition 1 in simulation.**



Notes: Upper Panel: optimal foreignness weight $\gamma^*(\rho_I) = (1 - \rho_I)\mu_F$. Lower Panel: abatement gain over uniform inspections by targeting regime, at a fixed inspection budget, as information quality ρ_I varies; the shaded band is the value of the foreignness signal.

Figure A2: **Proposition 2 in simulation.**



Notes: Inspection intensity for low-risk adapting plants, foreign vs. domestic ownership (mean ± 1 SD across 500 plants per cell).

B Appendix B: Elite Interviews

B.1 Population and Recruitment

The main interview population consists of managers at state-level environmental agencies across the U.S. In particular, I focus on the following two groups of officials:

- **Top agency leaders and commissioners.** These officials are typically the heads of environmental agencies, executive directors, commissioners, and deputy directors. They often review or approve major penalty decisions. Many are politically appointed and play important roles in setting agency priorities and managing relationships with elected officials and regulated firms.

- **Directors of departments, bureaus, divisions, and district offices.** These officials are responsible for the day-to-day implementation of environmental programs and frequently oversee permitting and enforcement activities. They often possess extensive technical and managerial experience and supervise rank-and-file inspectors and permit writers.

Directors of departments, bureaus, divisions, and district offices are more likely to have technical and engineering backgrounds. By contrast, top agency leaders and commissioners include individuals with technical and engineering backgrounds as well as attorneys who have previously worked on environmental compliance and enforcement matters.

Recruitment was conducted using a combination of email outreach, LinkedIn outreach, and snowball sampling. To identify potential interviewees, I systematically reviewed the websites of environmental agencies in all 50 states. Many agencies publish organizational charts, leadership directories, staff directories, or contact information for senior managers. Some agencies provide complete staff directories, including job titles and email addresses, while others provide only limited contact information for senior leadership.

Recruitment initially relied heavily on LinkedIn. However, I gradually shifted toward email recruitment after observing that response rates on LinkedIn were relatively low and that the platform imposed limits on the number of messages that could be sent to users who were not already connected. When email addresses were publicly available, I contacted officials directly by email. In addition to the LinkedIn outreach, I emailed 193 state environmental agency officials between May and June 2026. Of these emails, 20 were undeliverable—14 sent to invalid addresses and 6 blocked or rejected by recipient mail servers—leaving 173 successfully delivered.

Participation was entirely voluntary. Potential interviewees were informed that interviews were confidential, that no identifying information would be reported without permission, and that the project had been reviewed and approved by Harvard University's Institutional Review Board. Recruitment communications also emphasized that interviews could be conducted by phone, Zoom, Microsoft Teams, or in person, depending on the preferences and availability of the interviewee.

B.2 Completed Sample

I completed 27 interviews with 31 environmental regulators across 21 states. Nine participants are agency heads, and the remaining 22 are program or district heads. Most interviewees have worked in one of the three regulatory programs and therefore have inspection experience. Several, however, have held only policy or leadership roles and could not answer questions about inspections. Table B1 explains the composition of the completed sample.

Table B1: **Completed interviews with environmental regulators by state partisanship.**

	Democratic Strongholds	Swing States	Republican Strongholds
States covered	5	5	11
Interviews completed	9	6	12
Interview participants	9	6	16
Of whom: agency heads	1	2	6
Of whom: program/district heads	8	4	10
Of whom: former/current inspectors	4	5	9

In addition to regulators at state environmental agencies, I also interviewed one manager at a foreign-owned manufacturing firm in the U.S. (interview index number B1) and a regulator at a state-level occupational safety and health (OSH) regulatory agency (interview index number O1).

B.3 Interview Structure

The interviews were semi-structured and designed to elicit respondents' experiences and interpretations. I used a uniform set of guiding questions, shown below, across interviews to ensure comparability in responses. At the same time, the conversations were intentionally flexible and often adapted to each interviewee's area of expertise and professional background.

Nearly all completed interviews took place virtually over Zoom or Microsoft Teams. Each virtual interview lasted approximately 30 to 90 minutes. One interview took the form of written correspondence, where I provided the complete list of questions, and the participant provided detailed written responses.

Part A. Interstate Differences in Regulations

A1. In general, would you say that your state's environmental regulations differ substantially from the federal baseline under major programs such as the Clean Air Act, the Clean Water Act, or the Resource Conservation and Recovery Act? For example, does your state have more stringent requirements, or does it regulate pollutants or activities that federal programs do not cover?

Part B. External Investors

B1. Does your agency coordinate with state leadership to facilitate permitting for external investors, namely companies from other states or foreign countries?

B2. When external investors come to your state, do they tend to request more generous investment incentives (e.g., tax credits or grants) by pointing to competing offers from other states? Do they also seek accommodations or flexibility related to environmental regulations, permitting, or enforcement?

B3. Based on your experience, are out-of-state or foreign-owned companies more likely than comparable in-state companies to experience compliance challenges, especially during their first few years operating in the state? In particular, do these firms face challenges in understanding your state's regulations?

- Have you observed compliance differences that appear to be related to company management structures, internal decision-making processes, or corporate culture?
- Do these external companies usually overcome these difficulties over time as they become more established?

B4. Do these external companies generally bring more environmentally friendly technologies or practices?

Part C. Inspections

C1. When facilities change ownership and transfer their permits, do inspectors review the facilities' records or conduct audits to update permit information? Do they also conduct in-person inspections to verify compliance and become familiar with new managers or operators?

C2. For new greenfield facilities established by out-of-state or foreign-owned companies, does your agency conduct proactive oversight after operations begin, such as inspections, compliance assistance visits, evaluations, or audits? By proactive oversight, I mean actions beyond receiving routine self-reported data required by permits.

C3. In general, do facilities owned by out-of-state or foreign companies receive more frequent inspections compared to similar facilities owned by in-state companies?

Part D. Violations and Penalties

D1. Does your agency use detailed worksheets or formulas to determine fines?

D2. Does the agency consider the size of the facility or its parent company when determining fines? Do smaller in-state businesses receive reductions due to weaker financial capacity?

D3. I understand that facilities and companies may negotiate with the agency over proposed financial penalties or agreed orders. In your experience, how important are these negotiations in shaping the final penalties or settlement terms?

- How important is a company's demonstrated willingness to cooperate, correct violations, or show good faith during these negotiations?
- In penalty outcomes, do factors such as nationality or foreign ownership ever play a role, in addition to company size or other characteristics?

D4. Do out-of-state and foreign firms have apparently different approaches to the negotiations?

- For example, do they rely on external consultants and lawyers to deal with negotiations? If so, does this affect their credibility or ability to demonstrate goodwill?
- Are foreign companies less likely to negotiate and more willing to accept initial penalties, or do they negotiate more aggressively to reduce penalties?

C Appendix C: Context and Data

C.1 Administrative Data from the USEPA

Table C1: **Administrative data from the USEPA spanning the entire enforcement process.**

Program	**Scrutiny actions**	**Documented Violations**	**Penalties**	**Regulated Facilities**
Air (CAA)	All (n= 1,742k)	1) All High-priority violations (HPVs) & federally reportable violations (FRVs) 2) All stack tests 3) Self-reported deviations in Title-V Annual Compliance Certification: only Major facilities (n= 19k)	All (n= 103k)	All (n=220k)
Water (CWA)	All (n=1,706k)	1) All self-reported violations & late reporting to Discharge Monitoring Reports (DMR) for all facilities 2) All violations of schedule obligations in permits or enforcement actions 3) Significant Non-Compliance (SNC) levels rated by regulators	All (n= 160k)	All (n=910k)
Waste (RCRA)	All (n=1,152k)	1) All historical violations 2) All Significant Non-Compliance (SNC) status 3) Biennial Reports (BR) by "large-quantity" generators	All (n=379k)	All (n=1,512k)

Notes: For the CAA and CWA, the data are drawn from the USEPA's Enforcement and Compliance History Online (ECHO) system, which integrates information reported by facilities with enforcement records from both the USEPA and authorized state agencies. For RCRA, the data come from RCRAInfo, the USEPA's primary database for the RCRA program.

Figure C1: Number of facilities regulated under CAA, CWA, and RCRA by state

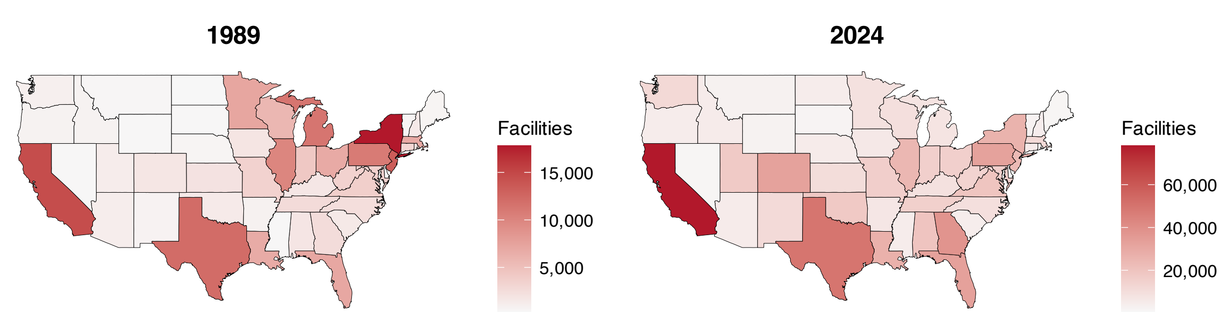
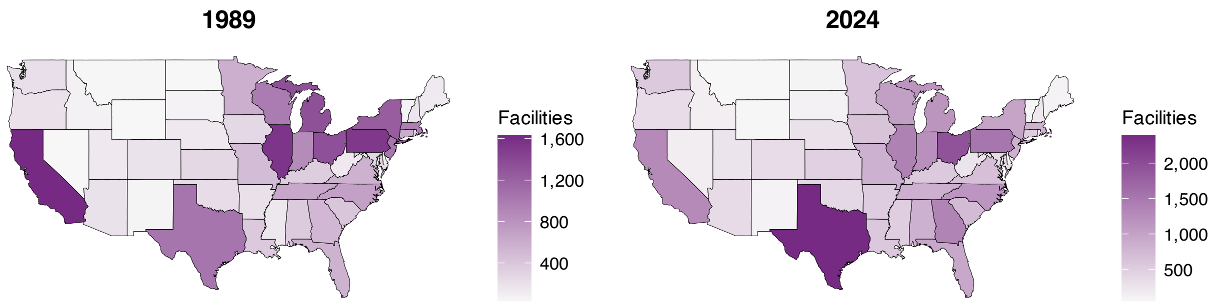


Figure C2: Number of TRI facilities by state



C.1.1 Additional Cleaning Details for EPA Data

Government-owned facilities. I exclude government-owned facilities, such as municipal water facilities, due to distinct enforcement practices against them (Konisky and Teodoro, 2016). These excluded facilities include 9,949 regulated by the CAA and 81,340 regulated by the CWA.

TRI sample. I include any facility that has ever submitted a TRI report and/or has a TRI-system identifier. Importantly, many such facilities have only reported in selected years during their life spans, so the USEPA's TRI data products only include facility-year units that submitted reports, and relying on them would lead to an incomplete, porous dataset instead of a panel with consecutive years.

RCRA facilities. In the main analysis, I focus on RCRA facilities that are "generators" of hazardous or solid waste, but not "non-generators," namely transporters and handlers. Among 1,367,370 RCRA facilities, 399,685 are generators.

C.2 Dun & Bradstreet

Foreign Ownership Definition For each establishment-year, D&B reports the establishment's own DUNS number, as well as the DUNS numbers of the firm's headquarters, immediate parent, and global ultimate owner. For example, `TOYOTA INDUSTRIAL EQP MFG IN` operates a plant in Elk Grove Village, Illinois. The data record for this plant shows the same name, `TOYOTA INDUSTRIAL EQP MFG IN`, but a distinct DUNS number. Its record also reports the DUNS number of the firm's headquarters in Columbus, Indiana; the DUNS number of its immediate parent, `TOYOTA INDUSTRIES N AMER IN`, also headquartered in Columbus, Indiana; and the DUNS number of the headquarters of the global ultimate owner, `TOYOTA INDUSTRIES CORPORATION`, located in Toyota, Aichi, Japan.

I classify an establishment as foreign-owned if any reported DUNS identifier for the headquarters, immediate parent, or global ultimate owner corresponds to an entity located outside the U.S.

Data on National Origin I can also identify the specific national origin of foreign headquarters, parents, and ultimate owners for 2009–2019. I use two D&B products: `DMI` (1969–2024) for U.S. establishments and `WorldBase` (2009–2019) for establishments worldwide. While `DMI` reports the DUNS numbers of foreign headquarters, parents, and ultimate owners, it does not have additional information for those establishments. Their national origins are shown in `WorldBase`.

C.3 Data Linkage

C.3.1 Basic Procedure

I link regulatory administrative records to business registration data using a three-step procedure.

1. I conduct extensive cleaning and standardization of the following variables that will be used as matching keys: name, street address, 5-digit zip code, and city.

2. I perform exact matching using firm name and street address as matching keys. To reduce false matches, I block on the 5-digit ZIP code and city name.

3. For records that remain unmatched, I apply fuzzy matching using the `LinkTransformer` approach (Arora and Dell, 2023). This method computes semantic similarity based on cosine distance between text embeddings of the match keys, again using firm name and street address, while applying the same set of blocking variables as in the exact-matching stage. I select a similarity threshold based on manual inspection to balance match quality and coverage. As a validation exercise, I separately re-implement this third step using probabilistic record linkage (Enamorado et al., 2019) in place of the embedding-based approach.

C.3.2 Standardizing Facility/Establishment Names and Addresses

Facility/establishment names are standardized as follows:

- I remove leading punctuation and normalize punctuation and separators such as hyphens, slashes, ampersands, periods, commas, and parentheses into spaces.

- I drop the leading article `THE` when it appears at the beginning of the name.

- I standardize common word variants to a single form, for example `ENTERPRISES` to `ENTERPRISE`, `TECHNOLOGIES` to `TECHNOLOGY`, `PROPERTIES` to `PROPERTY`, and `WHOLESALES` to `WHOLESALE`.

- I remove common legal suffixes and organizational endings, including forms such as `INC`, `INCORPORATED`, `COMPANY`, `CO`, `CORP`, `CORPORATION`, `LTD`, `LLC`, `LLP`, `LP`, `CPA`, and `COOPERATIVE`.

- I remove state-specific trailing geographic terms when they appear as suffixes at the end of a name.

- I iteratively strip generic end-of-name descriptors that often identify subdivisions rather than firms, such as `DIV`, `DIVISION`, `DEPT`, `DEPARTMENT`, `PLANT`, `WAREHOUSE`, `MANUFACTURING`, `ENGINEERING`, `PRODUCTS`, and `MACHINERY`.

- After cleaning, I collapse repeated whitespace and set empty or non-informative strings to missing.

For the USEPA data specifically, there are often trailing plant- or site-specific descriptors that appear after delimiters such as commas or hyphens, for example, strings of the form `- PLANT 1` starting with the hyphen `-`. I remove all of them because the D&B database does not have this naming pattern.

Street addresses are standardized as follows:

- I standardize directional indicators, for example converting `NORTH`, `SOUTH`, `EAST`, and `WEST` to `N`, `S`, `E`, and `W`, and collapsing combinations such as `N W` to `NW`.

- I standardize common address forms such as highway references, county trunk roads, post office boxes, and ordinal street names.
- I remove secondary unit designators and low-information sub-address elements, including forms such as `APT`, `SUITE`, `UNIT`, `BLDG`, `ROOM`, and `FLOOR`.
- I remove routing indicators such as rural routes and similar delivery-only address elements.
- I strip common street-type suffixes, including forms such as `STREET`, `ROAD`, `AVENUE`, `BOULEVARD`, `DRIVE`, `LANE`, `COURT`, `TERRACE`, `TRAIL`, and `WAY`.
- I trim whitespace and set cleaned addresses to missing when they are empty or contain too little alphanumeric information to support reliable matching.

C.3.3 Specifications for Exact Matching

The following stages are implemented sequentially:

1. Exact matching on cleaned facility name, cleaned street address, and 5-digit ZIP code.
2. Exact matching on cleaned facility name, cleaned street address, and city name.
3. Exact matching on cleaned facility name and 5-digit ZIP code.
4. Exact matching on cleaned street address and 5-digit ZIP code.
5. Exact matching on cleaned facility name and city name.
6. Exact matching on cleaned street address and city name.

Additional implementation details:

- Exact-matching stages are applied sequentially, so records matched in an earlier stage are removed before later stages are run.
- When street address is used as a match key, I require the cleaned address to contain at least two distinct tokens to avoid low-information matches.
- If a record matches multiple candidate business records within a stage, I resolve the tie by re-scoring the tied pairs using semantic similarity on the available text fields and retain the highest-scoring candidate.
- Multiple administrative records may link to the same business record at the exact-matching stage.

C.3.4 Specifications for Fuzzy Matching

Fuzzy matching is applied only to records that remain unmatched after the sequential exact-matching stages. I implement fuzzy matching using the `LinkTransformer` package (Arora and Dell, 2023), specifically its blocking-based nearest-neighbor matching procedure. This is the stage that produces all linkages used in this paper.

The following stages are implemented sequentially:

1. Fuzzy matching on cleaned firm name and cleaned street address, blocked on 5-digit ZIP code.
2. Fuzzy matching on cleaned firm name and cleaned street address, blocked on city name.

Additional implementation details:

- Similarity is computed from text embeddings using the `all-MiniLM-L6-v2` sentence-transformer model, and candidate pairs are ranked by cosine similarity.
- I use `LinkTransformer`'s `merge_blocking()` routine with `merge_type = "m:1"`, so that each record is assigned its single best candidate within a given blocking group before downstream filtering.
- I impose a minimum similarity threshold of 0.74. Candidate pairs below this threshold are discarded.
- When both ZIP-blocked and city-blocked candidates are available for the same record, the ZIP-blocked candidate is treated as the anchor. A city-blocked candidate is retained only if its similarity score exceeds the best ZIP-blocked candidate by at least 0.05.
- Fuzzy matching is iterative. After each round, newly matched records are removed from both datasets, and the procedure is rerun on the remaining unmatched records. I allow up to two rounds.
- Duplicate or conflicting fuzzy candidates are resolved using greedy one-to-one assignment within the residual candidate pool: higher-similarity pairs are retained first, and once a record has been assigned, competing lower-scoring candidates involving that record are discarded.
- When cleaned street address is used in fuzzy matching, I require the address string to contain at least two distinct tokens to avoid low-information comparisons.
- Matching is adaptive with respect to address information. If cleaned street address is observed on both sides, similarity is computed using both cleaned firm name and cleaned street address. If address is missing on either side, similarity is computed using cleaned firm name alone within the same blocking group.

C.4 Descriptive Statistics

Figure C3 describes the prevalence of externally owned facilities. I distinguish two modes of foreign/out-of-state entry from each facility's observed ownership history: *greenfield* facilities are foreign- or out-of-state-owned in every year they are observed (foreign/out-of-state since first observed), whereas *M&A* facilities are first observed under domestic ownership and later become foreign- or out-of-state-owned. Throughout, "non-TRI" refers to all regulated facilities except those reporting to the Toxics Release Inventory (TRI), and each panel compares the complete facility panel against the subset matched to Dun & Bradstreet (D&B) ownership records.

Figure C3: **The number of foreign-owned facilities over time, by entry mode.**

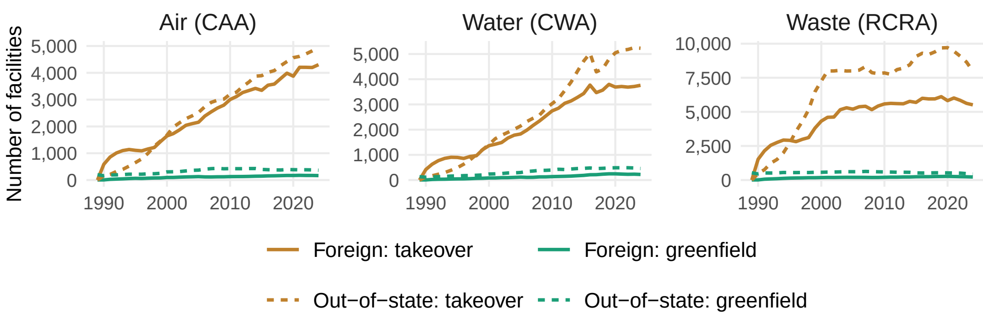
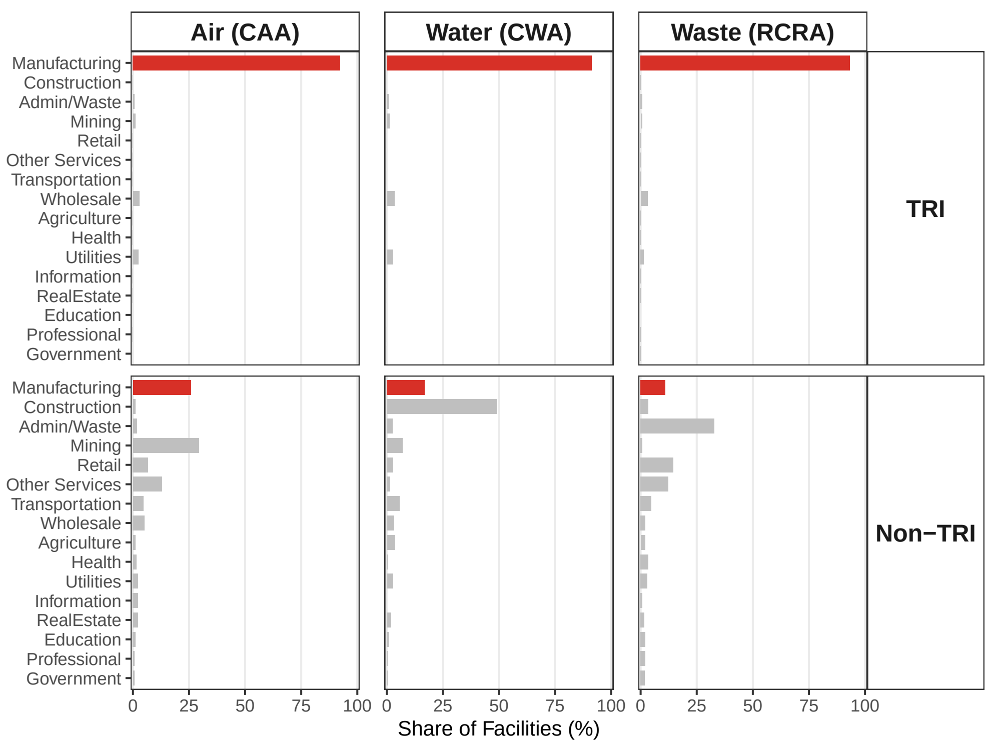


Figure C4: **Industrial Composition of Facilities in the Sample.**

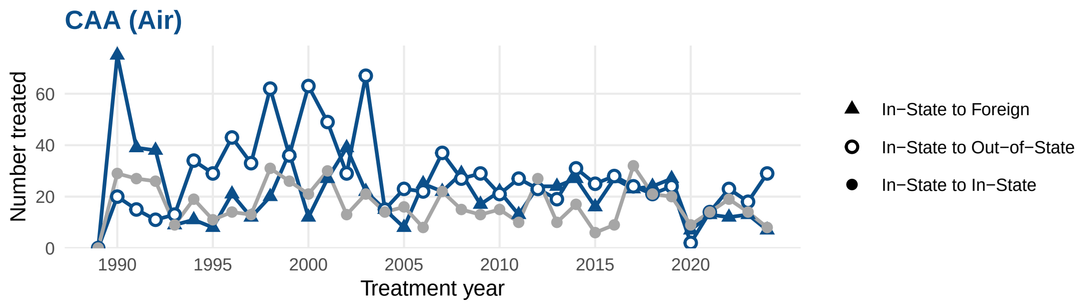


Notes: Sectors follow the two-digit NAICS sector definitions. Only sectors with a share above 0.1% are shown.

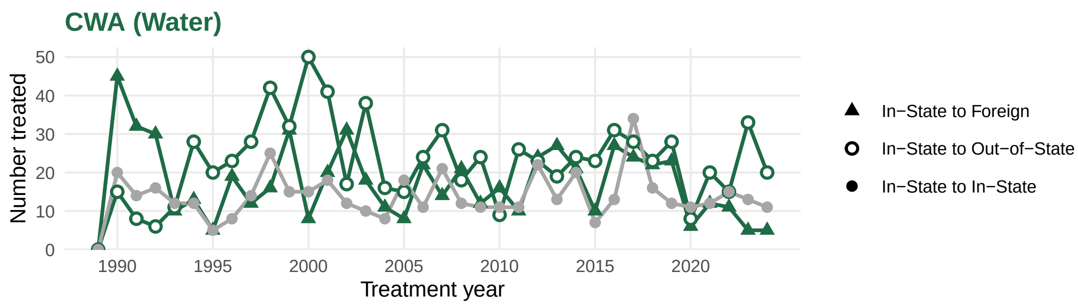
D Appendix D: How Scrutiny Actions Respond to Takeovers

D.1 Distribution of Treatment Years

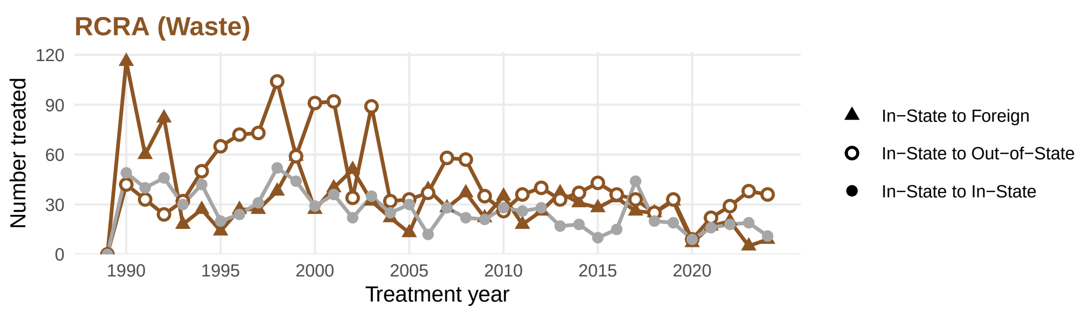
Figure D1: Distribution of treatment timing: Primary subsample with official data linkage



In−State to Foreign: 765 ever−treated units among 4,218 panel units (18.1%).
In−State to Out−of−State: 986 ever−treated units among 4,439 panel units (22.2%).
In−State to In−State: 609 ever−treated units among 7,637 panel units (8.0%).

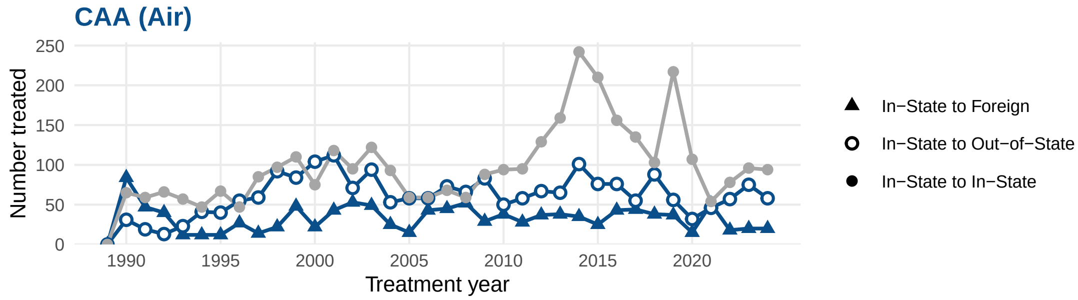


In−State to Foreign: 621 ever−treated units among 3,412 panel units (18.2%).
In−State to Out−of−State: 817 ever−treated units among 3,608 panel units (22.6%).
In−State to In−State: 498 ever−treated units among 5,770 panel units (8.6%).

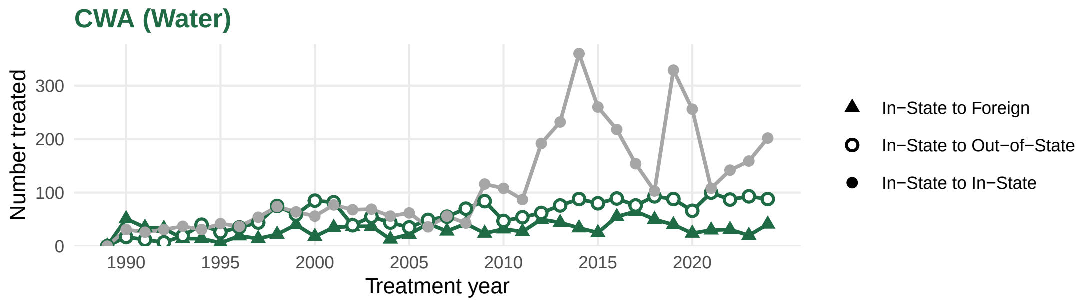


In−State to Foreign: 1,129 ever−treated units among 6,993 panel units (16.1%).
In−State to Out−of−State: 1,588 ever−treated units among 7,452 panel units (21.3%).
In−State to In−State: 936 ever−treated units among 13,093 panel units (7.1%).

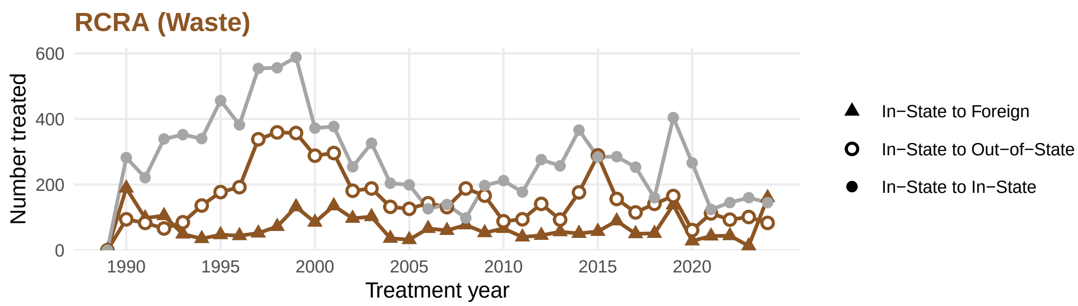
Figure D2: Distribution of treatment timing: Full sample including original data linkage



In−State to Foreign: 1,181 ever−treated units among 16,474 panel units (7.2%).
In−State to Out−of−State: 2,189 ever−treated units among 17,482 panel units (12.5%).
In−State to In−State: 3,505 ever−treated units among 88,266 panel units (4.0%).



In−State to Foreign: 1,114 ever−treated units among 24,139 panel units (4.6%).
In−State to Out−of−State: 2,119 ever−treated units among 25,144 panel units (8.4%).
In−State to In−State: 3,976 ever−treated units among 229,933 panel units (1.7%).



In−State to Foreign: 2,497 ever−treated units among 68,334 panel units (3.7%).
In−State to Out−of−State: 5,634 ever−treated units among 71,471 panel units (7.9%).
In−State to In−State: 9,876 ever−treated units among 497,387 panel units (2.0%).

Figure D2 shows that, in the full sample, the recorded in-state takeover events are disproportionately concentrated in 2014, 2015, and 2019 (e.g., 363 events in 2014 in the water sample versus 100–230 in adjacent years), years that coincide with major refreshes of D&B's family-tree data. Roughly 80 percent of the events in these years are facilities that D&B links to a parent for the first time. These links appear genuine—the parents are real multi-establishment firms and survive standard validity screens—but they reflect waves of expanding D&B coverage: affiliations formed in earlier years are first *recorded* at a vintage refresh, so the recorded treatment year may overstate the true acquisition year. Because this mistiming is unobservable at the facility level, I assess sensitivity by re-estimating the in-state takeover specifications after dropping all facilities first treated in 2014, 2015, or 2019 (about one fifth of treated units), leaving never-treated controls unchanged. Results are reported in Figure D40 and are similar to the baseline estimates.

D.2 Model Specification

PanelMatch pairs each treated unit with a set of control units that share identical treatment histories over the previous L years and then refines these matched sets using covariate-balancing matching and weighting methods. It computes a DiD estimate for each matched set and averages these estimates to obtain the final treatment effect. The estimator is formalized as follows:

$$\hat{\delta}(F, L) = \frac{1}{\sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it}} \sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it} \left\{ (Y_{i,t+F} - Y_{i,t-1}) - \sum_{i' \in M_{it}} \omega_{it}^{i'} (Y_{i',t+F} - Y_{i',t-1}) \right\} \tag{22}$$

where Y_{it} represents the dependent variable, the number of scrutiny actions under each program in a year; F denotes the number of post-treatment years for which the effect is estimated; L, as mentioned, represents the length of identical treatment history required for matching; D_{it} stands for the treatment status of facility i in year t. $\omega_{it}^{i'}$ represents the weights constructed for refinement, which give greater influence to units with more similar covariates.

I compare five refinement methods available in PanelMatch, and in each specification, I adopt the weights from the method that achieves the best covariate balance. The five refinement methods are: Mahalanobis matching, propensity score matching, propensity score weighting, Covariate Balancing Propensity Score (CBPS) matching, and CBPS weighting. For matching-based approaches, I cap the number of matched controls at five to ensure strong covariate balance.

I choose $L = 4$ and $F = 4$. I choose a moderate F because an F larger than L can lead to extrapolation risks. I use relatively small values of L because the sample period is not very long, and because mergers & acquisitions are not the construction of new facilities and therefore can be implemented within a few years. Robustness checks use alternative values $L = 5$ (Figure D35) and $L = 6$ (Figure D36).

D.3 Covariates

Table D1: **Covariates used in the `PanelMatch` design.**

Sample	**Type**	**Variable/Definition**
All	Prior Enforcement	Number of scrutiny actions
	Prior Enforcement	Number of penalties
	Facility Size	Log sales in U.S. dollars
	Facility Size	Log number of employees
	Facility History	Years since its first year in the D&B database
	Environmental Justice Area	Located in an EPA-designated “Environmental Justice” neighborhood
Air (CAA)	Prior Violations	Number of Federally Reportable Violations (FRV)
	Prior Violations	Number of High Priority Violations (HPV)
	Prior Violations	Number of failed air stack tests
	Prior Violations	Number of self-reported deviations in annual Title-V permit reviews
	Facility Emission	Number of Major and Synthetic Minor pollutants
	Facility Emission	Log amount of total air releases on site
	Facility Emission	Number of TRI-regulated chemicals
Water (CWA)	Facility Emission	Number of CWA permits
	Facility Emission	Number of CWA Major permits
	Facility Emission	Number of CWA permits issued by the USEPA
	Facility Emission	Log amount of total water releases on site
	Facility Emission	Number of TRI-regulated chemicals
	Prior Violations	# Serious Non-Compliance (SNC) in compliance/permit schedule
	Prior Violations	# SNC violations in effluent limits
	Prior Violations	# Reporting schedule violations
	Prior Violations	# All violation findings
Waste (RCRA)	Activity	Size of generator
	Activity	Transporter
	Activity	TSDF
	Activity	Number of TRI-regulated chemicals
	Violations	Any violation in the year
	Violations	Any Serious Non-Compliance (SNC) in the year
	Violations	Under any Corrective Action (CA) during the year
	Violations	Previous penalties

Notes: “Permit-quarters in Serious Non-Compliance (SNC) in compliance/permit schedule” combines two separate types of SNC in the NPDES data: SNC in compliance/permit schedule with at least 90 days of lateness (priority level 1) and SNC in compliance/permit schedule with at least 30 days of lateness (priority level 4). “Permit-quarters in SNC in effluent limits” combines two separate types of SNC in the NPDES data: SNC in monthly effluent limits (priority level 2) and SNC in non-monthly effluent limits (priority level 3). “Permit-quarters in reporting schedule violations” combines two separate counts: reporting schedule violations by major-status facilities (SNC, priority level 5) and other reporting schedule violations (non-SNC, priority level 6).

D.4 Covariate Balance

Covariate balance is assessed as the average standardized difference between treated and control units on the covariates.

Figure D3: **`PanelMatch` covariate balance summary statistics when the treatment is ownership change from an in-state parent company to a foreign one.**

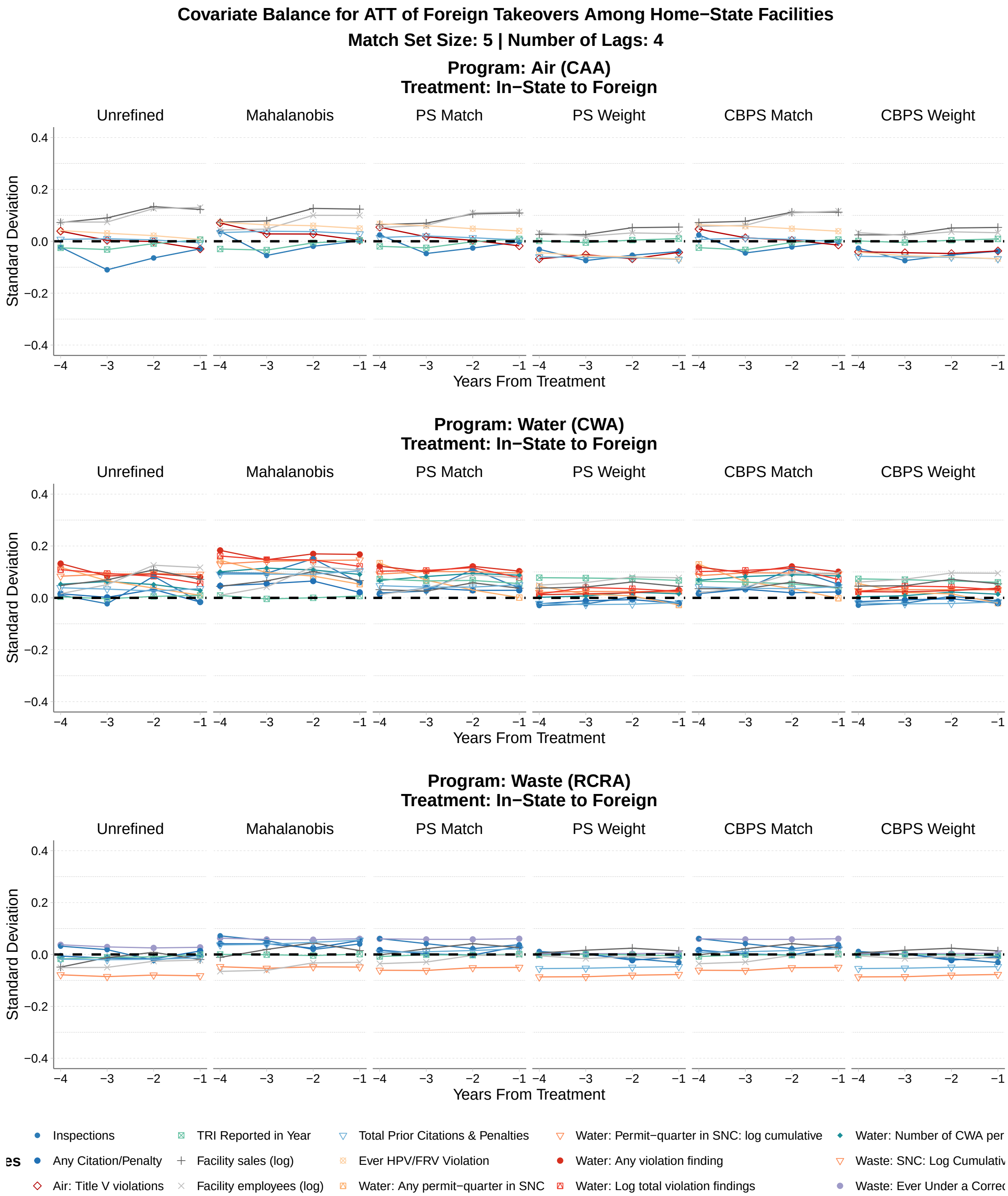


Figure D4: **`PanelMatch` covariate balance summary statistics when the treatment is ownership change from an in-state parent company to an out-of-state one.**

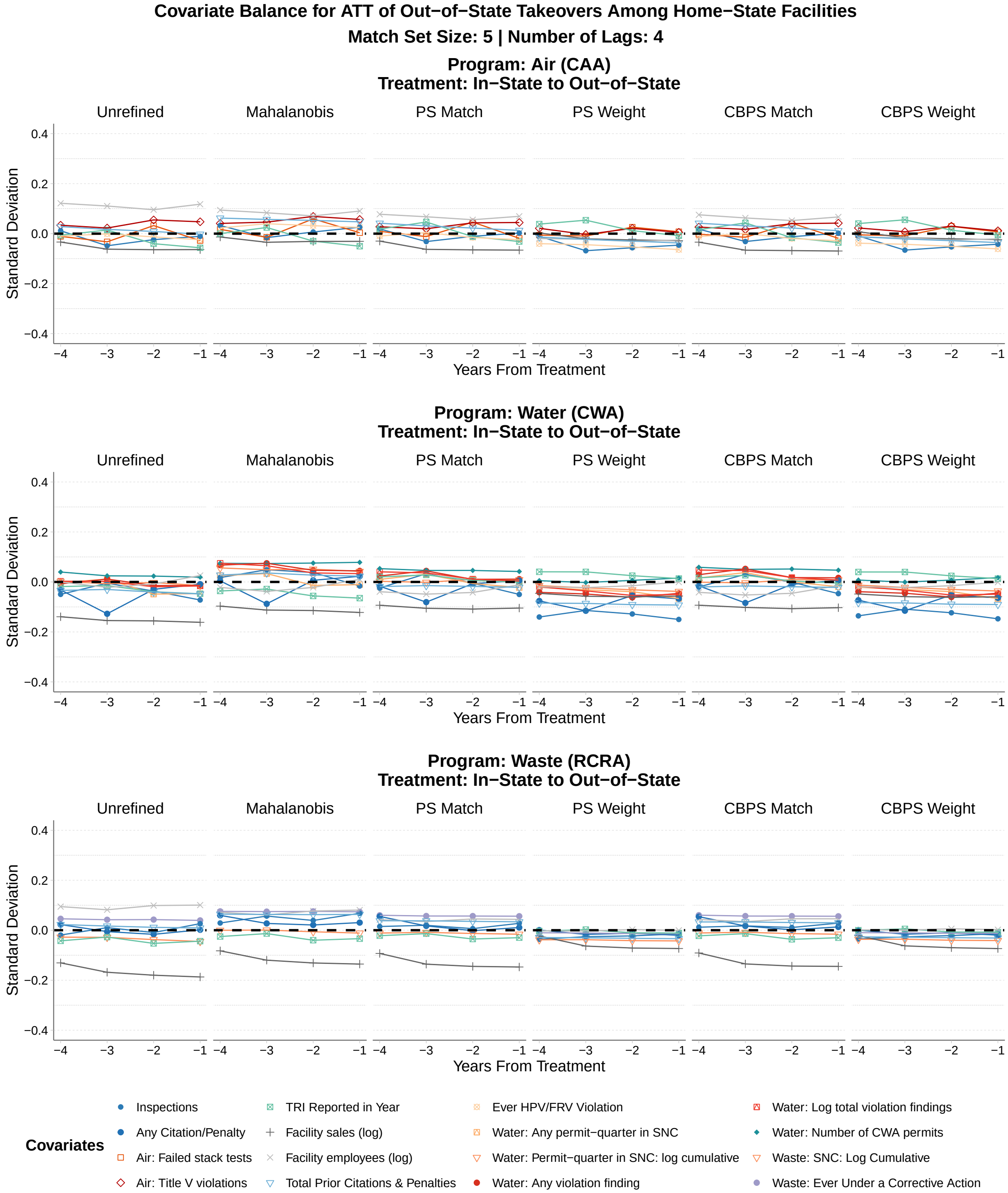
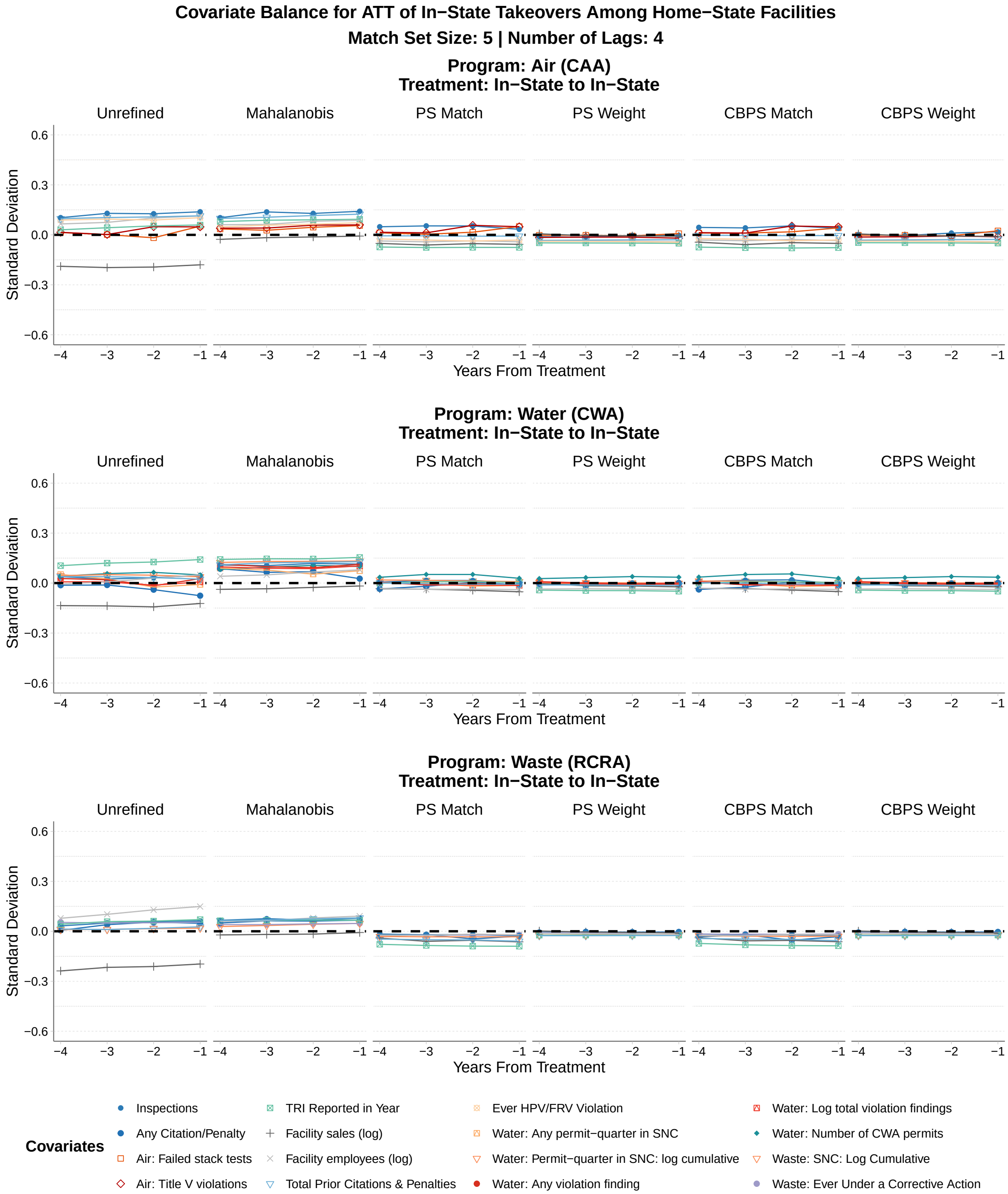


Figure D5: **`PanelMatch` covariate balance summary statistics when the treatment is ownership change from an in-state parent company to another in-state one.**



D.5 Distribution of DV

Figure D6: Distribution of the dependent variable: Primary subsample with official data linkage

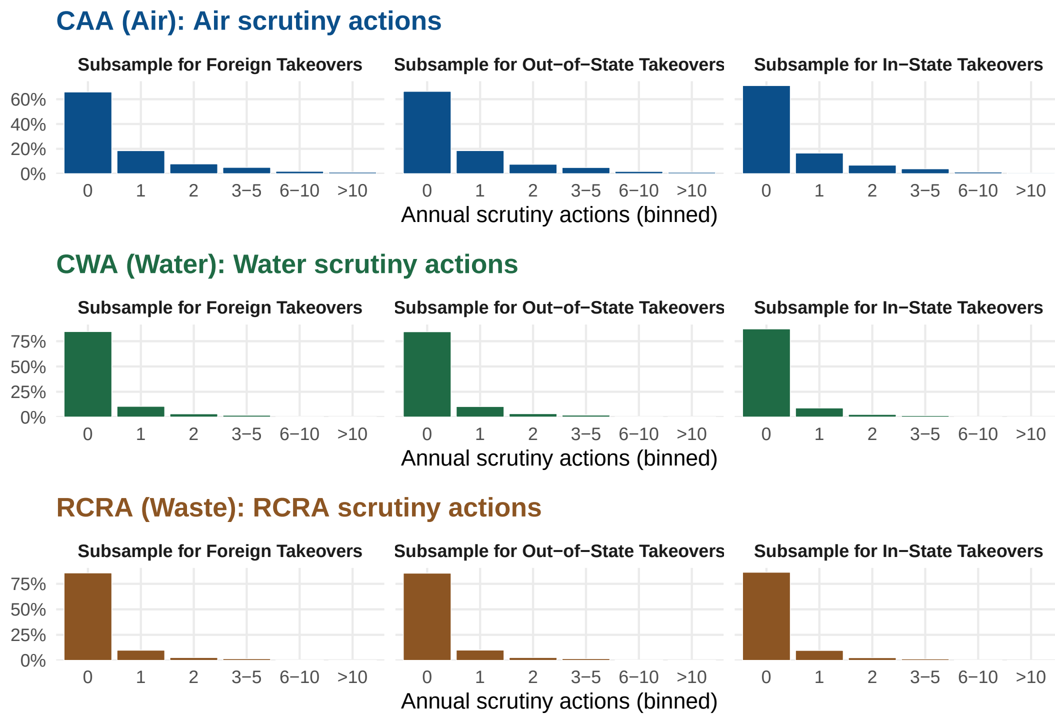


Figure D7: Distribution of the dependent variable: Full sample including original data linkage

D.6 State Partisanship Classification

Figure D8: **Classification of State Partisanship Based on Presidential Elections 1988–2024.**

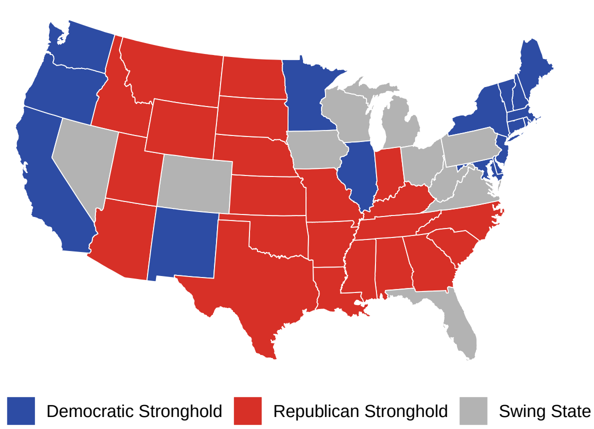


Table D2 reports the party winning each state in the ten presidential elections from 1988 to 2024, together with the resulting stronghold classification used throughout the analysis.

Table D2: State presidential election results and political classification, 1988–2024

State	1988	1992	1996	2000	2004	2008	2012	2016	2020	2024	Classification
Alabama	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Alaska	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Arizona	R	R	D	R	R	R	R	R	D	R	Rep. stronghold
Arkansas	R	D	D	R	R	R	R	R	R	R	Rep. stronghold
California	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Colorado	R	D	R	R	R	D	D	D	D	D	Swing state
Connecticut	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Delaware	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Florida	R	R	D	R	R	D	D	R	R	R	Swing state
Georgia	R	D	R	R	R	R	R	R	D	R	Rep. stronghold
Hawaii	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
Idaho	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Illinois	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Indiana	R	R	R	R	R	D	R	R	R	R	Rep. stronghold
Iowa	D	D	D	D	R	D	D	R	R	R	Swing state
Kansas	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Kentucky	R	D	D	R	R	R	R	R	R	R	Rep. stronghold
Louisiana	R	D	D	R	R	R	R	R	R	R	Rep. stronghold
Maine	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Maryland	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Massachusetts	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
Michigan	R	D	D	D	D	D	D	R	D	R	Swing state
Minnesota	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
Mississippi	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Missouri	R	D	D	R	R	R	R	R	R	R	Rep. stronghold
Montana	R	D	R	R	R	R	R	R	R	R	Rep. stronghold
Nebraska	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Nevada	R	D	D	R	R	D	D	D	D	R	Swing state
New Hampshire	R	D	D	R	D	D	D	D	D	D	Dem. stronghold
New Jersey	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
New Mexico	R	D	D	D	R	D	D	D	D	D	Dem. stronghold
New York	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
North Carolina	R	R	R	R	R	D	R	R	R	R	Rep. stronghold
North Dakota	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Ohio	R	D	D	R	R	D	D	R	R	R	Swing state
Oklahoma	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Oregon	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
Pennsylvania	R	D	D	D	D	D	D	R	D	R	Swing state
Rhode Island	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
South Carolina	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
South Dakota	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Tennessee	R	D	D	R	R	R	R	R	R	R	Rep. stronghold
Texas	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Utah	R	R	R	R	R	R	R	R	R	R	Rep. stronghold
Vermont	R	D	D	D	D	D	D	D	D	D	Dem. stronghold
Virginia	R	R	R	R	R	D	D	D	D	D	Swing state
Washington	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
Washington, D.C.	D	D	D	D	D	D	D	D	D	D	Dem. stronghold
West Virginia	D	D	D	R	R	R	R	R	R	R	Swing state
Wisconsin	D	D	D	D	D	D	D	R	D	R	Swing state
Wyoming	R	R	R	R	R	R	R	R	R	R	Rep. stronghold

Note: D = Democratic, R = Republican. Source: Ballotpedia, *Presidential voting history by state* (1988–2024).

Data source and coding rule. Election results are drawn from Ballotpedia's state-level presidential voting history, covering the ten elections held between 1988 and 2024.[68] Each state (and the District of Columbia) is assigned to one of three time-invariant categories based on the number of elections won by each party over this period. A state is classified as a *Republican stronghold* if the Republican candidate carried it in at least eight of the ten elections, and as a *Democratic stronghold* if the Democratic candidate did so in at least eight elections. All remaining states—those in which neither party won more than seven elections—are classified as *swing states*. Wisconsin, which Democrats won in eight elections, is reclassified as a swing state to reflect the pronounced rightward shift observed in its two most recent elections (2016 and 2024), both won by the Republican candidate. Under this rule, 17 states plus D.C. are Democratic strongholds, 23 states are Republican strongholds, and 10 states are swing states.

[68] See https://ballotpedia.org/Presidential_voting_history_by_state.

D.7 Additional Results

Figure D9: **Effects of ownership takeovers on air scrutiny actions by state partisanship.**

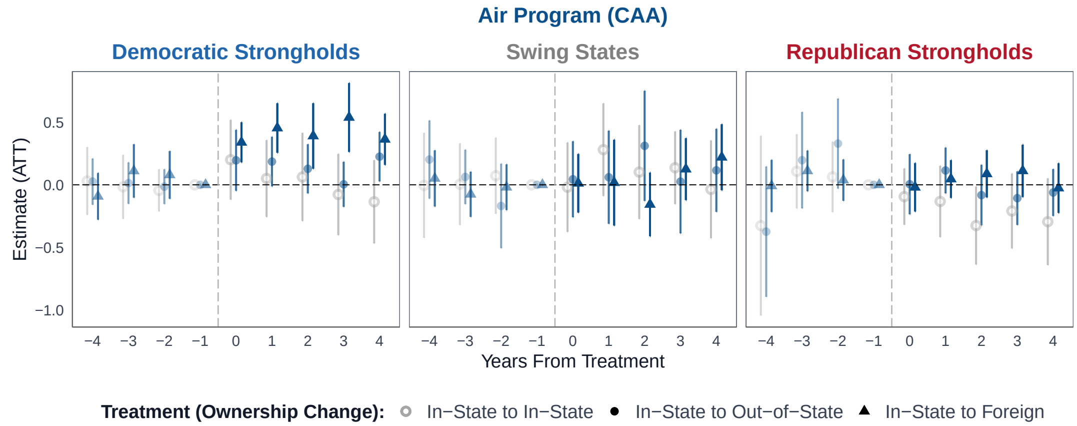
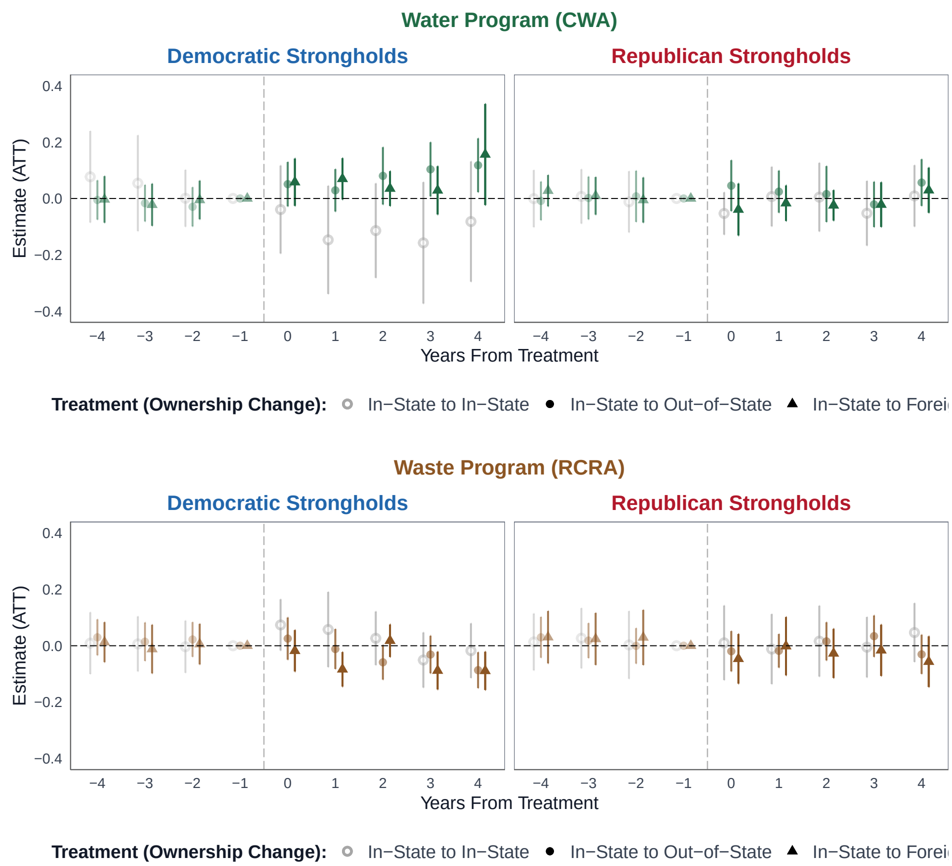


Figure D10: **Results for the CWA and RCRA samples by state partisanship.**



D.7.1 Results Using Full Sample (Including Original Linkage)

Figure D11: **Baseline results from the full sample.**

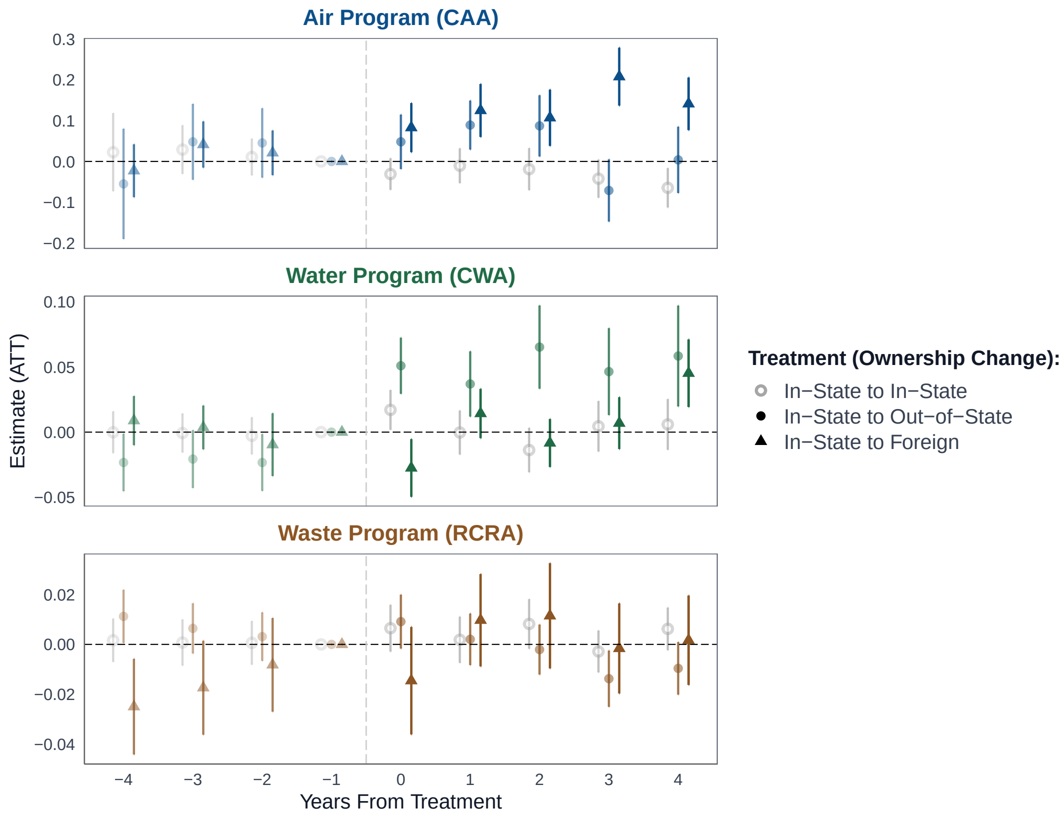
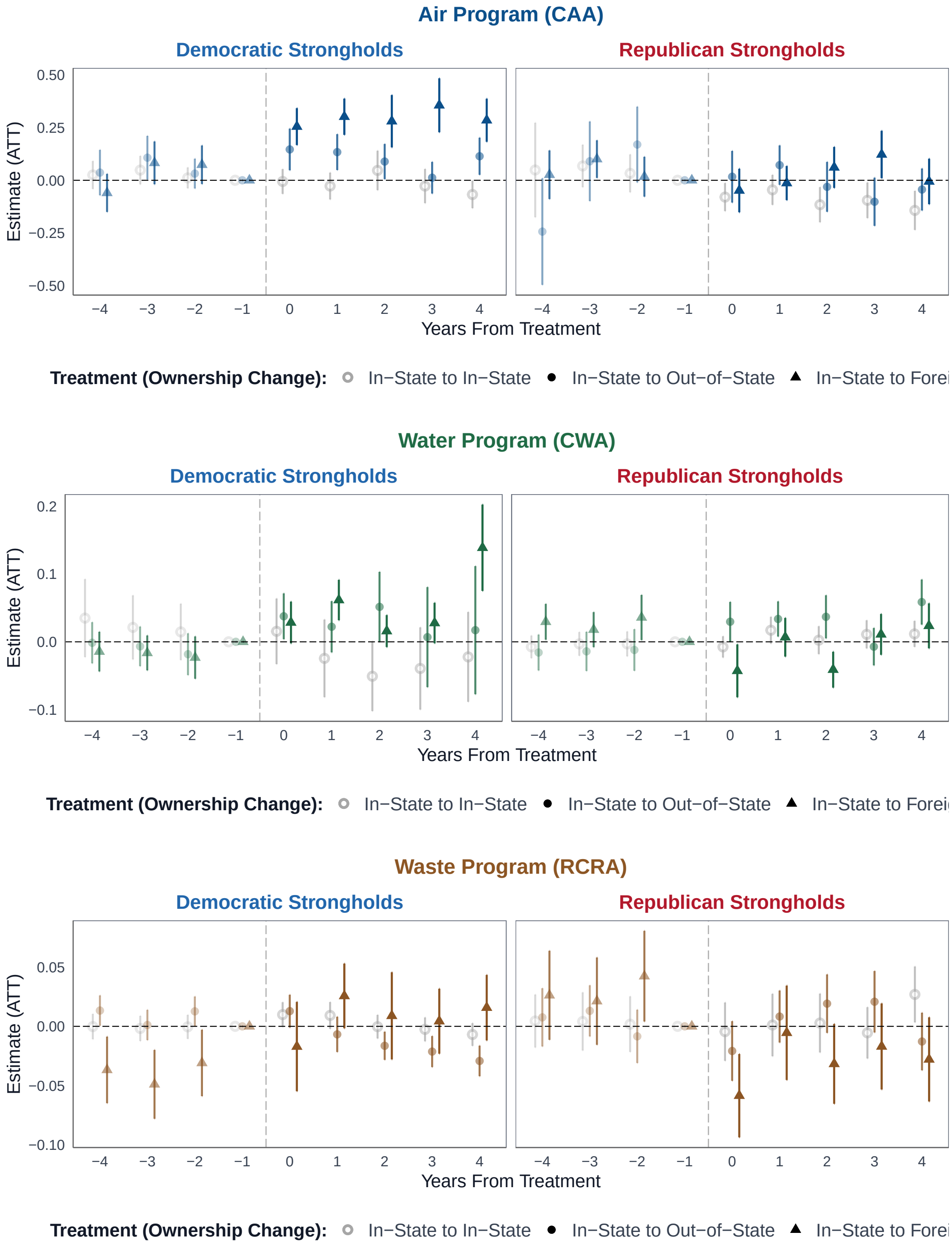


Figure D12: **State heterogeneity results from the full sample.**



D.7.2 Differences between external takeover effects vs. in-state takeover effects

Figure D13: **Differences between external takeover effects vs. in-state takeover effects: Baseline**

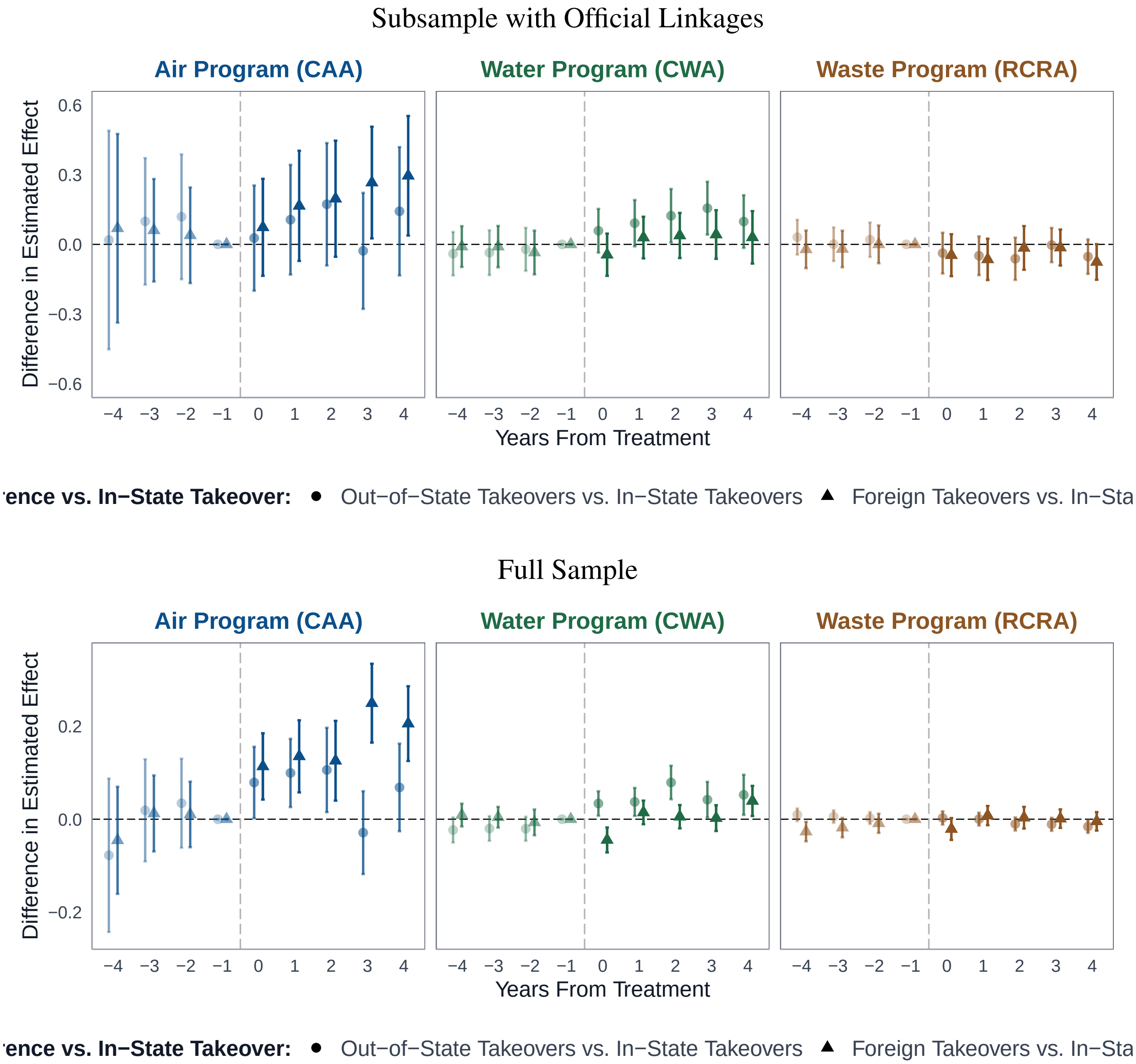
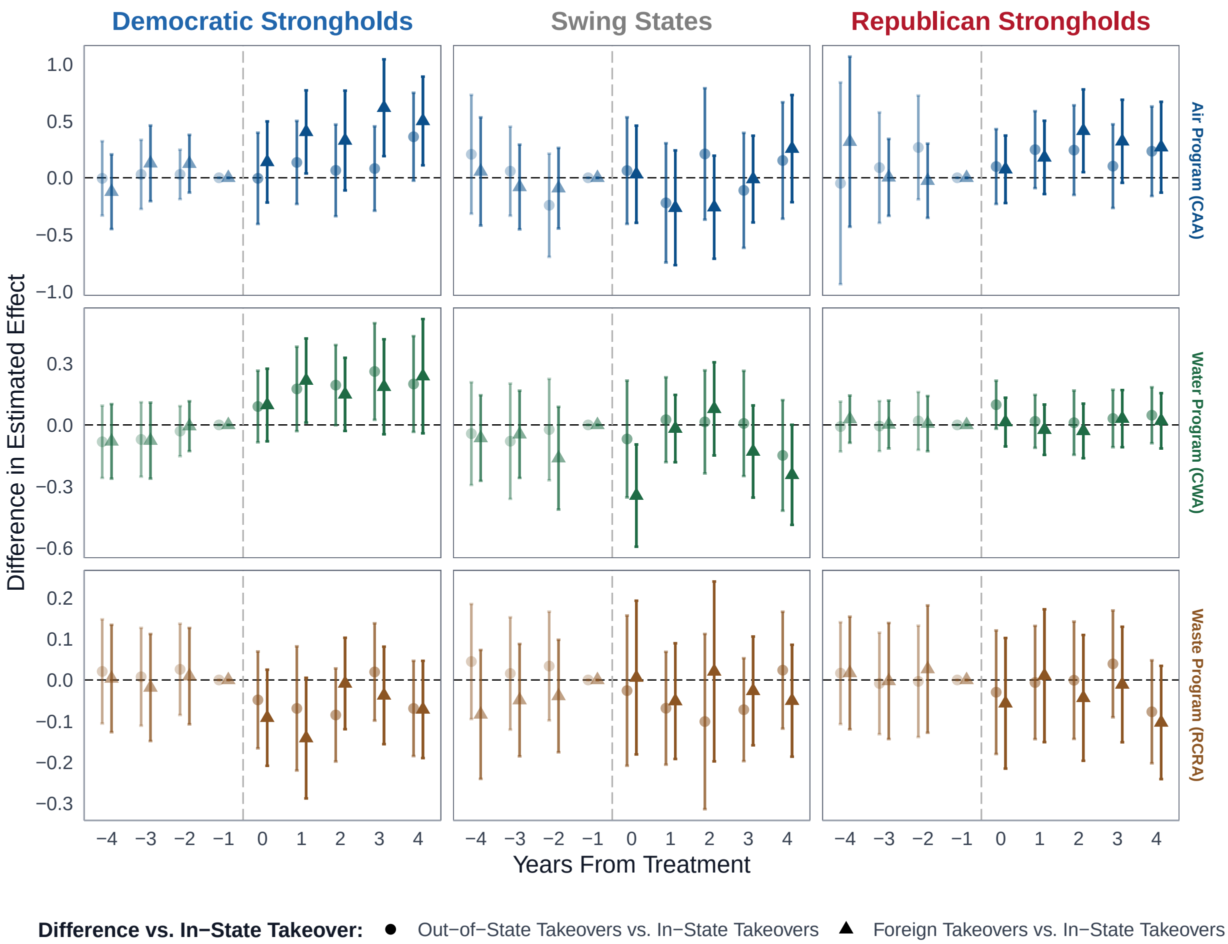


Figure D14: **Differences between external takeover effects vs. in-state takeover effects: State Heterogeneity**

Subsample with Official Linkages



Full Sample

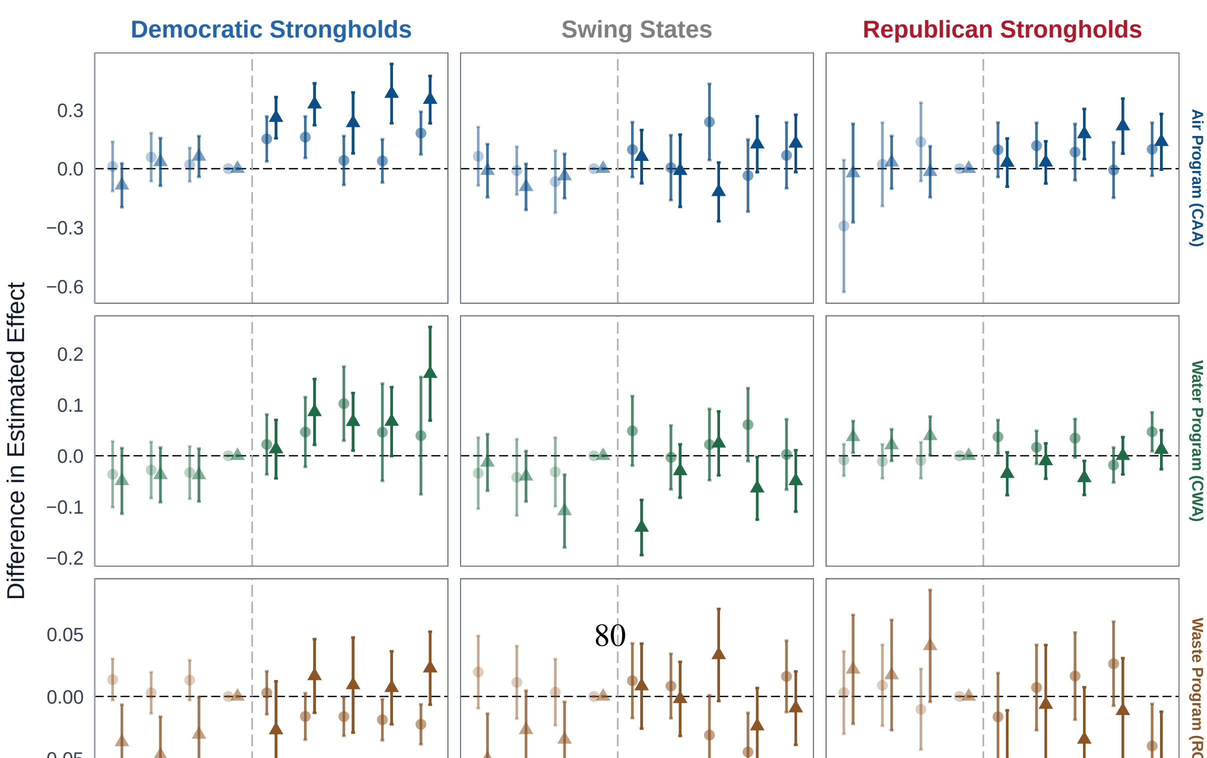
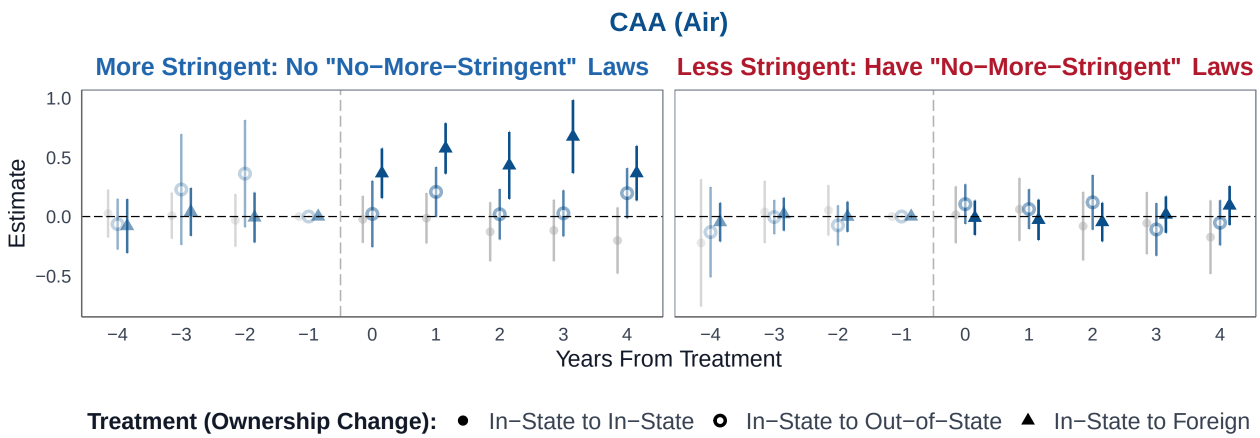


Figure D15: **State heterogeneity by an alternative measure of the stringency of state-level environmental regulatory standards.**

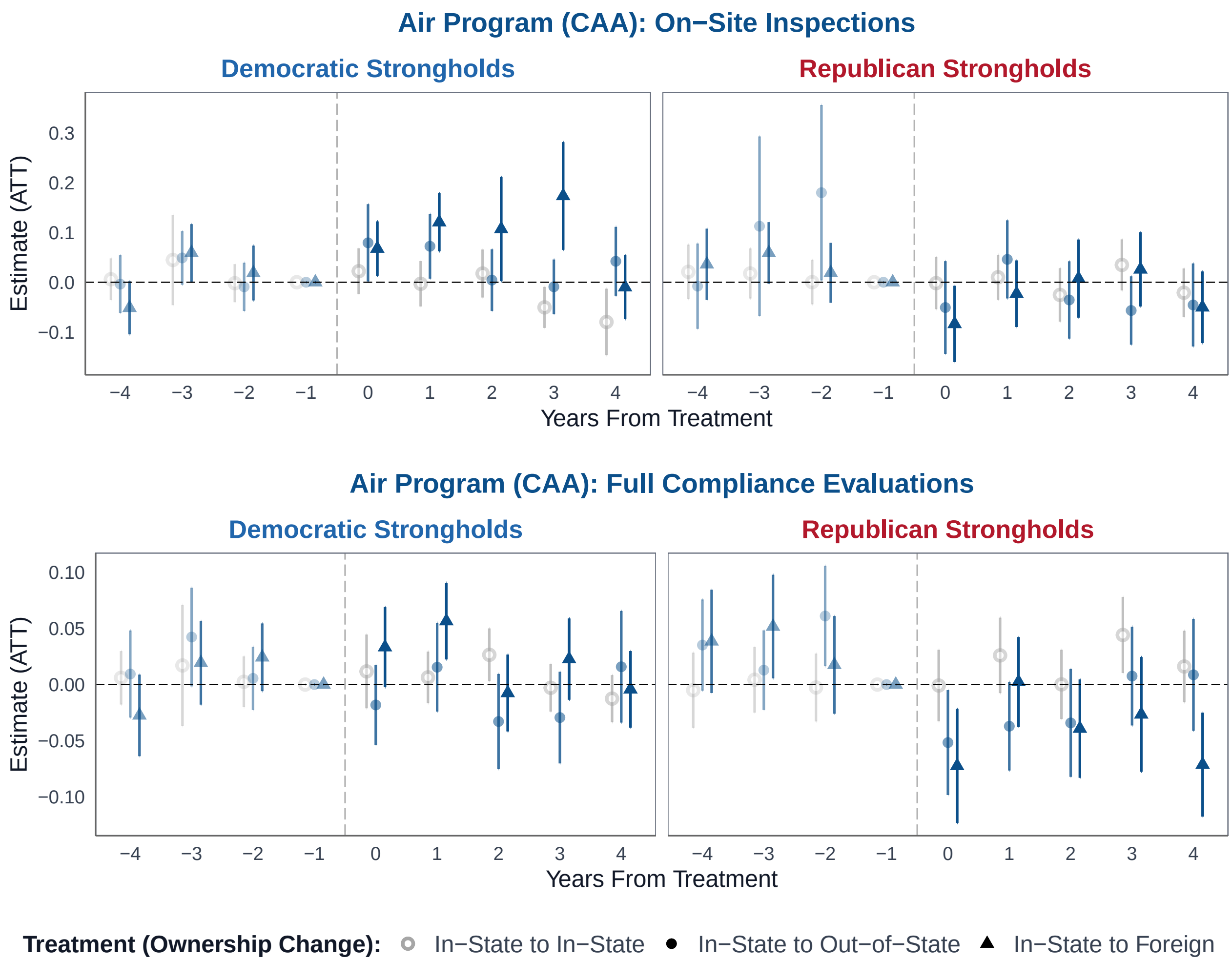


Notes: The stringency measure is based on whether a state has enacted a state law that prohibits state regulations from being more stringent than federal requirements under the CAA, CWA, or RCRA during the period from 1989 to 2024.

D.7.3 Intensity of Inspections

The figure below presents the results by scrutiny intensity. In Democratic strongholds, I find increases in both physical inspections and Full Compliance Evaluations (FCEs) following foreign takeovers. Out-of-state takeovers show no positive and statistically significant effects in any post-treatment year, and the effects turn negative in the second year after treatment. In Republican strongholds, I find no significant positive effects on either physical inspections or FCEs following either foreign or out-of-state takeovers.

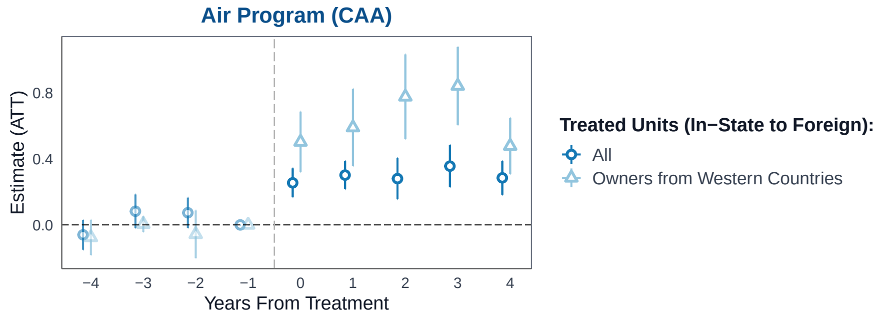
Figure D16: **Effects of foreign and out-of-state takeovers on scrutiny actions by intensity and state partisanship (CAA sample).**



The evolution of effects across post-treatment years is consistent with the theoretical predictions on the temporal dynamics of information-driven discrimination. The effect on physical inspections is positive and statistically significant in the treatment year and the three subsequent years, but becomes statistically indistinguishable from zero by the fourth year. For FCEs, the effect is positive and statistically significant in the treatment year and the first year after treatment, after which the estimates decline in magnitude and lose statistical significance. Both patterns are consistent with information-driven bias attenuating as regulators learn about firms' compliance and update their beliefs.

D.7.4 National Origin

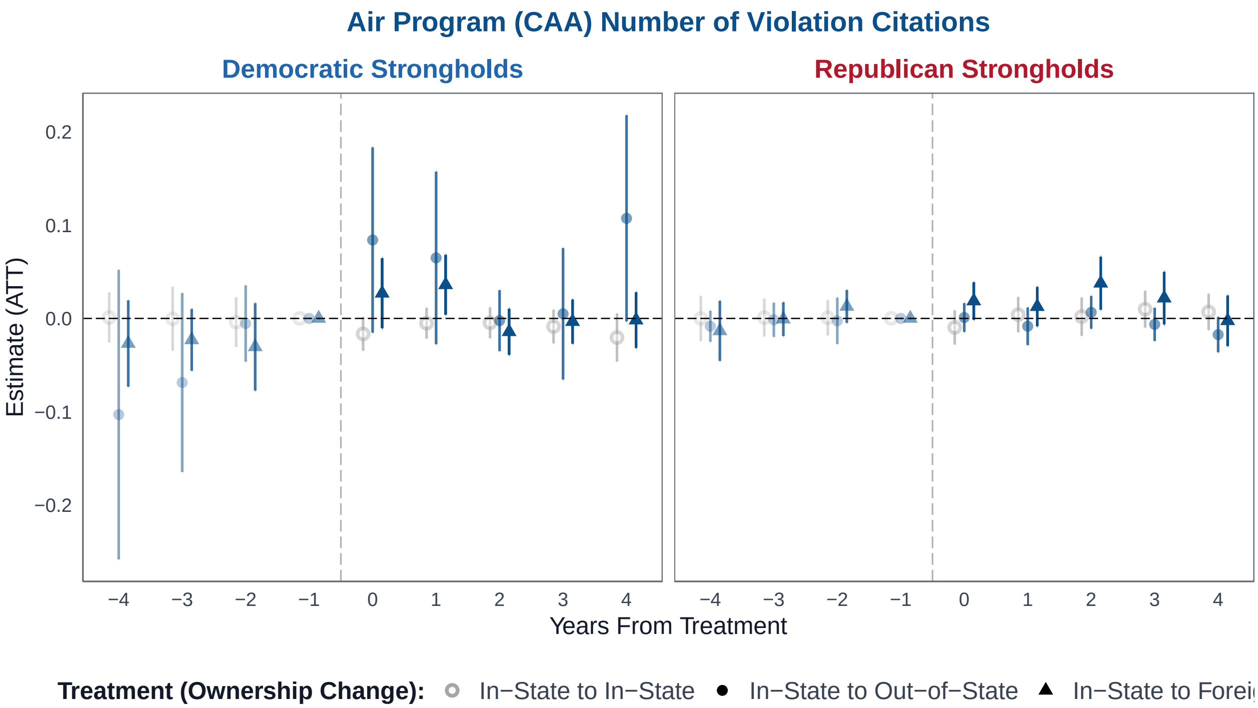
Figure D17: **Effects of foreign takeovers on air program (CAA) scrutiny actions by national origin within Democratic strongholds.**



D.8 Mechanism Tests

D.8.1 Effects of Takeovers on Violations

Figure D18: **Effects of takeovers on violation citations (air program).**

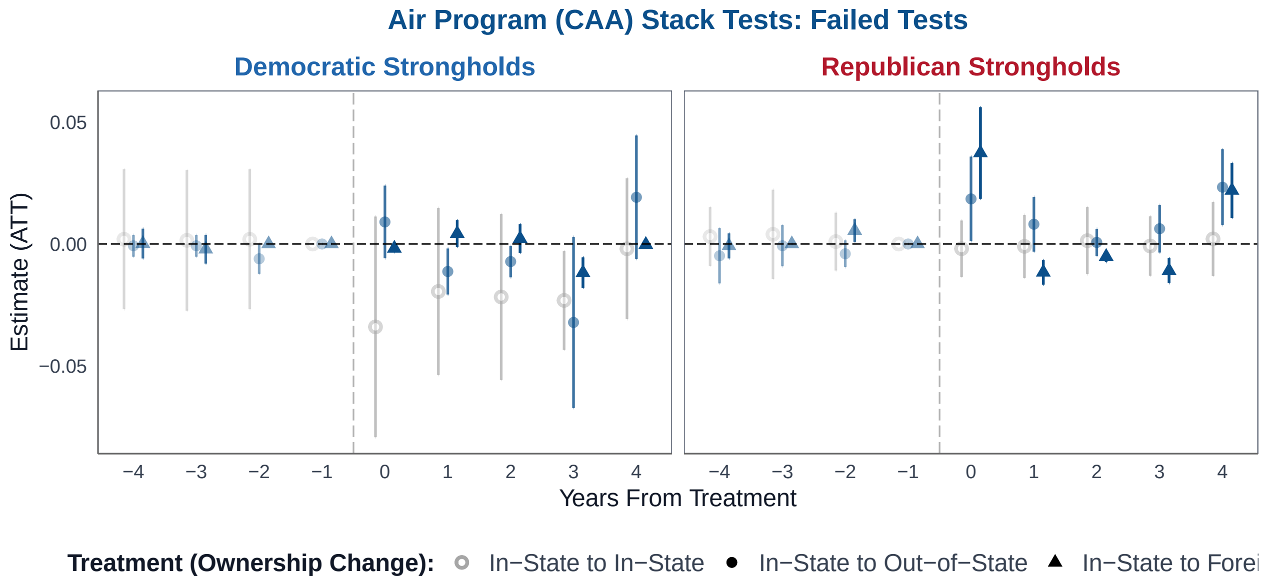


Notes: These violation citations cover all non-punitive measures regulators take after identifying a violation—including citations, warning letters, notices, and tickets, but excluding monetary and non-monetary penalties—which the USEPA collectively labels "informal enforcement actions."

D.8.2 Effects of Takeovers on Self-Reported Violations

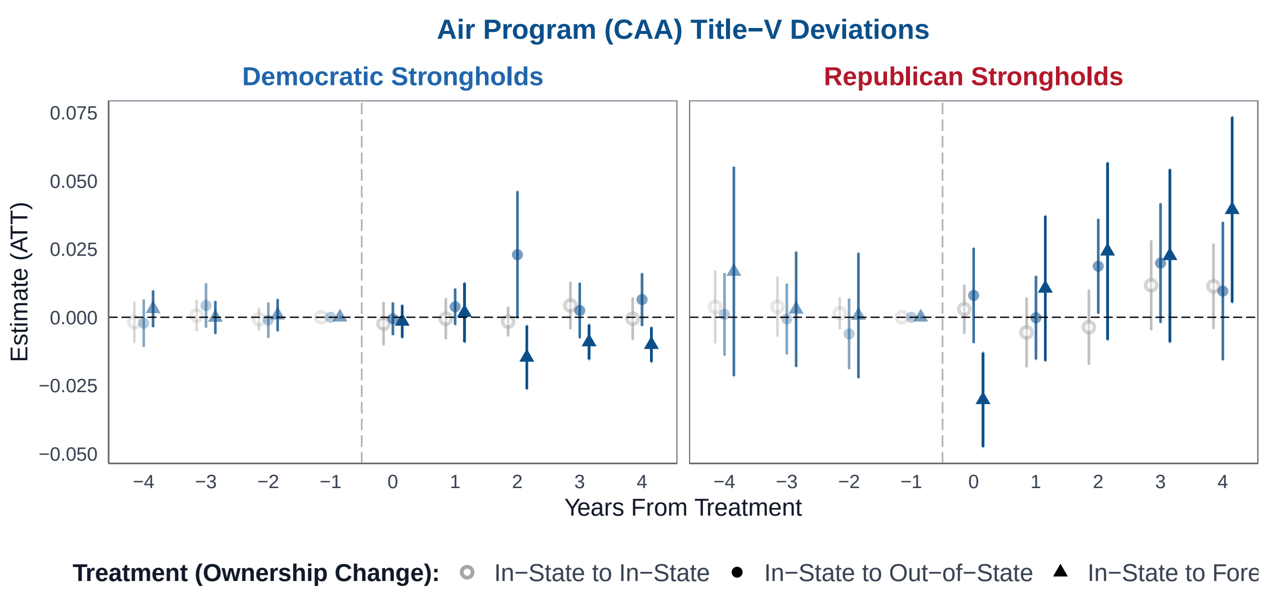
D.8.3 Effects of Takeovers on Designated Facility Size

Figure D19: **Effects of takeovers on compliance: Number of failed stack tests.**



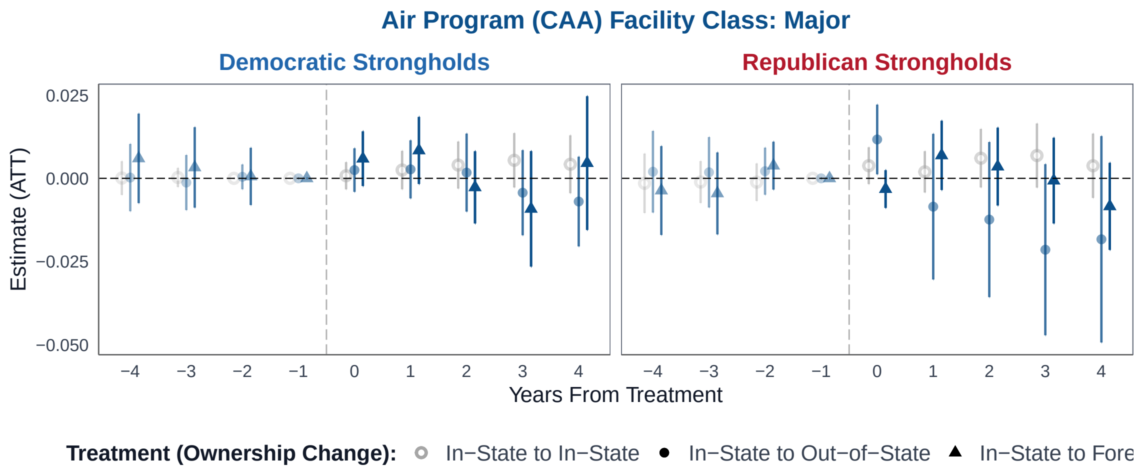
Notes: Dependent variable: number of reported failed stack tests per facility-year.

Figure D20: **Effects of takeovers on compliance: Number of Title-V deviations.**



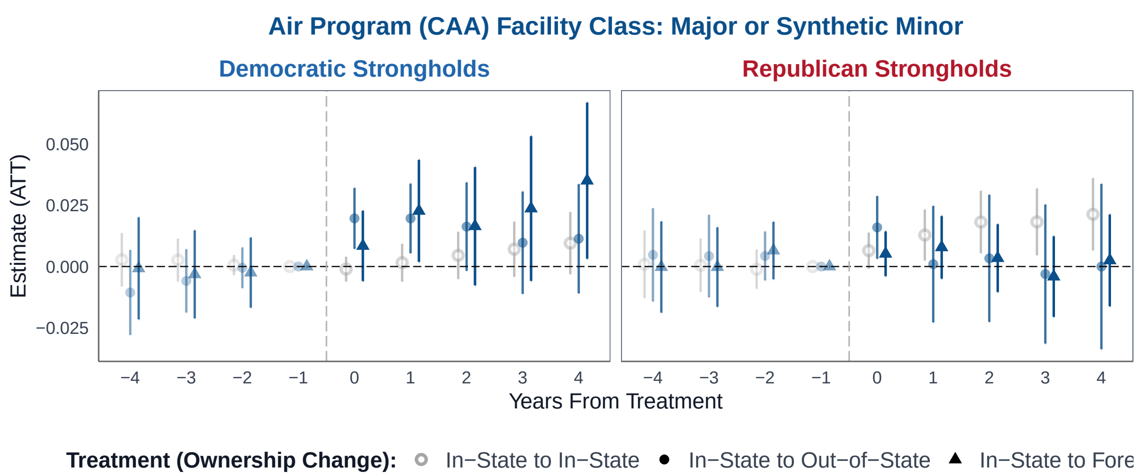
Notes: Dependent variable: number of reported Title-V deviations per facility-year.

Figure D21: **Effects of takeovers on designated facility class: Major vs. Synthetic Minor or Minor.**



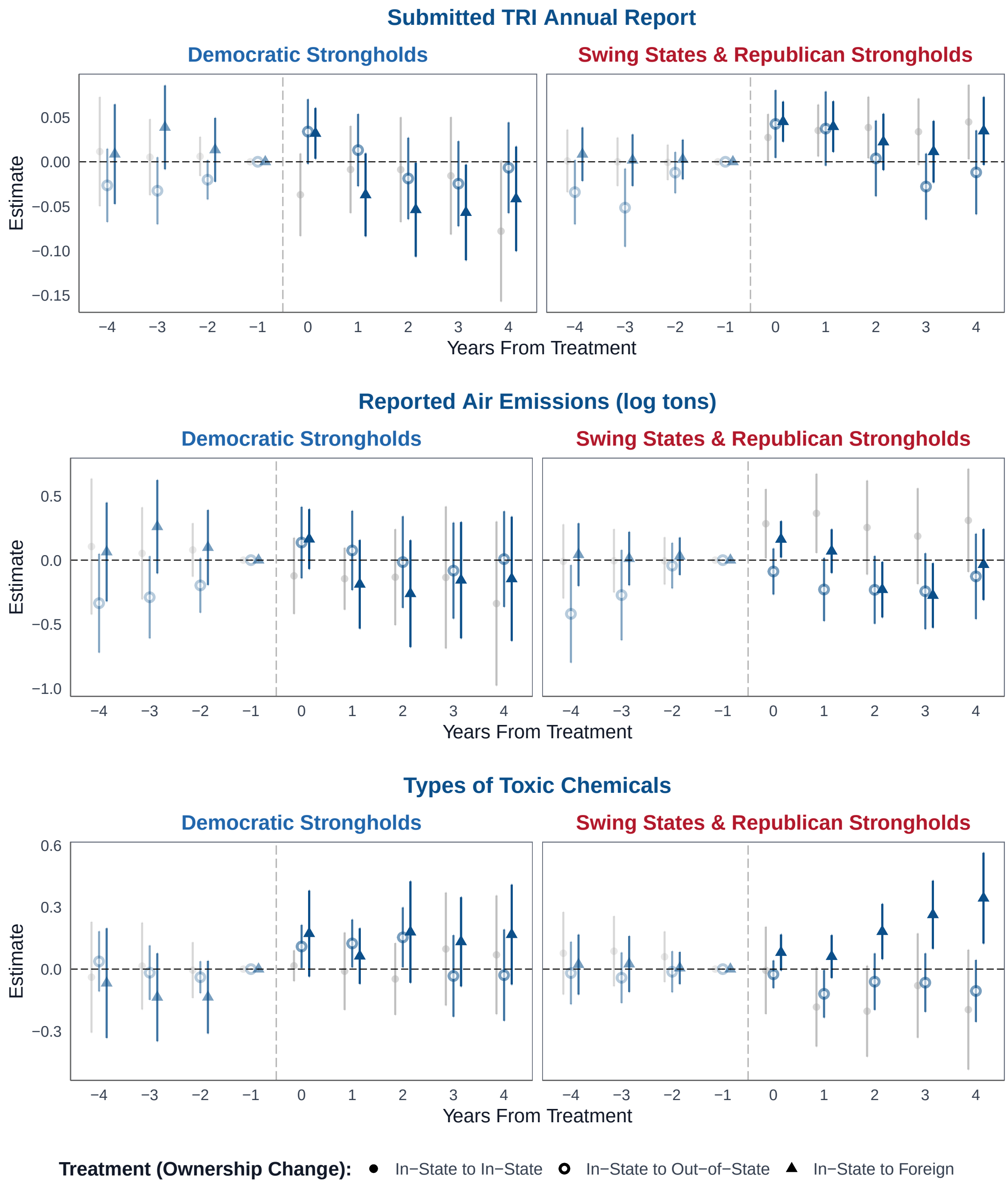
Notes: The dependent variable is a binary indicator for the facility being designated as Major in the CAA program.

Figure D22: **Effects of takeovers on designated facility class: Major or Synthetic Minor vs. Minor.**



Notes: The dependent variable is a binary indicator for the facility being designated as Major or Synthetic Minor in the CAA program.

Figure D23: **Effects of takeovers on expansion: Air emission activities reported via TRI.**



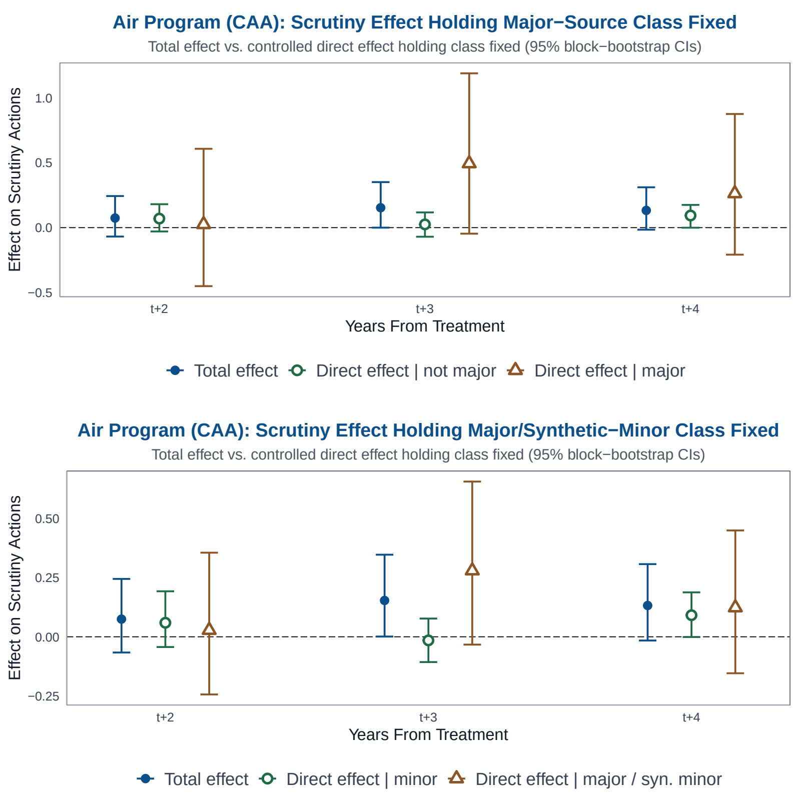
D.8.4 Causal Mediation Analysis

I estimate the *controlled direct effect* (CDE) of a takeover on scrutiny actions while holding facility class fixed (VanderWeele, 2015). Using the matched samples underlying the takeover ATTs, I regress the number of scrutiny actions $S_{i,s}$ on the takeover indicator T_i, the facility's post-takeover class M_i measured in the year before the outcome, their interaction, and the pre-treatment covariates in Table D1. I estimate the model separately by horizon s using matching weights:

$$S_{i,s} = \alpha + \tau\, T_i + \delta\, M_i + \gamma\,(T_i M_i) + X_i'\beta + \varepsilon_{i,s}.$$

The controlled direct effect $\mathrm{CDE}(m) = \tau + \gamma m$ is the takeover effect on scrutiny holding facility class at the lower ($m = 0$) or higher ($m = 1$) tier, while γ tests whether the effect varies by facility class. Unlike a natural-indirect-effect decomposition, the CDE remains valid under treatment-induced confounding between facility class and scrutiny—a first-order concern because takeovers may also expand production—requiring only that no *unmeasured* confounder of that relationship remain conditional on X_i. I report 95% CIs from a block bootstrap over matched sets for two facility-class indicators: major versus below and major-or-synthetic-minor versus minor (Figure D24).[69]

Figure D24: **Controlled direct effect of takeovers on scrutiny actions, holding major-facility class fixed.**



Notes: Points report the total effect and the controlled direct effects holding class at each tier, by year since treatment; bars are 95% block-bootstrap CIs over matched sets. Covariates follow Table D1.

[69] I omit a proportion-mediated statistic because the total effect lies near the threshold of significance, making the ratio of mediated to total effect numerically unstable.

D.8.5 Out-of-State U.S. Companies vs. Foreign MNCs

Figure D25: **Placebo test: Out-of-state companies restricted to U.S.-based MNCs.**

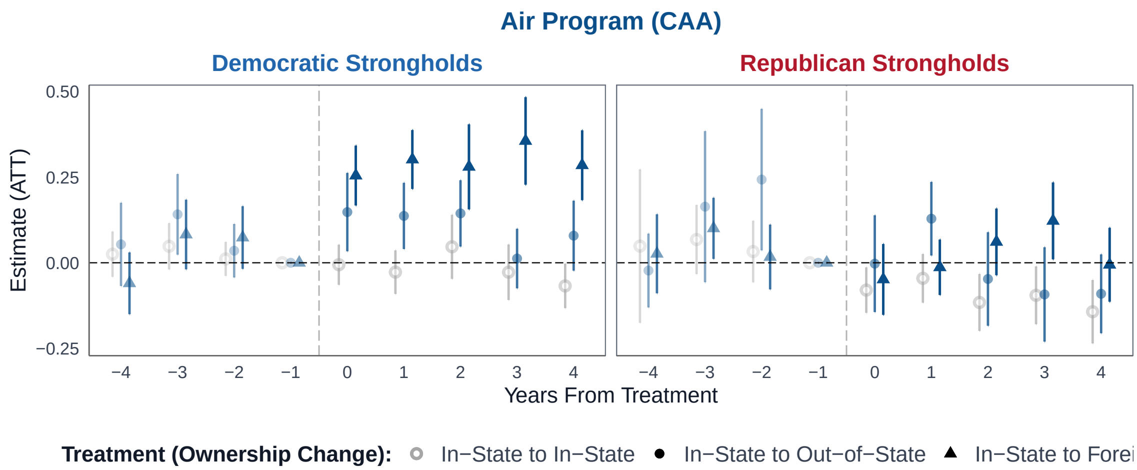
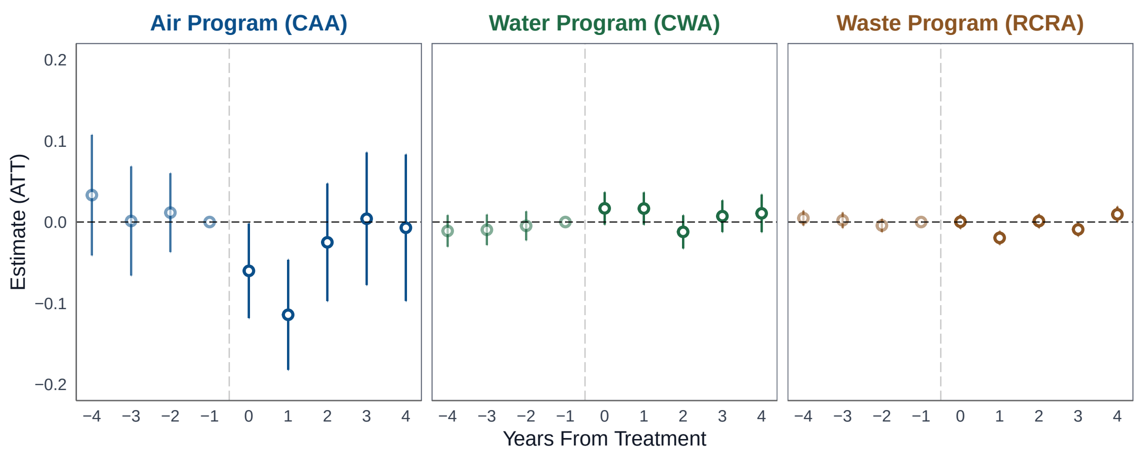


Figure D26: **Results from a further placebo test: Ownership change from out-of-state parent to foreign parent.**



D.8.6 Citizen Complaints?

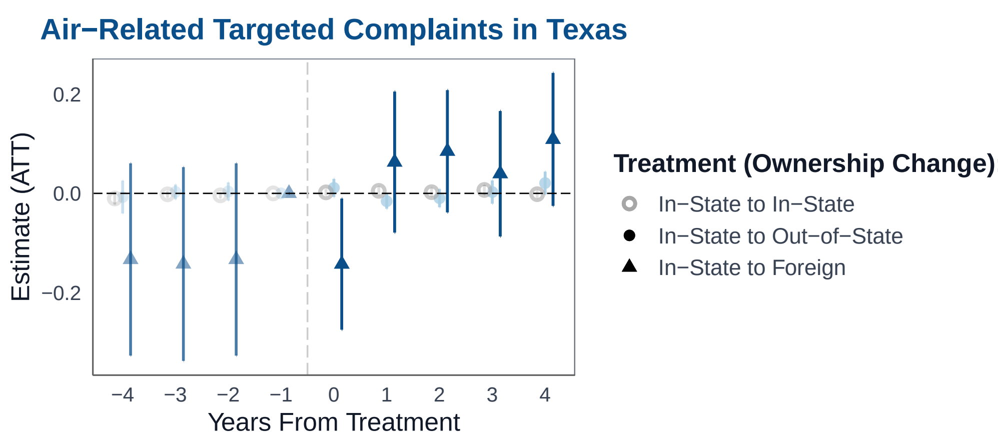
The immediate increase in scrutiny actions following foreign takeovers is unlikely to be driven by reactions to citizen complaints.

Complaints inform but do not automatically trigger scrutiny because agencies receive large volumes of complaints, many of which are noisy or legally irrelevant (Majumder et al., 2025). Staff must assess whether alleged conduct falls within their jurisdiction and plausibly violates regulations. Even credible complaints may not lead to scrutiny, as regulators may instead contact facilities informally, coordinate with other authorities, or defer action to align with scheduled scrutiny actions and resource constraints (U.S. Environmental Protection Agency, 2024, 2025). Moreover, citizens may require time to form perceptions of foreign ownership, observe potential violations or environmental incidents, and file complaints. If so, citizen complaints are unlikely to drive short-run increases in scrutiny actions.

To test whether citizen complaints respond more slowly than regulators, I examine data from Texas covering all complaints submitted to the Texas Commission on Environmental Quality (TCEQ) since 2000. To my knowledge, Texas is the only state that provides comprehensive case-level complaint data linked to both enforcement actions and facility characteristics.[70] I link the Texas data to the D&B database to construct ownership histories and adapt the baseline identification strategy to estimate the ATTs of foreign and out-of-state takeovers on the number of complaints targeting a facility, focusing on CAA-regulated facilities.

Figure D27 presents the results. Citizen complaints decrease during the treatment year and weakly increase in later years. This timing mismatch suggests that citizens require more time to perceive foreign ownership, observe violations, and file complaints, and are therefore unlikely to drive the short-run increase in scrutiny actions.

Figure D27: **Effects of foreign and out-of-state takeovers on citizen complaints in Texas.**



[70]Ideally, this test would be conducted in a Democratic-majority state, where information-driven discrimination is most likely to arise. However, given data availability, Texas provides the only feasible setting. Moreover, if anything, citizens in Republican-leaning contexts may be more likely to exhibit bias against foreign-owned firms, strengthening the role of complaints as a potential mechanism.

D.8.7 Effects of Foreign and Out-of-State Takeovers on Penalties?

Figure D28: **Effects of takeovers on penalties: Number of monetary penalties.**

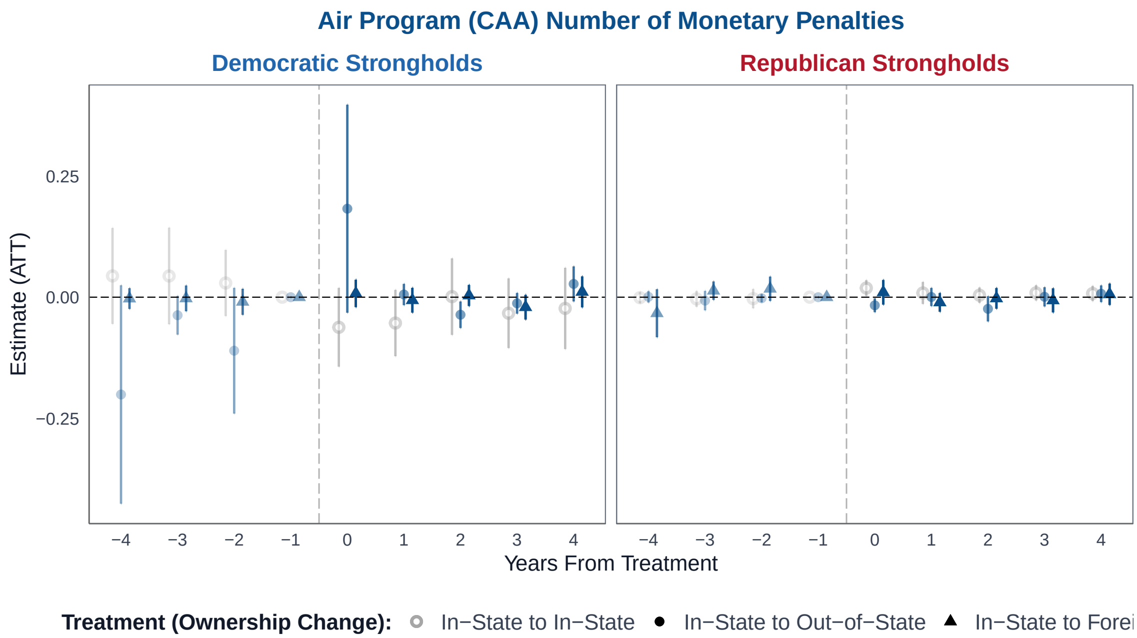
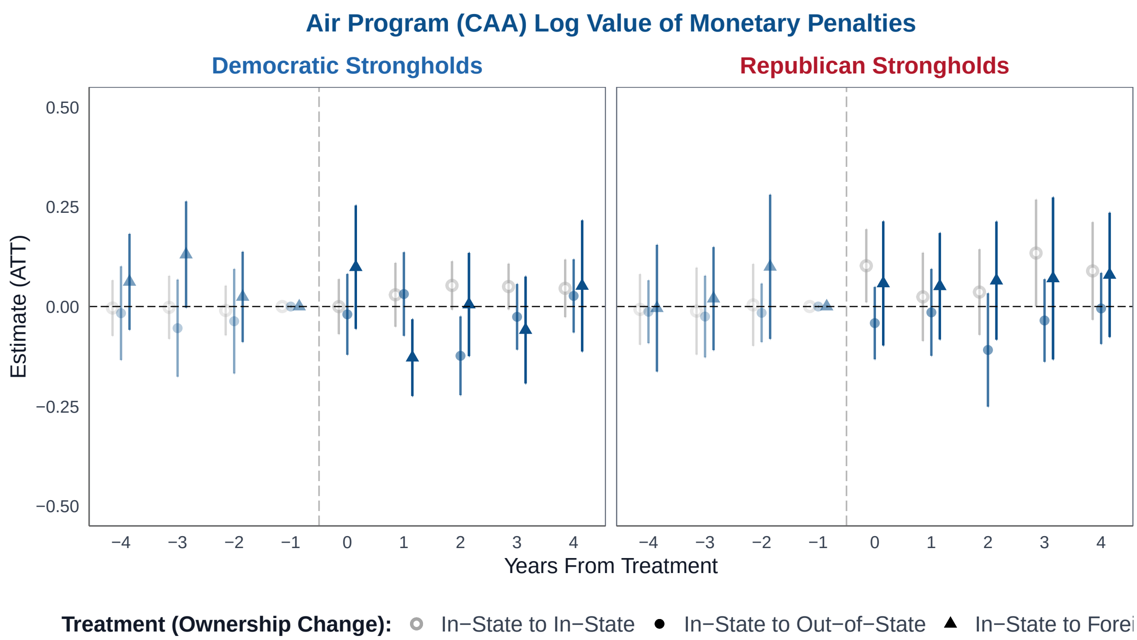


Figure D29: **Effects of takeovers on penalties: Log value of monetary penalties.**



D.9 Robustness Checks

I present the following robustness checks. To save space, I only report the state heterogeneity results here.

- TRI facilities only: Figure D30
- binary indicator for any scrutiny action as the dependent variable: Figure D31
- bootstrap standard errors: Figure D32
- alternative covariates: Figures D33 and D34
- alternative lag structures (L): Figures D35 and D36
- alternative estimator, the doubly robust DiD estimator proposed by Sant'Anna and Zhao (2020): Figure D38
- alternative estimator, Fixed-Effects Counterfactual Estimator implemented in `fect` (Liu et al., 2022): Figure D39
- re-estimating the in-state takeover specifications after dropping all facilities first treated in 2014, 2015, or 2019: Figure D40

Figure D30: **Robustness check: Results from the TRI sample by state partisan stronghold classification.**

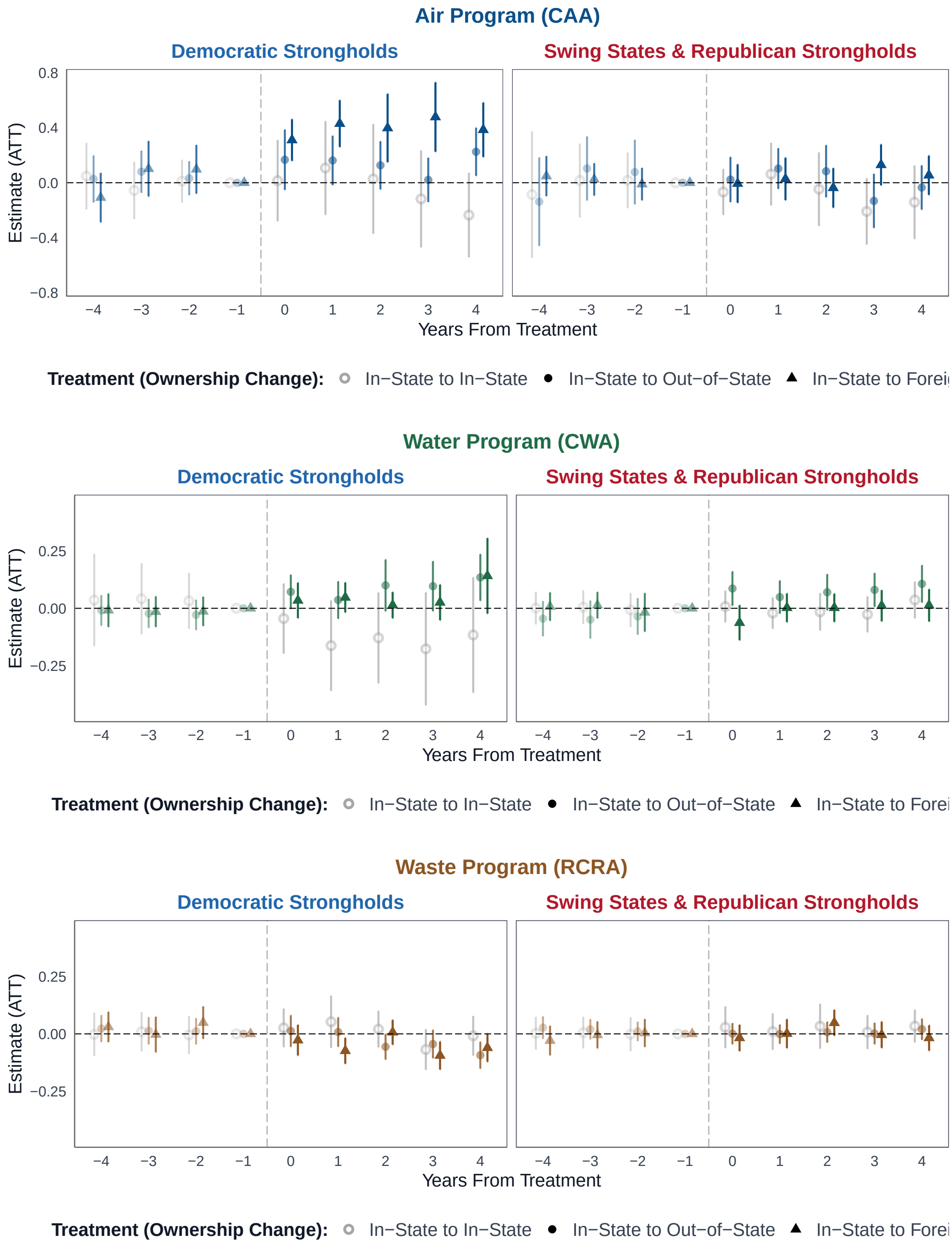


Figure D31: **Robustness check: Binary dependent variable.**

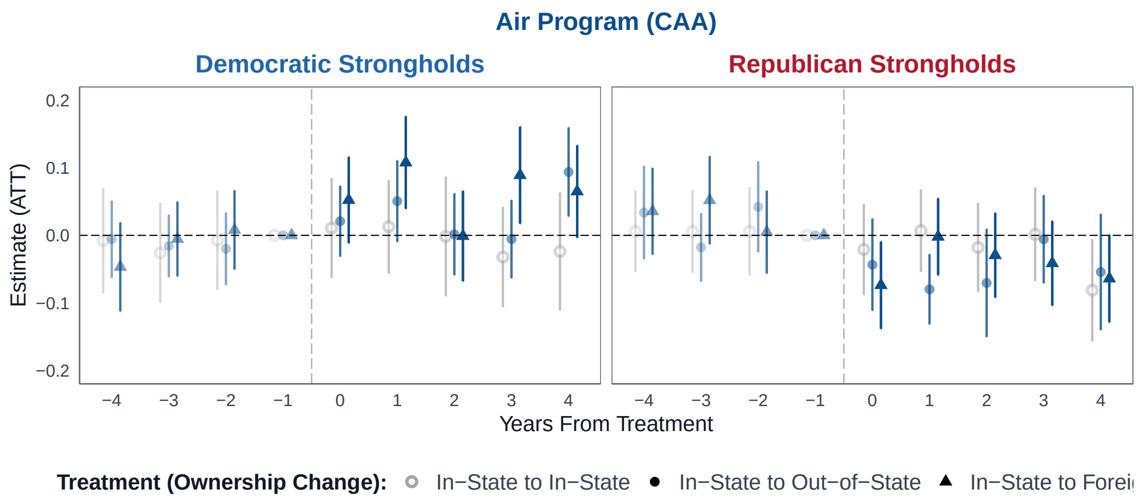


Figure D32: **Robustness check: Bootstrap standard errors.**

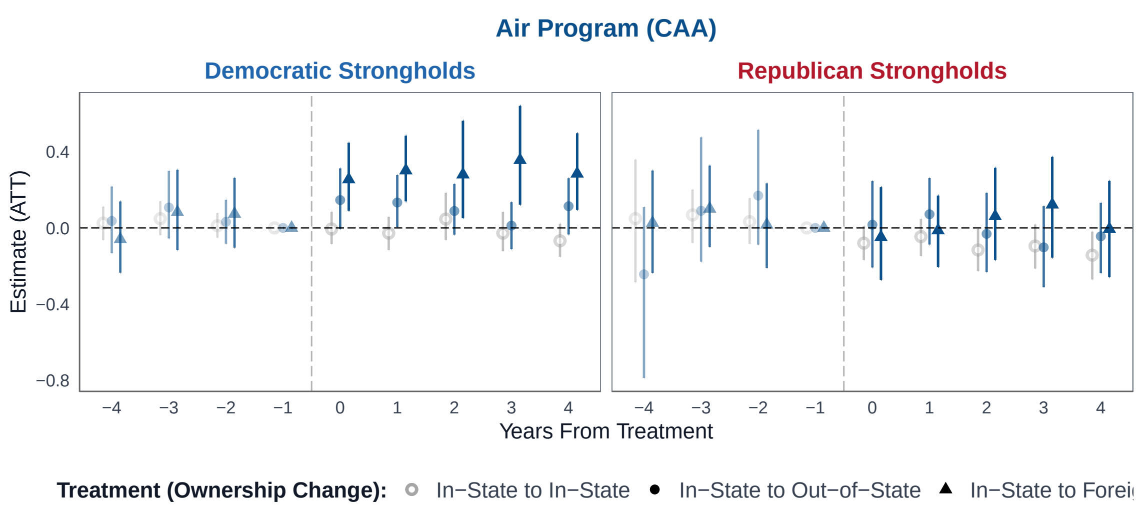
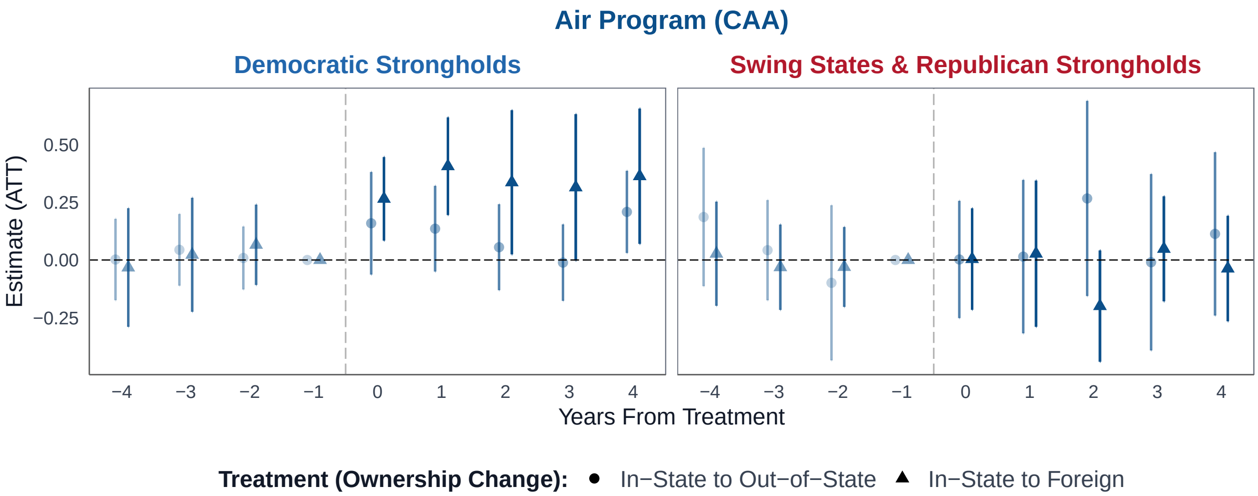
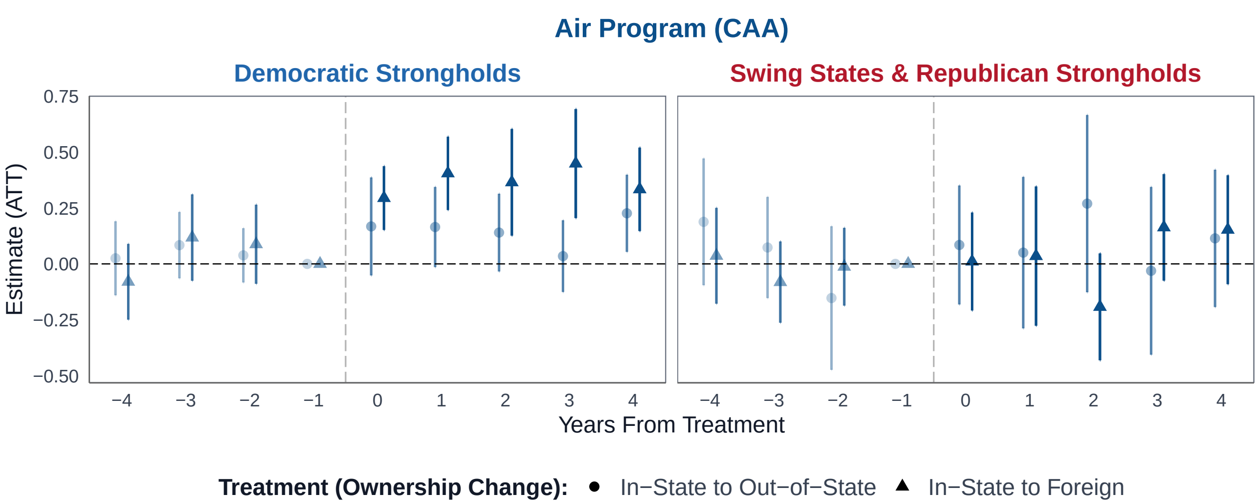


Figure D33: **Robustness check for baseline results in Figure 2: additional covariates.**



Notes: Covariates now include whether the county had a designated status of "non-attainment" for any listed type of pollutant in each year during the placebo period, plus a time-invariant opening year variable and a time-invariant indicator for whether the facility is located in an "Environmental Justice" designated census tract. Other model details follow Figure 2.

Figure D34: **Robustness check for baseline results in Figure 2: additional covariates.**



Notes: Covariates now include whether the county had a designated status of "non-attainment" for any listed type of pollutant in each year during the placebo period.

Figure D35: **Robustness check for baseline results in Figure 2: 5-year lags.**

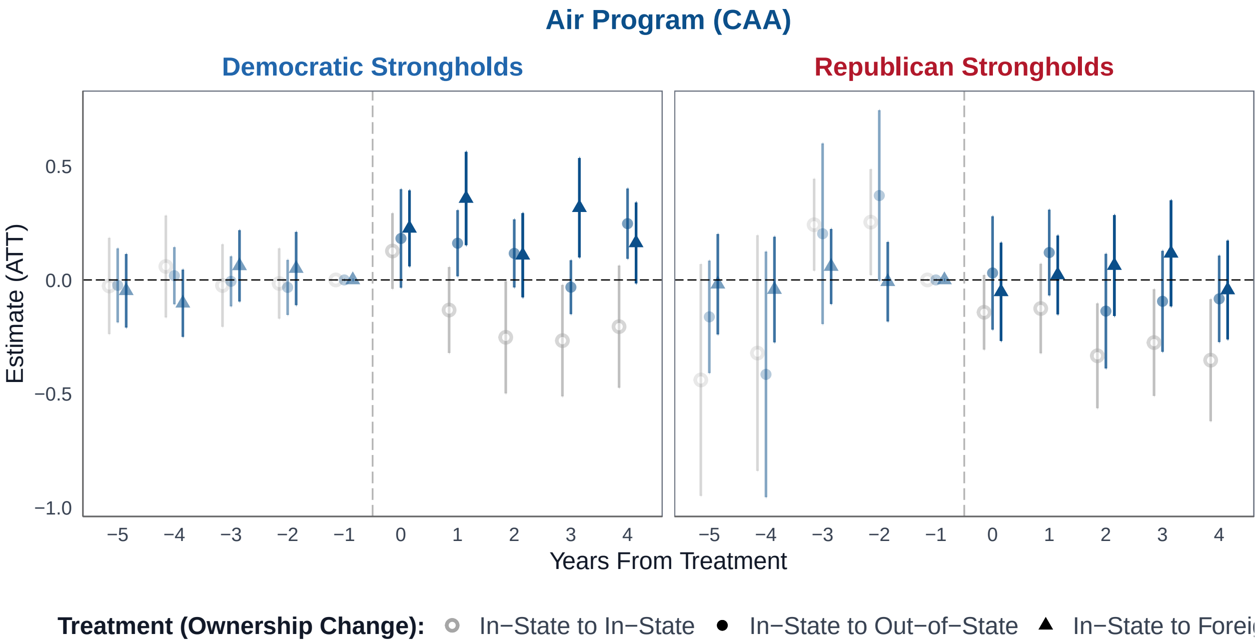


Figure D36: **Robustness check for baseline results in Figure 2: 6-year lags.**

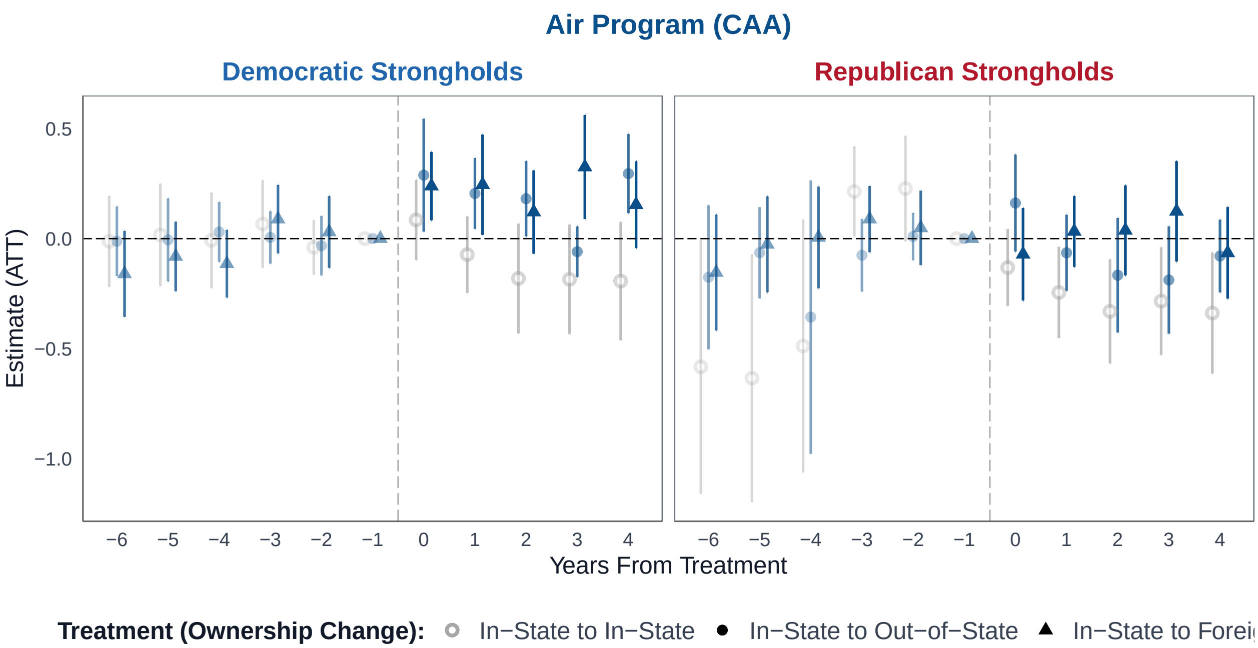


Figure D37: **Robustness check for baseline results in Figure 2: 10 post-treatment years.**

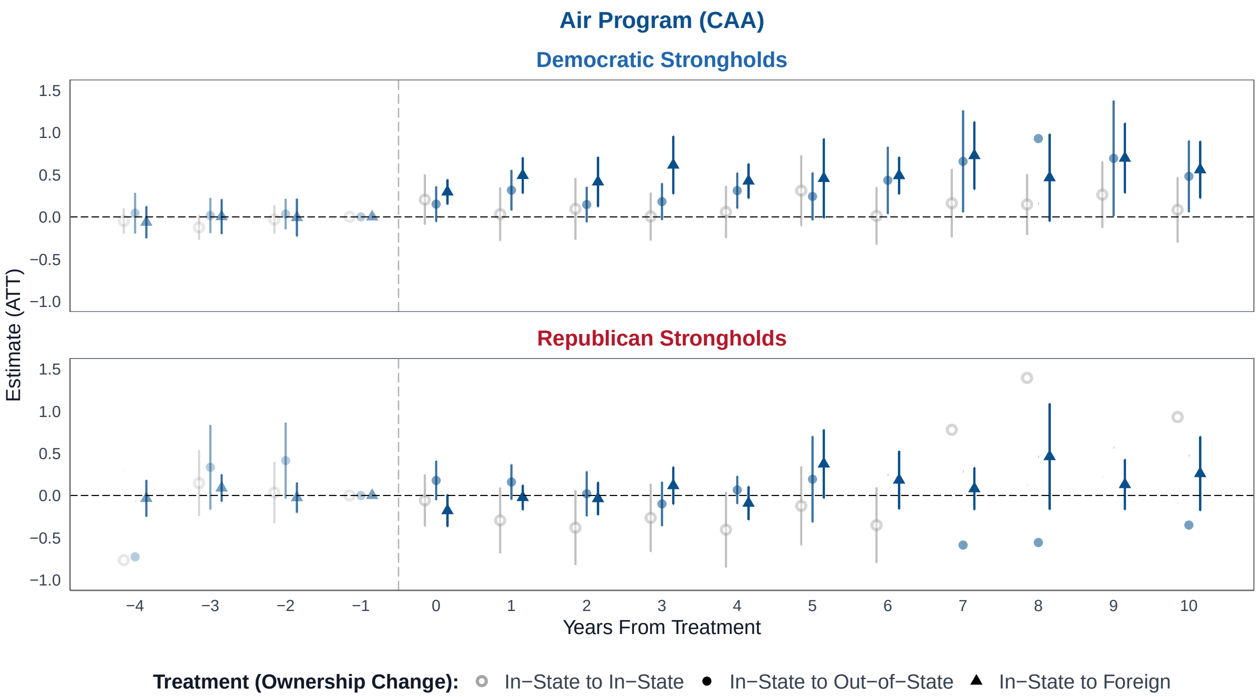
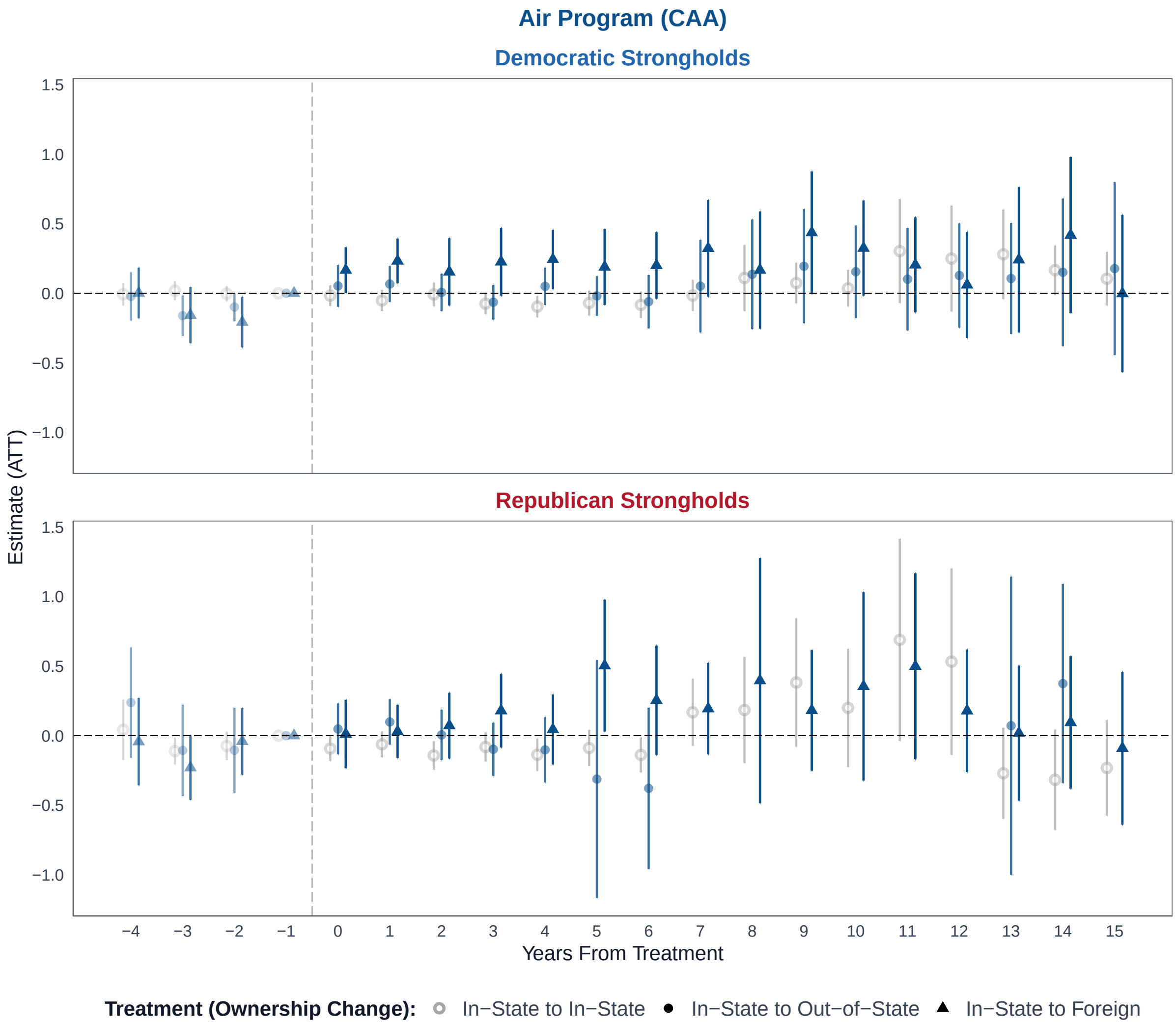
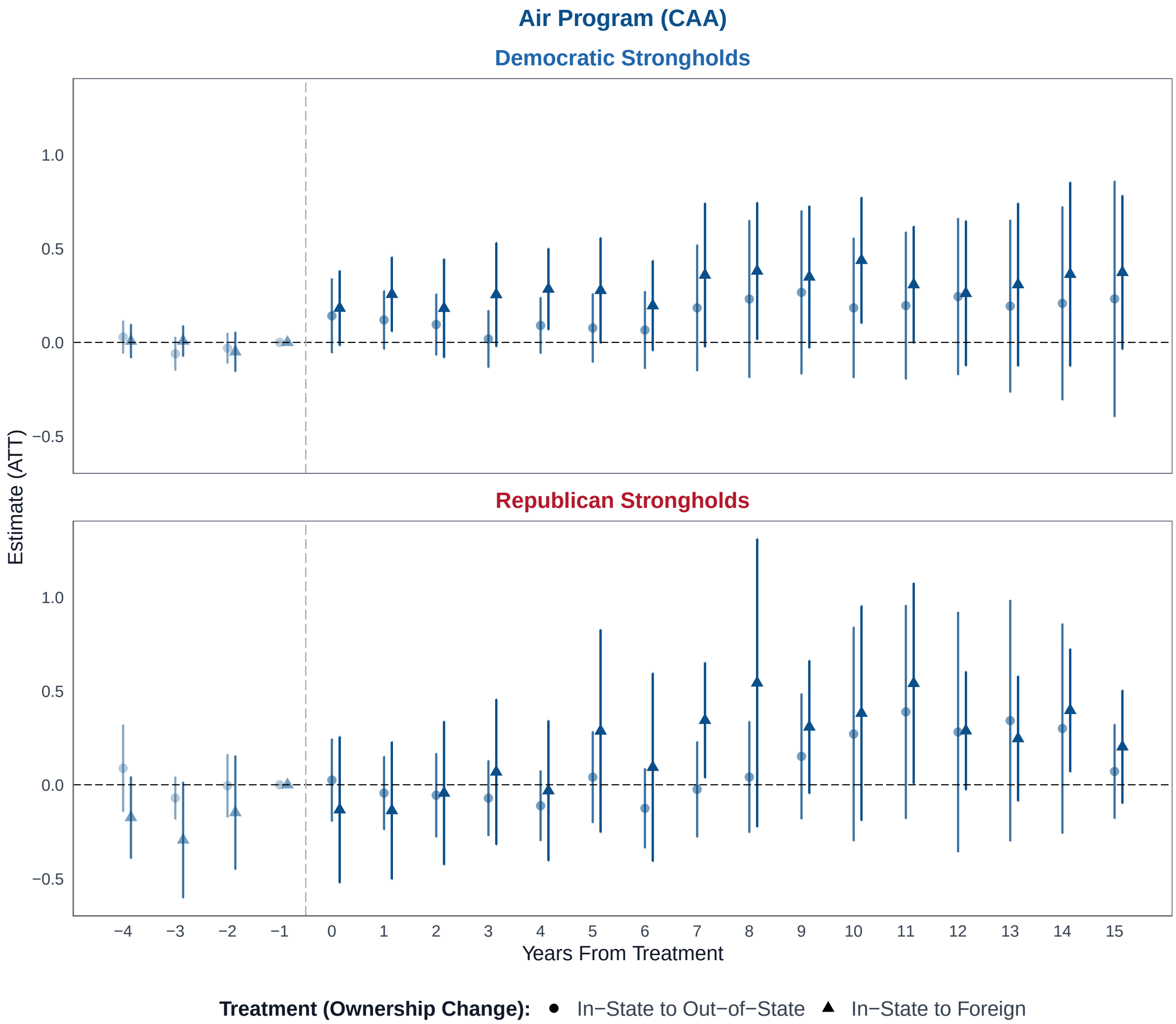


Figure D38: **State heterogeneity results from alternative estimator: doubly robust DiD estimator following Sant'Anna and Zhao (2020).**



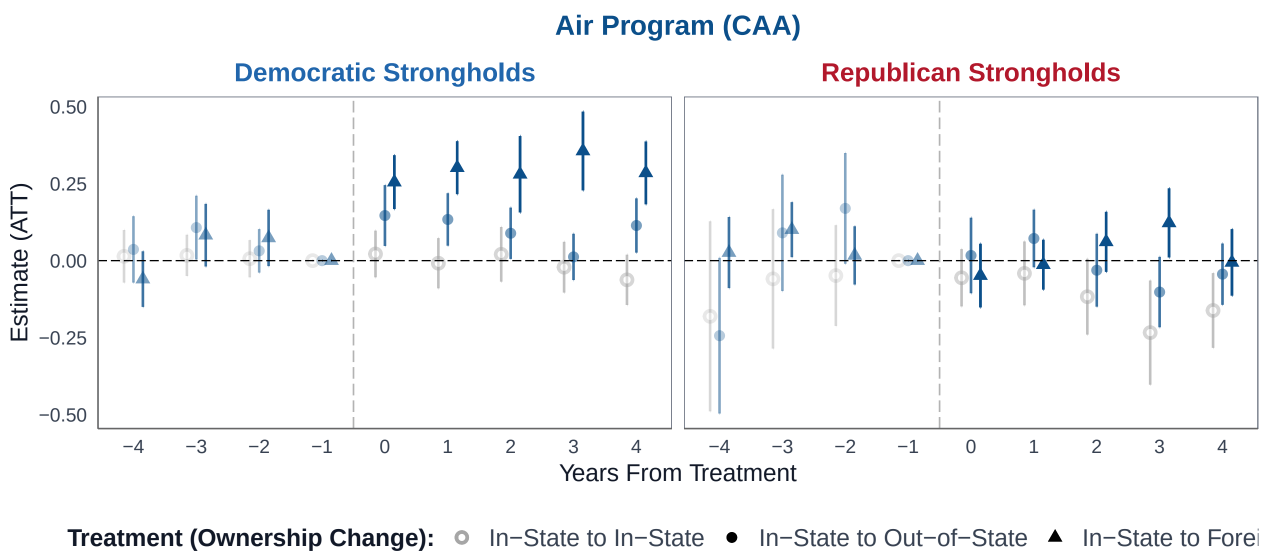
Notes: Applied to the original comparison sets determined by my definitions of treated and control units, without refinement via `PanelMatch`. Full sample containing original linkages.

Figure D39: **State heterogeneity results from alternative estimator: fixed-effects counterfactual estimator (`fect`) (Liu et al., 2022).**



Notes: Applied to the original comparison sets determined by my definitions of treated and control units, without refinement via `PanelMatch`. Full sample containing original linkages.

Figure D40: **Robustness check: Re-estimating the in-state takeover specifications after dropping all facilities first treated in 2014, 2015, or 2019.**



E Appendix E: How Scrutiny Actions Respond to Greenfield Investment

E.1 Design Framework

This subsection sets out the design framework for the greenfield analysis of Section 4: how eligible control units are chosen, how they are made comparable to treated units, and how ownership effects are estimated once they are. Because the violation-case analysis of Section 5 studies the same time-invariant foreign and out-of-state ownership, it shares this framework; Appendix F therefore refers back to this subsection rather than restating it, and records only where its own choices differ.

The design has three steps. First, control units are selected within a defined *stratum*, such that treated and control units must be identical on the stratifying characteristics. For matching methods, this requires matched units to share these characteristics exactly. For weighting methods, it requires estimating weights separately within each stratum.

Second, the remaining potentially confounding characteristics are *balanced* using matching or weighting. Units may differ on these characteristics, but the comparison group is constructed so that their distributions resemble those of the treated group. The matching and weighting methods respectively restrict and reweight eligible control units so that they more closely resemble treated units in industry, facility class, and the other balancing covariates used in the takeover design above.

Finally, I estimate ownership effects within these refined samples of treated and control units. The outcome regressions include state-by-year and industry-by-year fixed effects to absorb time-varying shocks at the state and industry levels. They also control for any covariates that remain imbalanced after preprocessing.

Which characteristics eligible control units must share with treated units is the main design choice, and it is an empirical question rather than a matter of preference, because each added requirement carries two costs. First, every requirement shrinks the pool of eligible controls, and a treated unit left with no eligible control drops out of the analysis—which quietly changes the estimand from the effect on all treated units to the effect on those that happened to have a counterpart. Second, a stricter requirement can worsen balance on everything it does not cover: the units that survive agree on the required characteristics, but the surviving pools may be too thin to balance on the remaining covariates. I therefore add a requirement only where at least 90 percent of treated units keep an eligible control—the same threshold used below to screen individual methods—and report what each candidate requirement costs.

Table E1 summarizes how the greenfield analysis resolves these choices; the subsections below explain each decision in turn.

E.1.1 Scope of Eligible Control Units

Potential control units for each externally owned facility are the in-state-*owned* facilities located in the same **state**, with the same **Toxics Release Inventory (TRI) status**, and in the same five-year **facility-age bracket**:

- Control units must come from the same state because scrutiny is administered by state agencies, so a cross-state comparison would confound ownership with enforcement regimes.
- They must have the same TRI status because the sample combines facilities that exceed the emission thresholds requiring annual TRI reporting with those that do not.

Table E1: **Design decisions by dimension: the greenfield analysis.**

Unit	Facility-year
Sample size	388,637 facility-years (foreign comparison); 605,922 (out-of-state comparison)
State	**Stratum**
Year	Fixed effect; balancing covariate and **stratum** in robustness specifications
TRI status	**Stratum**
Facility class	Covariate; **stratum** in a robustness specification
Facility age	**Stratum** (five-year bracket) and moderator
Industry	Covariate + industry-by-year FE
Estimand	ATT
Methods	All 12 matching methods via `MatchIt` and 7 weighting methods via `WeightIt`
Selection	Screen on pathologies and treated retention, then rank on balance, then ESS
Fixed effects	Industry-by-year and state-by-year, plus matched-pair (matching arm) or pool (weighting arm)
Clustering	Facility

- They must sit in the same age bracket because facility age is the moderator the analysis is about: if a treated facility could draw controls from a different bracket, the pair's own age gap would enter every age-specific estimate, and matched-pair fixed effects cannot absorb a difference that varies within the pair. Age brackets run 0–4, 5–9, and so on to 40 or more years since opening.

I considered three further characteristics as requirements and rejected them; each enters the model as a balancing covariate instead:

- **Calendar year** is the natural candidate, but state-by-year pools are often too small to contain any eligible control, so year is absorbed by state-by-year fixed effects at the outcome stage; robustness specifications add it to the model and, separately, impose it as a requirement.
- **Facility class** (Major, Synthetic Minor, and Minor) matters because the USEPA recommends inspection frequencies that differ by class, but it is an attribute the model can balance directly; a robustness specification nonetheless requires it.
- **Industry** would strand too many treated units in thin state-by-industry pools; it enters the model and is absorbed by industry-by-year fixed effects.

I make these decisions based on the performance of alternative method specifications that use different combinations of these candidates. Appendix E.2 reports what every candidate requirement costs and returns, and how the preferred combination was chosen.

E.1.2 Matching and Weighting Method Specifications

Balancing covariates Within each pool of eligible controls, the matching and weighting methods balance two sets of covariates:

- The characteristics considered but not imposed as requirements—calendar year, facility class, and industry—appear in this set, so they are balanced rather than left free.

- The covariates of the takeover design (Appendix D.3): recent scrutiny (lagged inspection and enforcement counts), compliance history (federally reportable and high-priority violations, failed stack tests, and Title V deviation reports, lagged one year and summed over the two prior years), TRI reporting activity, facility size (logged sales and employment), industry, and the facility's air-program class.

Matching or weighting Matching and weighting implement the design differently, and the difference matters for how their results should be read. Matching selects: a treated unit keeps only control units from its own pool, and unmatched controls are discarded. The matched sets can be absorbed by fixed effects in the outcome regression, so anything constant within a set is controlled whether measured or not. Its cost is information: discarded controls carry estimating power, and precision suffers accordingly.

Weighting keeps every control and reweights it, fitting a separate model within each pool. It is the more efficient of the two and discards no treated units, but it must estimate a model inside every pool, so it degrades as the pools multiply and thin. And it forms no matched sets: the finest grouping it defines is the pool itself, which the outcome regressions hold fixed directly—pool fixed effects are the weighting counterpart of the matched-pair fixed effects—so comparisons stay within pools, though nothing finer than the pool is held fixed.

Neither arm is subordinated to the other: the main text and the appendices report both side by side, because agreement between two methods that fail in different ways is more informative than either alone. Each contributes something the other cannot. Matching meets the design's requirements exactly, and holding anything constant within its matched sets controls it whether measured or not; weighting retains every control unit and delivers the more precise estimates. For the same reason, the design comparison below reports the two arms separately rather than pooling them: where they disagree about a design, the disagreement reflects how each arm pays for the design's requirements, and is itself informative.

List of methods No single matching or weighting method dominates across samples, so rather than commit to one I run every method that converges on these data and let the diagnostics choose. The `MatchIt` and `WeightIt` packages (Ho et al., 2011; Greifer, 2024) are well suited to this: each implements a comprehensive and actively maintained set of estimators in current use. I use the complete set of methods each package provides, twelve matching and seven weighting.

The twelve matching methods are:

1. `nearest_1`: 1:1 nearest-neighbor matching on the propensity score;
2. `nearest_3`: the same, keeping the three nearest controls;
3. `nearest_1_caliper`: 1:1 nearest-neighbor matching within a propensity-score caliper of 0.2 standard deviations;
4. `nearest_1_mahalanobis`: 1:1 nearest-neighbor matching on Mahalanobis distance;
5. `optimal_1`: 1:1 optimal matching, which minimizes total distance across all pairs rather than matching greedily;
6. `optimal_3`: 1:3 optimal matching;
7. `full`: full matching, which uses every control unit in a variable-ratio design;
8. `full_caliper`: full matching within a propensity-score caliper of 0.2 standard deviations;
9. `quick`: quick generalized full matching, a fast approximation suited to large samples;
10. `cem`: coarsened exact matching, which bins the covariates and matches exactly on the bins;

11. `cardinality`: cardinality matching, which maximizes matched sample size subject to explicit balance constraints;
12. `subclass`: propensity-score subclassification into six strata.

The seven weighting methods are:

1. `ebal`: entropy balancing, which solves for the minimum-divergence weights that balance covariate means exactly;
2. `cbps`: Covariate Balancing Propensity Scores, which estimate the propensity score and the balance conditions jointly;
3. `gbm`: generalized boosted modeling, a machine-learning propensity score tuned to minimize the maximum standardized mean difference;
4. `optweight_03`: optimization-based weights that minimize weight variance subject to a balance tolerance of 0.03 standard deviations;
5. `optweight_05`: the same, with a tolerance of 0.05;
6. `optweight_10`: the same, with a tolerance of 0.10;
7. `super`: a super-learner ensemble combining regularized regression, random forests, and a mean model.

Selection Criteria Selection proceeds in three steps.

- **Estimation failure/instability**: A method is first **screened out** if:
  - it fails to converge,
  - produces degenerate weights, or
  - discards more than 10 percent of treated units.
- **Covariate balance**: Surviving methods are then **ranked** on covariate balance, measured as the largest standardized mean difference across the full diagnostic covariate set.
- **Effective sample size (ESS)**: ESS **breaks ties** among methods that balance comparably, since balance bought by discarding information is not free.

The ladder is judged on treated retention alongside balance rather than on balance alone. First, the number of treated units in the sample is small to begin with: 360 foreign-owned and 985 out-of-state-owned facilities under the air program. Second, the subgroup carrying the result is smaller still—86 of the 360 foreign-owned facilities sit in Democratic strongholds. Thus, a design that balances well by matching only the facilities easiest to match is treated as a worse design, not a better one.

Three further features of this rule are worth noting. Convergence warnings enter the screen but not the ranking: a method reporting a benign numerical message should not be preferred over one that balances substantially better. The retention screen is applied wherever any method can meet it, which is everywhere except the design combining calendar year, facility class, and an age bracket. There, no method clears it in any program, because the strata rather than the method are what strand the treated units. The ranking therefore proceeds on balance alone, and those specifications are reported as tested and set aside rather than as robustness checks. And propensity-score subclassification pools units across strata rather than comparing within them, so it cannot implement the restrictions described above; it remains available as a robustness check but is barred from serving as the main specification, since reporting it as such would describe a design that had not in fact been imposed.

E.1.3 Outcome Regression Model Specifications

Model On the preprocessed sample I estimate the effect of external ownership by ordinary least squares, weighting observations by the matching or weighting weights so that the regression reproduces the comparison the preprocessing constructed. I additionally control for any covariate that remains imbalanced after preprocessing, so that residual differences are adjusted for rather than assumed away. Equation (23) below gives the specification, in which the effect is estimated separately within facility-age brackets rather than through a linear interaction between ownership and facility age.

$$Y_{it} \;=\; \sum_k \beta_k \left(D_i \times A_{it}^k\right) \;+\; X_{it}'\gamma \;+\; \lambda_{p(i)} \;+\; \mu_{j(i),t} \;+\; \nu_{s(i),t} \;+\; \varepsilon_{it}, \tag{23}$$

where Y_{it} is the number of scrutiny actions received by facility i in year t; D_i indicates time-invariant external (foreign or out-of-state) ownership; A_{it}^k indicates that facility i's age falls in bracket k in year t; X_{it} collects the matching covariates together with the facility's class history; and $\lambda_{p(i)}$, $\mu_{j(i),t}$, and $\nu_{s(i),t}$ denote the grouping, industry-by-year, and state-by-year fixed effects described below. Each β_k is the marginal effect of external ownership on annual scrutiny actions within age bracket k, so comparisons across the β_k trace how the ownership effect evolves over the facility life cycle without extrapolating a functional form to ages where treated and control support is thin. Under the preferred design matched pairs share an age bracket by construction, so the bracket main effects are collinear with the pair fixed effects and absorbed by them; in the robustness specifications that leave age unconstrained they are estimated as part of X_{it}. Following the takeover analysis, I estimate Equation (23) separately for Democratic strongholds, swing states, and Republican strongholds.

Fixed effects All outcome regressions include industry-by-year and state-by-year fixed effects, together with a grouping effect whose definition differs by arm: matched-pair in the matching arm, pool in the weighting arm. The pair effects hold fixed whatever the matching held fixed, so the estimate comes from comparisons within matched sets; the pool effects are the weighting counterpart, confining comparisons to within pools, though a pool is coarser than a matched set. The industry-by-year and state-by-year effects absorb shocks common to an industry or a state in a given year—a change in state enforcement posture, or an industry-wide downturn—which would otherwise be attributed to ownership if externally owned facilities were unevenly distributed across states or sectors. Where a stratum and a fixed effect coincide, as when preprocessing is already within state and year, the fixed effect is redundant and is absorbed automatically.

Clustering Standard errors are clustered at the facility level, which is the level at which treatment assignment repeats. Ownership is time-invariant, so a facility contributes many observations that all share one treatment status and whose outcomes are serially correlated. Clustering on state-by-year instead would split each facility's observations across many clusters and treat them as independent, understating the standard errors in precisely the dimension where dependence is strongest. The state-by-year dimension is in any case already absorbed by the corresponding fixed effect.

E.2 Model Specification

The framework above fixes the principle—hold a characteristic constant only where doing so retains essentially all treated facilities—but not which characteristics clear that bar. Table E2 answers that empirically. Each row is a complete design: every specification was estimated on every sample, and the entries report the method the selection rule retained, averaged over the six samples.

Two of the columns need a word before the table can be read. First, imbalance is put on a common footing across the arms in two ways: it is computed on the covariates both arms report, excluding the exact-matching strata—those are zero by construction under matching and absent from the weighting diagnostics, so including them would credit matching with balance it never had to achieve—and it is a 95 percent trimmed mean, because cells with almost no within-cell variance produce standardized differences in the hundreds and a plain mean is dominated by them. Second, convergence matters because a cell in which the selected method fails contributes nothing to the estimate, so a low value means the balance and precision reported beside it describe only the cells that survived.

Three patterns organize the table. First, **calendar year is the expensive restriction, not facility age**. The design at the bottom holds year, class, and age fixed simultaneously and retains 39 percent of treated facilities, meeting the requirement in none of the six samples. Removing year while keeping age lifts retention to 95 percent and more than halves the imbalance. Second, **the age restriction is close to free once year is dropped**: the age designs balance as well as the state design that leaves age unconstrained (0.19 against 0.18), at retention three points lower. Third, **holding class fixed buys nothing**: on its own it balances exactly as well as the state design that leaves class free (0.18 under matching) while retaining four points fewer treated facilities, and imposed on top of the age bracket it is not affordable at all—retention falls to 0.76 and the design clears the screen in only two of the six samples. Class is therefore adjusted for as a covariate rather than held fixed.

Table E2: **Design ladder: what each restriction costs and returns.**

	Matching			**Weighting**		
Characteristics held fixed	Imbal.	Retained	Conv.	Imbal.	Retained	Conv.
Preferred						
State, TRI status, age (5-year)	0.19	0.95	91	0.46	0.95	61
Reported as robustness						
State, TRI status	0.18	0.98	91	0.18	0.99	87
State, TRI status, class	0.18	0.94	90	0.36	0.94	65
State, TRI status, year	0.39	0.94	73	0.48	0.94	64
State, TRI status, age (5-year), year covariate	0.22	0.95	91	0.57	0.95	61
Tested and set aside: fails the retention requirement						
State, TRI status, class, age (5-year)	0.20	0.76	89	0.63	0.85	44
State, TRI status, year, class, age (5-year)	0.41	0.39	53	0.78	0.45	18

Notes: Each row is a separate design, estimated on all six samples—the foreign and out-of-state comparisons under each of the three programs (air, water, and waste); five further designs were estimated and are omitted as duplicates of the contrasts reported here. "Imbal." is the mean absolute standardized difference for the method the selection rule retained, averaged over samples; lower is better. "Retained" is the share of externally owned facilities that keep a comparison unit. "Conv." is the percentage of stratum cells in which the selected method reached a solution. "Samples passing" counts the samples in which the retained method holds at least 90 percent of treated facilities.

Weighting partitions the estimation, fitting a separate model in each state-by-TRI-by-bracket cell. This is why the convergence rates in Table E2 diverge as strata are added: matching's model count does not grow with the strata—304 models for the state design and for the age design alike—

while weighting's rises from 581 for the state design to 4,321 for the age design and 88,660 for the design at the foot of the table, each fitted on a correspondingly smaller slice of data.

The preferred design is therefore the one holding state, TRI status, and five-year age bracket fixed. Two features recommend it beyond the table. Every matched pair shares an age bracket, so each bracket-specific estimate is a comparison between facilities of the same vintage rather than a comparison across vintages. And because pairs never straddle a five-year boundary, the two-period contrast reported in the main text is identified within matched pairs at every split point considered, which is not true of any design that leaves age unconstrained.

One limitation should be stated plainly. Holding age fixed while letting the calendar year vary means that a matched pair is of the same age but not from the same era: in the air sample the externally owned facilities are on average about seven years more recent than their comparison units. State-by-year fixed effects absorb the level of scrutiny in any given year, but they do not equalize this composition difference. The specification that adds year to the model reduces the gap by roughly a fifth, at a cost of about 0.03 in imbalance, and is reported alongside the preferred estimates for that reason.

Covariate balance. Covariate balance under the chosen design is reported in Table E3 for the air sample, which compares the treated and control distributions on every diagnostic covariate before and after preprocessing, under both arms.

Outcome regression. Because a binary contrast is easier to read than four bracket-specific coefficients, the figures in the main text collapse Equation (23) to two periods, an earlier and a later one, by replacing the bracket indicators A_{it}^{k} with a single indicator for facility age below a cutoff. The cutoff is placed on a bracket boundary, which keeps each matched pair wholly within one period: because the preferred design matches within five-year brackets, every cutoff at a multiple of five leaves pairs intact. A cutoff falling inside a bracket would split pairs across periods and reintroduce the age gap the matching removes, which is what happens in the specifications that leave age unconstrained—there between a third and two thirds of pairs straddle the cut. I report results for alternative cutoffs, and Appendix E.3 shows the estimates for every cutoff between 10 and 30 years.

One difference in the ranking deserves comment rather than silence. Under matching, the age designs balance best; under weighting, the design holding only state and TRI status does. The reason is the structural one above: matching leaves the model the whole cell to work with, whereas weighting fits a separate model inside each cell, and as strata multiply those cells thin and balance poorly. The two arms therefore need not agree on which design is best, and the ladder reports them separately rather than averaging over a disagreement that is informative in itself.

Table E4 reports the method retained in each of the six samples under the preferred design, for both arms side by side.

Table E3: **Covariate balance: CAA (air) sample.**

Comparison	Foreign v. In-State		Out-of-State v. In-State	
	Unweighted	Weighted	Unweighted	Weighted
	(1)	(2)	(3)	(4)
Log sales	-0.006***	-0.01***	-0.02***	-0.0006
	(0.001)	(0.002)	(0.001)	(0.002)
Log employees	0.01**	0.02**	0.03***	0.005
	(0.006)	(0.009)	(0.006)	(0.008)
Federally reportable violations$_{t=-1}$	0.06***	0.05***	0.04**	-0.003
	(0.01)	(0.02)	(0.02)	(0.04)
High-priority violations$_{t=-1}$	-0.01*	0.06	-0.03***	-0.03
	(0.008)	(0.03)	(0.010)	(0.05)
Failed stack tests$_{t=-1}$	-0.0008	0.02	0.006	-0.01
	(0.01)	(0.02)	(0.02)	(0.03)
Title V violations$_{t=-1}$	0.01	0.07**	0.03**	-0.10*
	(0.01)	(0.03)	(0.01)	(0.06)
Federally reportable violations$_{t=-2,-3}$	0.06***	0.03	0.04**	0.03
	(0.02)	(0.03)	(0.02)	(0.03)
High-priority violations$_{t=-2,-3}$	-0.02	0.07**	-0.03**	-0.006
	(0.01)	(0.03)	(0.01)	(0.03)
Failed stack tests$_{t=-2,-3}$	-0.005	-0.0009	-0.005	-0.03
	(0.010)	(0.02)	(0.01)	(0.04)
Title V violations$_{t=-2,-3}$	-0.006	0.07***	0.004	0.02
	(0.009)	(0.02)	(0.01)	(0.02)
tri_naics_2d21	-0.06	-0.08	0.33*	-0.03
	(0.19)	(0.40)	(0.18)	(0.10)
tri_naics_2d22	-0.11	-0.24	0.33**	-0.47***
	(0.18)	(0.35)	(0.16)	(0.09)
tri_naics_2d23	-0.11	-0.26	-0.17	-0.64***
	(0.18)	(0.39)	(0.17)	(0.15)
tri_naics_2d31	-0.03	-0.19	0.05	-0.35***
	(0.18)	(0.33)	(0.16)	(0.09)
tri_naics_2d32	0.04	-0.19	0.10	-0.45***
	(0.18)	(0.33)	(0.16)	(0.07)
tri_naics_2d33	0.08	-0.17	0.19	-0.45***
	(0.18)	(0.33)	(0.16)	(0.07)
tri_naics_2d42	-0.08	-0.23	-0.01	-0.31***
	(0.18)	(0.34)	(0.17)	(0.10)
tri_naics_2d44	-0.10	-0.42	0.09	-0.15
	(0.18)	(0.36)	(0.20)	(0.14)
tri_naics_2d45	-0.16		0.35	0.07
	(0.17)		(0.24)	(0.08)
tri_naics_2d48	-0.08	-0.26	0.06	-0.24
	(0.19)	(0.40)	(0.20)	(0.16)
tri_naics_2d49	-0.18		-0.33**	-0.88***
	(0.17)		(0.16)	(0.07)
tri_naics_2d52	0.10	0.06	0.29	-0.48**
	(0.31)	(0.42)	(0.27)	(0.23)
tri_naics_2d53	-0.07	-0.16	0.10	-0.43**
	(0.20)	(0.44)	(0.23)	(0.19)
tri_naics_2d54	0.01	0.05	0.07	-0.38**
	(0.19)	(0.35)	(0.18)	(0.15)
tri_naics_2d56	-0.11	-0.36	0.13	-0.26
	(0.19)	(0.36)	(0.21)	(0.16)
tri_naics_2d62	-0.20	-0.69**	0.04	-0.24
	(0.17)	(0.32)	(0.19)	(0.16)
tri_naics_2d71	-0.18	-0.67**	0.13	0.006
	(0.17)	(0.33)	(0.31)	(0.09)
tri_naics_2d81	-0.10	-0.29	-0.19	-0.46**
	(0.19)	(0.40)	(0.18)	(0.20)
tri_naics_2d72			0.67***	0.11
			(0.16)	(0.07)
tri_naics_2d92			0.65***	0.13*
			(0.16)	(0.07)
Observations	17,262	7,568	25,008	22,979
R^2	0.05	0.04	0.09	0.03

Table E4: **Selected preprocessing methods, greenfield design (state, TRI status, five-year age bracket).**

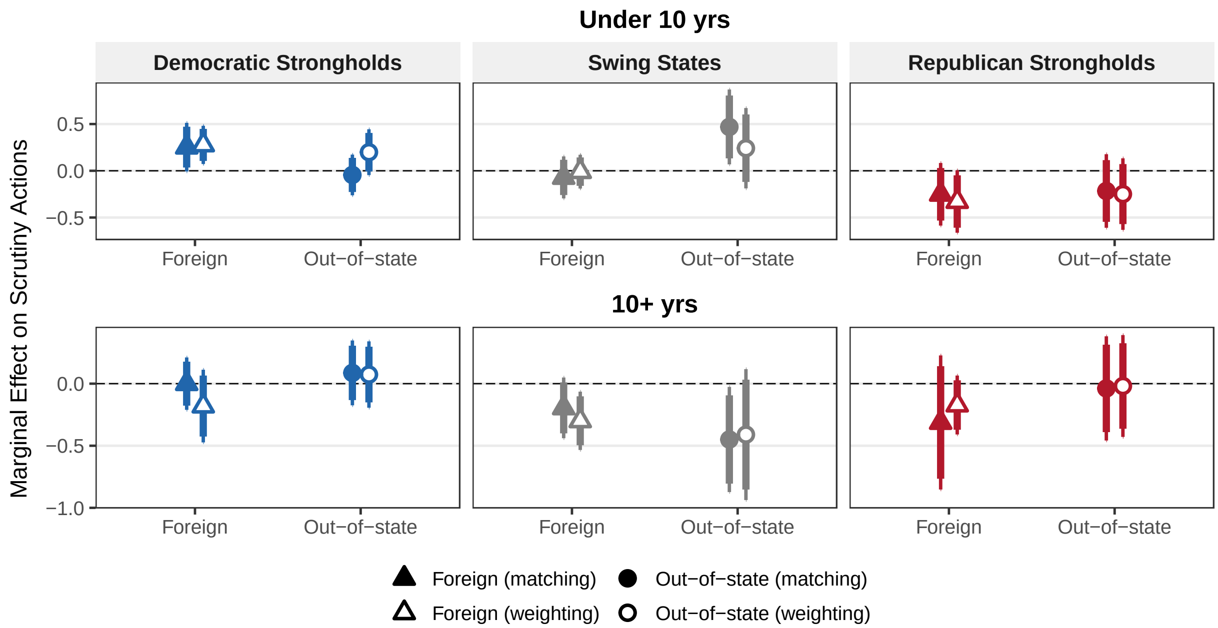
Comparison	Program	Matching	Weighting
Foreign vs. In-State	Air (CAA)	`nearest_1_mahalanobis`	`cbps`
	Water (CWA)	`quick`	`cbps`
	Waste (RCRA)	`quick`	`cbps`
Out-of-State vs. In-State	Air (CAA)	`quick`	`optweight_10`
	Water (CWA)	`full`	`cbps`
	Waste (RCRA)	`full`	`cbps`

Notes: Each row reports the preprocessing method the selection rule of Appendix E.1 retained for that sample: methods failing the convergence, degeneracy, or treated-retention screen are excluded, survivors are ranked on covariate balance, and ties are broken on effective sample size. Method names are those used by `MatchIt` and `WeightIt`.

E.3 Results from the Preferred Specification

E.3.1 Supporting Detail for the Main-Text Results

Figure E1: **Effects of foreign and out-of-state ownership on scrutiny actions, younger versus older facilities, with swing states shown separately.**



Notes: As Figure 6, but reporting swing states as a third group rather than omitting them. Filled markers are the matching arm, hollow the weighting arm; thick bars are 90 percent intervals, thin bars 95 percent.

Table E5: **The preferred design estimated by matching and by weighting.**

	Democratic		Swing		Republican	
Estimation arm	Earlier	Later	Earlier	Later	Earlier	Later
Foreign vs. in-state ownership						
Matching (preferred)	0.253^{+}	0.000	−0.071	−0.195	−0.251	−0.313
	(0.133)	(0.108)	(0.115)	(0.125)	(0.171)	(0.275)
Weighting	0.160^{+}	−0.128	−0.007	-0.271^{**}	-0.317^{+}	-0.213^{+}
	(0.091)	(0.136)	(0.093)	(0.102)	(0.172)	(0.119)
Out-of-state vs. in-state ownership						
Matching (preferred)	−0.045	0.087	0.469^{*}	-0.450^{*}	−0.216	−0.038
	(0.111)	(0.133)	(0.204)	(0.216)	(0.201)	(0.214)
Weighting	0.224^{+}	0.034	0.200	−0.331	−0.225	0.020
	(0.120)	(0.134)	(0.204)	(0.250)	(0.179)	(0.207)

Notes: Both arms use the preferred design and differ only in how the comparison group is built. Weighting retains every control unit and so estimates on 3,601 facility-years against 1,224. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table E6 reports the ownership effect within each five-year age bracket under the preferred design, separately by ownership type and state group, with the matching estimates in Panel A and the weighting estimates in Panel B. Because the design holds the bracket fixed, each entry

Table E6: **Effect of external ownership on annual scrutiny actions, by facility-age bracket.**

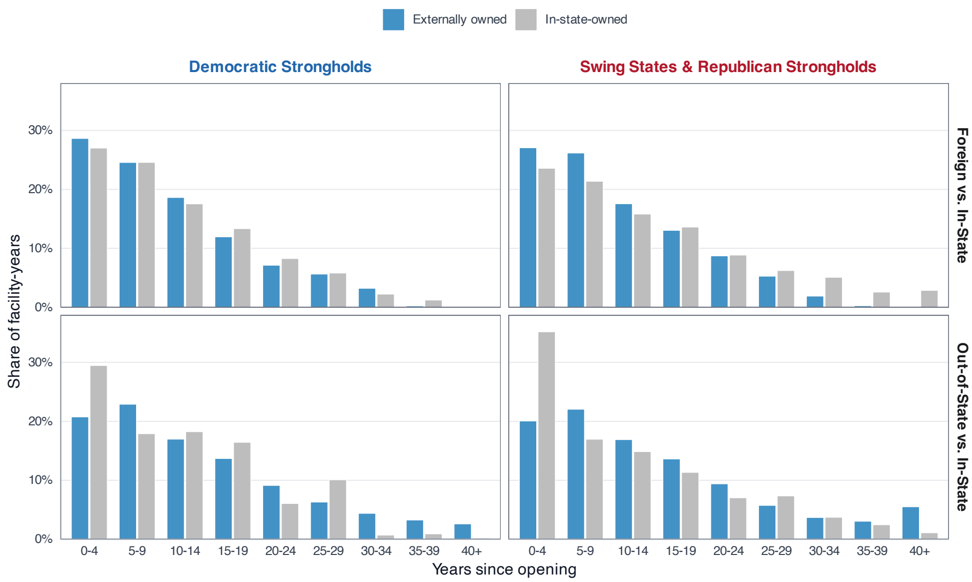
Years since opening	0–4	5–9	10–14	15–19	20–24	25–29	30–34	35–39	40+
Panel A. Matching									
Foreign vs. in-state ownership									
Democratic strongholds	0.324^{+}	0.191	0.032	−0.026	−0.113	−0.036	0.105	0.772	—
	(0.176)	(0.157)	(0.152)	(0.165)	(0.288)	(0.329)	(0.407)	(0.590)	
	[0.30]	[0.26]	[0.18]	[0.11]	[0.07]	[0.05]	[0.03]	[<0.01]	—
Swing states	−0.067	−0.061	−0.084	−0.331^{+}	0.008	−0.661*	−0.161	0.530	—
	(0.146)	(0.166)	(0.208)	(0.183)	(0.251)	(0.311)	(0.300)	(0.643)	
	[0.25]	[0.25]	[0.18]	[0.13]	[0.08]	[0.06]	[0.04]	[0.01]	—
Republican strongholds	−0.131	−0.378	0.068	−0.055	−0.722*	−0.914	−2.475^{+}	—	—
	(0.140)	(0.276)	(0.201)	(0.259)	(0.364)	(0.819)	(1.382)		
	[0.29]	[0.27]	[0.17]	[0.13]	[0.09]	[0.04]	[0.01]	—	—
Out-of-state vs. in-state ownership									
Democratic strongholds	0.066	−0.145	0.303*	0.361^{+}	−0.101	−0.540^{+}	−0.419	0.472	0.194
	(0.120)	(0.161)	(0.153)	(0.202)	(0.244)	(0.275)	(0.391)	(0.319)	(0.435)
	[0.22]	[0.25]	[0.17]	[0.14]	[0.09]	[0.06]	[0.04]	[0.02]	[<0.01]
Swing states	0.327	0.547*	0.405	−0.318	−0.482	−1.433**	−1.061*	−0.291	−4.508**
	(0.219)	(0.243)	(0.454)	(0.222)	(0.433)	(0.538)	(0.490)	(0.381)	(1.572)
	[0.19]	[0.22]	[0.17]	[0.13]	[0.09]	[0.06]	[0.04]	[0.04]	[0.07]
Republican strongholds	−0.275	−0.147	−0.431	0.078	0.261	0.180	0.401	0.435	−1.657
	(0.186)	(0.308)	(0.337)	(0.292)	(0.314)	(0.265)	(0.367)	(0.451)	(1.044)
	[0.22]	[0.25]	[0.18]	[0.15]	[0.10]	[0.05]	[0.03]	[0.02]	[0.01]
Panel B. Weighting									
Foreign vs. in-state ownership									
Democratic strongholds	0.260*	0.043	0.029	−0.172	−0.163	−0.115	−0.781	−0.970	—
	(0.126)	(0.126)	(0.206)	(0.147)	(0.212)	(0.241)	(0.599)	(1.033)	
	[0.29]	[0.26]	[0.18]	[0.11]	[0.07]	[0.05]	[0.03]	[<0.01]	—
Swing states	0.025	−0.040	−0.111	−0.305*	−0.289	−0.596**	−0.394	0.065	—
	(0.118)	(0.117)	(0.157)	(0.154)	(0.228)	(0.210)	(0.251)	(0.298)	
	[0.25]	[0.25]	[0.18]	[0.13]	[0.08]	[0.06]	[0.04]	[0.01]	—
Republican strongholds	−0.333^{+}	−0.286	0.121	−0.064	−0.544*	−0.696**	−1.561*	—	—
	(0.189)	(0.221)	(0.189)	(0.206)	(0.262)	(0.260)	(0.653)		
	[0.28]	[0.27]	[0.17]	[0.13]	[0.09]	[0.05]	[0.01]	—	—
Out-of-state vs. in-state ownership									
Democratic strongholds	0.312*	0.135	0.156	0.098	−0.050	−0.205	−0.286	0.421^{+}	0.148
	(0.148)	(0.142)	(0.158)	(0.200)	(0.228)	(0.206)	(0.248)	(0.218)	(0.302)
	[0.22]	[0.24]	[0.18]	[0.14]	[0.09]	[0.06]	[0.04]	[0.02]	[<0.01]
Swing states	0.176	0.232	0.611^{+}	0.099	−0.356	−0.935*	−0.683	−0.959	−5.352**
	(0.198)	(0.272)	(0.320)	(0.299)	(0.367)	(0.385)	(0.453)	(0.652)	(1.059)
	[0.19]	[0.22]	[0.16]	[0.13]	[0.09]	[0.06]	[0.04]	[0.04]	[0.08]
Republican strongholds	−0.239^{+}	−0.211	−0.081	0.049	0.198	−0.100	0.282	0.403	−1.220*
	(0.130)	(0.330)	(0.278)	(0.323)	(0.245)	(0.317)	(0.408)	(0.418)	(0.611)
	[0.22]	[0.24]	[0.18]	[0.14]	[0.10]	[0.05]	[0.04]	[0.02]	[0.01]

Notes: Square brackets give the share of that sample's weighted facility-years falling in the bracket, so each row's shares sum to one across the brackets it populates; they show how much of the sample stands behind each estimate. A dash marks a bracket with no treated facilities. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

compares facilities of the same vintage under either arm—matching within the pair, weighting within the stratum cell—and the profile across brackets is what carries the life-cycle information.

The two-period contrast in the main text collapses this profile at ten years. At that cutoff, foreign-owned facilities in Democratic strongholds receive 0.25 more scrutiny actions per year than otherwise similar in-state facilities of the same vintage (s.e. 0.13), against a base rate of roughly 0.55—an increase of close to a half; among facilities ten years or older the same comparison gives 0.00 (s.e. 0.11), a tightly estimated zero rather than merely an imprecise one. Its robustness to that choice is shown by the cutoff scan. As the cutoff widens from 10 to 30 years, the earlier-period estimate for foreign ownership in Democratic strongholds declines steadily—0.253, 0.240, 0.192, 0.173, 0.149, 0.146, 0.137, 0.138, 0.136 at cutoffs of 10, 12, 15, 18, 20, 22, 25, 28, and 30 years—and is significant at the ten percent level up to a cutoff of 18 years, losing significance thereafter as the earlier period increasingly absorbs ages at which there is no effect. The later-period estimate is indistinguishable from zero at every cutoff; its point estimate drifts upward past 25 years, but on standard errors two to three times the earlier-period ones, so nothing should be read into it. This is the pattern expected if the effect is concentrated in the first decade and progressively diluted as later years are folded into the earlier period, and it is not the pattern expected from an artifact of where the split falls.

Figure E2: **Distribution of Facility Age.**



Notes: Age distribution underlying Figure 6. The upper panel is the foreign versus in-state comparison, the lower panel out-of-state versus in-state; columns are the two state groups. Bars give the share of each group's weighted facility-years falling in each five-year age bracket, so the bars within a group sum to one and panels holding very different numbers of units remain comparable. Gray bars are the control units (in-state facilities). Read the bracket-specific estimates only over the range where both groups have appreciable mass.

Table E7: **Facilities with time-invariant ownership: All three programs.**

	CAA (Air)		CWA (Water)		RCRA (Waste)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Matching						
Foreign	-0.05		-0.04		-0.05	
	(0.12)		(0.04)		(0.03)	
Foreign × Years	-0.009		0.004*		0.002	
	(0.01)		(0.002)		(0.003)	
Out-of-State		0.46***		-0.07*		-0.01
		(0.16)		(0.04)		(0.02)
Out-of-State × Years		-0.04***		0.004**		-0.0008
		(0.009)		(0.002)		(0.0010)
DV Mean	0.77	0.92	0.25	0.25	0.21	0.20
Weight	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Match Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,724	22,773	13,498	21,645	26,942	42,674
R^2	0.73	0.68	0.70	0.57	0.61	0.55
Panel B. Weighting						
Foreign	-0.04		-0.06		-0.03	
	(0.11)		(0.05)		(0.03)	
Foreign × Years	-0.01*		0.003		-0.002	
	(0.007)		(0.003)		(0.003)	
Out-of-State		0.55***		-0.04		-0.02
		(0.18)		(0.05)		(0.02)
Out-of-State × Years		-0.05***		0.002		-0.0010
		(0.01)		(0.002)		(0.001)
DV Mean	0.77	0.92	0.25	0.25	0.21	0.20
Weight	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
state-TRI-db_years_dec FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,033	22,781	13,510	21,760	26,958	42,718
R^2	0.45	0.36	0.48	0.33	0.38	0.35

Notes: Panel A is the matching arm, Panel B the weighting arm; both estimate the same specification on the same programs. Under weighting, observations are weighted by the `WeightIt` weights and the stratum cell is absorbed by fixed effects, which is why the two panels differ in their final fixed-effect row and in the number of observations retained. Dependent variable: number of scrutiny actions per facility-year. OLS models. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E8: **CAA facilities with time-invariant ownership: Effects on the number of penalties.**

	CAA (Air)					
	Democratic Strongholds		Swing States		Republican Strongholds	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign	0.06		0.08		-0.008	
	(0.10)		(0.08)		(0.05)	
Foreign × Years	-0.0007		-0.003		0.006	
	(0.006)		(0.004)		(0.005)	
Out-of-State		-0.12**		0.06		0.11**
		(0.05)		(0.04)		(0.04)
Out-of-State × Years		0.006**		-0.003		-0.005**
		(0.003)		(0.002)		(0.002)
DV Mean	0.13	0.12	0.12	0.11	0.16	0.15
Weight	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Match Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,224	6,250	1,952	7,354	3,208	8,799
R^2	0.77	0.55	0.77	0.46	0.72	0.43

Notes: OLS models. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

E.4 Results from Alternative Specifications

Three alternatives vary one design choice each, so that any difference can be attributed to that choice rather than to a change of design. Rather than report a regression table for each, two tables give all of them in the form used in the main text—the ownership effect earlier and later in the life cycle, at the same ten-year cutoff as Figure 6—so that every design can be read directly against the headline estimate. Table E9 covers foreign ownership and Table E10 out-of-state ownership, each with the matching arm in Panel A and the weighting arm in Panel B, so the two arms can be compared design by design within a single table.

Year in the balancing model. Adding a year covariate to the preferred design addresses the era gap described above. It reduces the year imbalance by roughly a fifth and costs about 0.03 in overall balance, leaving retention unchanged. This is the specification to consult on whether the early effect reflects the life cycle or the period in which recent facilities were built, and it is the most demanding of the four: it cuts the early estimate from 0.192 to 0.049.

No age restriction. Holding only state and TRI status fixed gives the highest retention of any design (0.99) and, on the weighting arm, the best balance. It cannot support the bracket-specific estimates, since matched pairs there differ in age by about ten years on average, but it provides a check on whether the overall ownership effect depends on the age restriction.

Year as a stratum. Requiring a common calendar year rather than adjusting for it is the natural alternative to the preferred treatment of year. It remains feasible on its own—retention 0.94—and is reported for completeness, though its balance is markedly worse (0.47).

One thing should be said before the comparison is read. These are small samples, and the designs differ in how much of the sample they keep, so they differ in how precisely anything can be estimated. The designs that discard treated units are estimated on substantially fewer observations—in places fewer than half—and their standard errors are correspondingly wider, in places nearly twice those of the preferred design. Differences across the ladder should be read with that in mind.

Table E9: **Effect of foreign ownership on annual scrutiny actions, earlier versus later in the facility life cycle, across designs.**

	Democratic		Swing		Republican	
Characteristics held fixed	Earlier	Later	Earlier	Later	Earlier	Later
Panel A. Matching						
State, TRI status, age (5-year)	0.253^{+}	0.000	−0.071	−0.195	−0.251	−0.313
	(0.133)	(0.108)	(0.115)	(0.125)	(0.171)	(0.275)
State, TRI status, age (5-year), year cov.	0.054	−0.142	-0.356^{*}	-0.418^{**}	-0.603^{*}	−0.197
	(0.115)	(0.122)	(0.151)	(0.153)	(0.249)	(0.246)
State, TRI status (no age restriction)	−0.065	0.023	−0.077	-0.452^{**}	−1.079	−0.102
	(0.159)	(0.147)	(0.210)	(0.151)	(0.664)	(0.259)
State, TRI status, class	0.033	-0.230^{+}	−0.010	-0.237^{+}	-0.326^{*}	−0.215
	(0.091)	(0.125)	(0.090)	(0.132)	(0.160)	(0.163)
State, TRI status, year	0.261	−0.299	−0.263	−0.194	-1.193^{+}	−0.073
	(0.281)	(0.288)	(0.167)	(0.135)	(0.704)	(0.396)
State, TRI status, year, class, age (5-yr)	0.266	−0.081	−0.317	−0.110	−0.866	0.351
	(0.235)	(0.498)	(0.193)	(0.161)	(0.664)	(0.766)
Panel B. Weighting						
State, TRI status, age (5-year)	0.160^{+}	−0.128	−0.007	-0.271^{**}	-0.317^{+}	-0.213^{+}
	(0.091)	(0.136)	(0.093)	(0.102)	(0.172)	(0.119)
State, TRI status, age (5-year), year cov.	0.104	−0.194	−0.079	-0.318^{*}	-0.511^{*}	-0.337^{*}
	(0.133)	(0.171)	(0.102)	(0.139)	(0.200)	(0.138)
State, TRI status (no age restriction)	0.275^{+}	−0.141	−0.001	-0.223^{*}	-0.362^{+}	−0.295
	(0.145)	(0.155)	(0.095)	(0.107)	(0.186)	(0.226)
State, TRI status, class	0.023	0.074	−0.027	-0.254^{*}	-0.321^{*}	-0.207^{+}
	(0.131)	(0.127)	(0.086)	(0.109)	(0.127)	(0.118)
State, TRI status, year	0.110	−0.205	−0.043	−0.192	-0.321^{+}	-0.433^{*}
	(0.183)	(0.209)	(0.115)	(0.119)	(0.187)	(0.188)
State, TRI status, year, class, age (5-yr)	0.368^{+}	−0.158	-0.370^{+}	−0.040	−0.996	0.562
	(0.198)	(0.334)	(0.193)	(0.153)	(0.621)	(0.887)

Notes: Foreign versus in-state ownership, air program. Each row is a separate design, and the same six designs appear in both panels. "Earlier" is under ten years since opening, "later" ten or more. The designs differ in how much of the sample they retain, so they differ in precision; rows that discard treated units are estimated on fewer observations and carry wider standard errors. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table E10: **Effect of out-of-state ownership on annual scrutiny actions, earlier versus later in the facility life cycle, across designs.**

	Democratic		Swing		Republican	
Characteristics held fixed	Earlier	Later	Earlier	Later	Earlier	Later
Panel A. Matching						
State, TRI status, age (5-year)	-0.045	0.087	0.469^{*}	-0.450^{*}	-0.216	-0.038
	(0.111)	(0.133)	(0.204)	(0.216)	(0.201)	(0.214)
State, TRI status, age (5-year), year cov.	0.094	0.089	0.422^{*}	-0.164	-0.274	-0.067
	(0.113)	(0.138)	(0.196)	(0.217)	(0.175)	(0.226)
State, TRI status (no age restriction)	-0.086	0.207	0.425^{*}	-0.299	-0.141	0.237
	(0.134)	(0.168)	(0.173)	(0.242)	(0.156)	(0.176)
State, TRI status, class	0.113	0.045	0.314^{*}	-0.247	-0.231	0.119
	(0.131)	(0.128)	(0.133)	(0.174)	(0.187)	(0.164)
State, TRI status, year	0.159	0.030	0.183	-0.836^{**}	-0.244	-0.303
	(0.118)	(0.168)	(0.201)	(0.222)	(0.234)	(0.199)
State, TRI status, year, class, age (5-yr)	0.055	-0.031	0.240	0.225	-0.137	0.043
	(0.193)	(0.098)	(0.245)	(0.239)	(0.360)	(0.502)
Panel B. Weighting						
State, TRI status, age (5-year)	0.224^{+}	0.034	0.200	-0.331	-0.225	0.020
	(0.120)	(0.134)	(0.204)	(0.250)	(0.179)	(0.207)
State, TRI status, age (5-year), year cov.	0.105	0.044	0.188	-0.356	-0.313	-0.083
	(0.126)	(0.136)	(0.209)	(0.244)	(0.324)	(0.193)
State, TRI status (no age restriction)	0.106	0.151	0.345	-0.245	-0.078	0.145
	(0.125)	(0.149)	(0.225)	(0.213)	(0.180)	(0.237)
State, TRI status, class	0.088	0.067	0.163	-0.225	-0.287^{+}	0.117
	(0.094)	(0.115)	(0.169)	(0.174)	(0.167)	(0.245)
State, TRI status, year	0.247^{+}	0.061	0.249	-0.527^{*}	-0.081	0.010
	(0.131)	(0.162)	(0.225)	(0.241)	(0.222)	(0.204)
State, TRI status, year, class, age (5-yr)	-0.149	-0.015	0.159	-0.193	-0.374	-0.249
	(0.215)	(0.087)	(0.271)	(0.258)	(0.333)	(0.477)

Notes: Out-of-state versus in-state ownership, air program. Each row is a separate design, and the same six designs appear in both panels. "Earlier" is under ten years since opening, "later" ten or more. The designs differ in how much of the sample they retain, so they differ in precision; rows that discard treated units are estimated on fewer observations and carry wider standard errors. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

F Appendix F: Bias in Penalties

This appendix documents the violation-case analysis of Section 5. It applies the design framework set out in Appendix E.1—the same principle, method menu, and selection rule the greenfield analysis follows—and does not restate it; the reader is referred there for the common design and for the criterion governing where the two analyses diverge. What follows records the two datasets the analysis draws on, then the model specifications, then the results.

F.1 CAA Pipeline Dataset

F.1.1 Data Coverage

By comparing the CAA Pipeline dataset with the original ECHO violation records, I verify that it covers the universe of HPVs and FRVs recorded in ECHO since 2015. An additional 3,836 cases are not HPVs or FRVs themselves but are related to such violations. The dataset does not comprehensively cover violations before 2015; only a small number of cases from 1994 to 2015 are included, accounting for approximately 1 percent of the dataset. Accordingly, I restrict the analysis to violation cases identified since 2015.

The Pipeline dataset contains a facility identifier shared with the TRI database, allowing TRI facilities in the Pipeline data to be matched to TRI records. To align violation cases with facility-year information, I assign each case to its identification year; if unavailable, I use the year of the associated enforcement action.

F.1.2 Variables Capturing Violation Characteristics

For the CAA Pipeline analyses, the covariate-balancing matching/weighting procedures use both facility-level covariates from the TRI–FRS panel and violation-level covariates from the Pipeline data. The same covariate set is used for the `MatchIt` and `WeightIt` procedures, subject to sample-specific screening of sparse indicators.

Facility-level covariates include lagged CAA inspection and enforcement history: prior-year values and two-year lagged sums of air inspections, air enforcement, FRV counts, HPV counts, failed stack tests, and Title V deviation reports. I also include TRI reporting status, nonattainment status, major-source facility classification, logged D&B sales, and logged D&B employment.

Violation-level covariates include whether the case was identified through an evaluation, whether it was classified as an HPV, whether it involved a USEPA-level violation, the number of CAA sub-programs involved, and the number of pollutants involved. In addition, the procedures consider 27 indicators for specific CAA sub-programs and 197 indicators for specific pollutants associated with the violation.

Because these program and pollutant indicators are high-dimensional and often sparse, I retain individual indicators only when they have sufficient support across treatment and comparison groups within the relevant sample; otherwise, balance is assessed using the aggregate program and pollutant counts. For `MatchIt`, I additionally exact-match on state and two-digit NAICS industry where feasible.

F.2 Campaign Contributions Data

This study uses itemized contribution records from the Database on Ideology, Money in Politics, and Elections (DIME) to construct measures of firms' political activity. The analysis focuses on contributions made directly to candidates and excludes other forms of political spending.

Specifically, I restrict the sample to transactions classified as contributions (transaction types 15, 15C, 15E, 15Z, and, for state elections, 15S) and further limit the data to records where the recipient is a candidate (`recipient.type == "CAND"`). This ensures that the measures capture financial flows directed to candidates' campaign committees.

By construction, this approach excludes several categories of political spending that are conceptually distinct from direct contributions. These include independent expenditures (e.g., transaction types 24A and 24E), electioneering communications (e.g., transaction type 29), and transfers across committees or other organizational entities. As a result, the contribution measures do not capture spending that is made independently of candidates or routed through party committees, PACs, or other intermediaries without being attributed to a specific candidate.

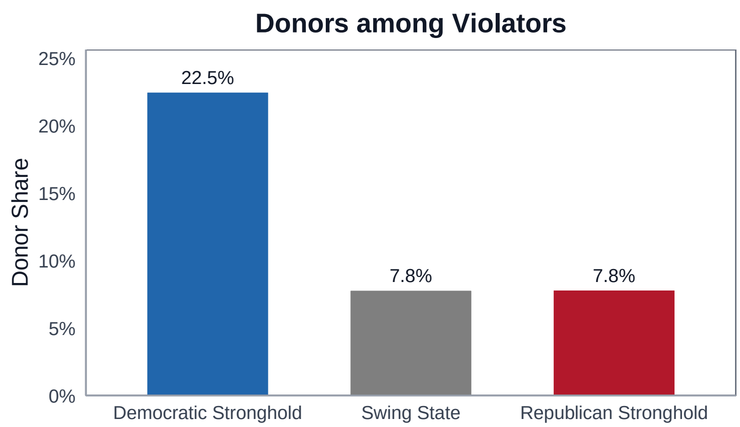
It is important to note that the retained contribution categories include not only direct monetary contributions (type 15), but also earmarked contributions (15E) and in-kind contributions (15Z). Therefore, the measures should be interpreted as capturing candidate-directed contributions in a broad sense, rather than strictly direct cash transfers.

I link corporate donors to the D&B database, which allows me to match donors to facilities in the USEPA datasets. I apply the same linkage procedure described in Appendix C.3.

F.2.1 Descriptive Patterns

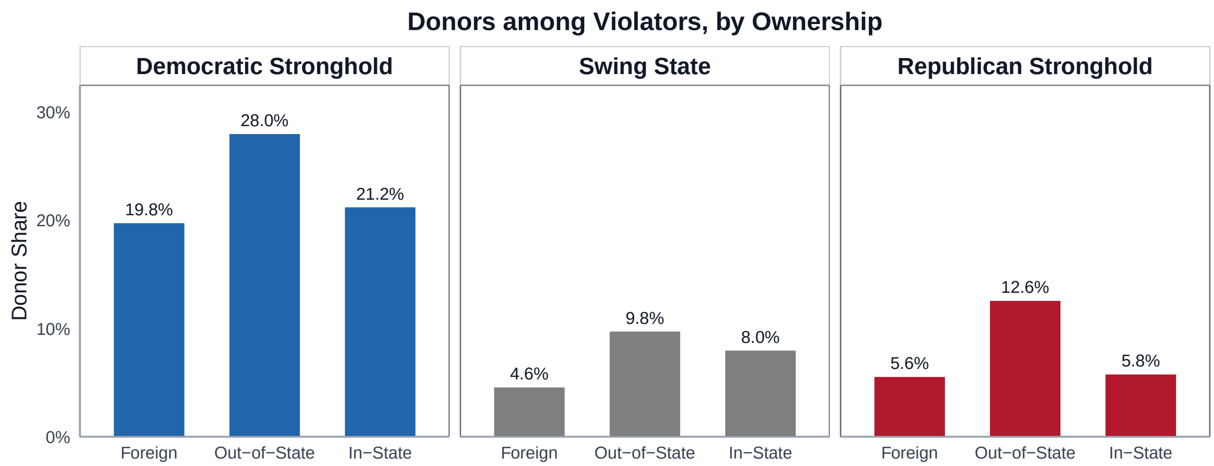
This subsection describes the samples before any matching, so that the estimates in Section 5 can be read against the raw distribution of the treatment and of the outcome's support. All figures use the five-year donation window, the treatment definition used throughout the paper.

Figure F1: **Share of violation cases whose owner donated in state, by state political type.**



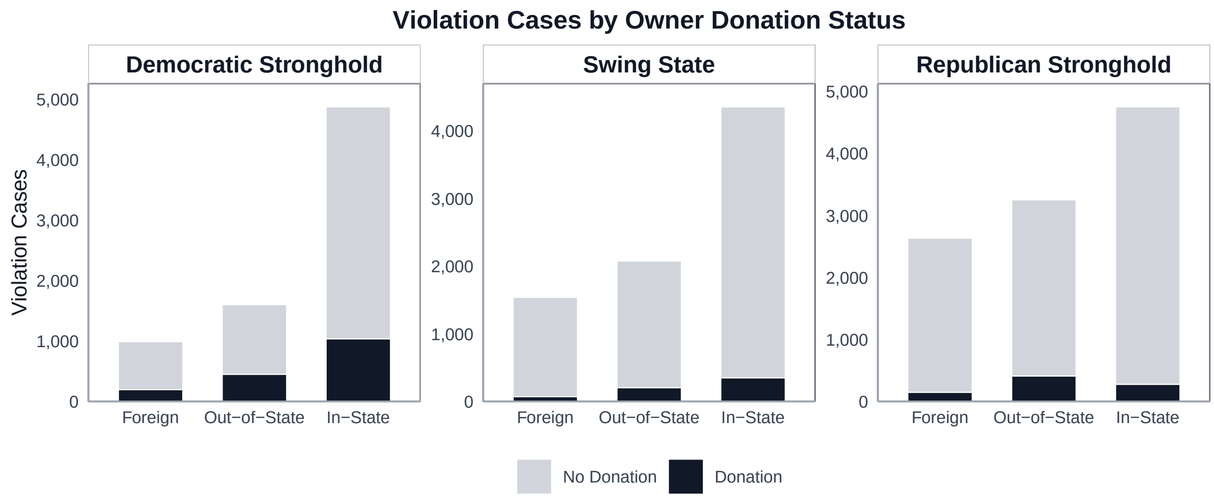
Notes: Bars give the unmatched share of violation cases whose owning firm made at least one contribution to a candidate for state office in the five years before the violation was identified—the treatment definition used in the text. Donor prevalence in Democratic strongholds is roughly three times that in swing states or Republican strongholds.

Figure F2: **Donor share by ownership category.**



Notes: As Figure F1, split by whether the facility's owner is foreign, domestic but headquartered out of state, or in state. Rates rather than counts, so that categories of very different size are comparable. Out-of-state domestic owners are the most likely to have donated in every state type.

Figure F3: **Violation cases by owner donation status, ownership category, and state political type.**



Notes: Raw counts, shown alongside the rates above because the cell sizes determine how much of the sample the matched comparisons can retain. Dark segments are donor cases. Vertical scales differ across panels.

F.3 Design and Choice of Specification

This subsection introduces how I apply the broader design framework described in Appendix E.1 to the sample of violation cases here, including where the implementation departs from the greenfield one.

The unit of analysis is the individual violation case rather than the facility-year, which drives every difference between the two designs. The estimation sample comprises 45,196 Federally Reportable Violations identified since 2015 and successfully merged to the facility panel, out of 55,195 cases in the Pipeline dataset. Because a case is observed only in the year it is identified, and because serious violations are rare relative to the facility population, the analysis sample is roughly two orders of magnitude smaller than the greenfield panel. This is what makes the finer stratifications used there infeasible here: the design choices must be coarser, not because coarser choices are preferable, but because the sample cannot support finer ones. Table F1 summarizes the resulting decisions; the rest of this subsection explains them.

Table F1: **Design decisions by dimension: the violation-case analysis.**

Unit	Violation case
Sample size	45,196 cases
State	**Stratum**
Year	Fixed effect only
TRI status	Covariate + fixed effect
Facility class	Covariate
Facility age	Not used (design conditions on a violation)
Industry	Covariate + industry-by-year FE
Estimand	ATT
Methods	All 12 matching methods via `MatchIt` and 7 weighting methods via `WeightIt`
Selection	Screen on pathologies and treated retention, then rank on balance, then ESS
Fixed effects	Industry-by-year, state-by-year, and TRI status
Clustering	Facility

Preprocessing is performed within state. Finer stratifications used in the greenfield design are not viable at this sample size. Adding year would leave the sparsest subsample—foreign-owned facilities in swing states—with 26 percent of its cases, and several other subsamples with between 57 and 72 percent. Adding industry would discard half the treated cases in the smallest subsample, foreign-owned facilities in Democratic strongholds. Both dimensions are instead handled through the matching/weighting model and the outcome fixed effects, which is the framework's default for anything not held fixed by construction.

TRI status is treated differently here than in the greenfield design, where control units must share it. Among violation cases it is strongly associated with both treatments—26.4 percent of TRI-sourced cases involve foreign-owned facilities against 10.7 percent of FRS-sourced ones, and 7.2 against 3.0 percent involve donors—so it cannot be left unadjusted. But requiring control cases to share it would cost overlap exactly where overlap is scarcest: state-by-TRI-status pools keep every treated case yet lose about a quarter of the control cases in the smallest foreign subsamples.

TRI status therefore enters the matching/weighting model and is additionally absorbed by a fixed effect.

Facility age, central to the greenfield design, plays no role here. That analysis follows facilities over their life cycle and asks how the ownership effect changes with age; this one conditions on a violation having occurred, so every comparison is anchored to a specific event rather than to a point in the life cycle.

F.3.1 Balancing Covariates

The matching/weighting model contains the facility-level covariates used in the time-invariant ownership design—recent scrutiny actions, violation and citation histories, TRI reporting status, facility class, industry, and facility size—augmented with violation-level characteristics from the Pipeline dataset that capture the nature and severity of the case: whether the violation is a High Priority Violation, its enforcement level, whether an evaluation preceded it, and counts of the programs and pollutants cited.

The design holds the state stratum fixed and adjusts for everything else, so the one specification choice left open is which covariates enter the matching/weighting model. Two sets are estimated:

- The *main* set contains the facility covariates of the greenfield design, the base violation characteristics, and aggregate counts of the programs and pollutants cited, together with those individual program and pollutant indicators carried by at least ten treated *and* ten control cases.
- The *all-covariate* set adds the remaining individual indicators regardless of how rare they are, in two codings—one using individual pollutant codes, one using pollutant categories.

The main set is preferred, for a reason about how the comparison groups are constructed rather than about parsimony. The individual indicators are sparse: many are carried by a handful of cases, and some appear only among treated or only among control cases. The balancing methods that solve a constrained optimization—entropy balancing, CBPS, and the optimization-based weights—have no feasible solution when a covariate has no variation in one group, and a near-degenerate one when it has almost none. Table F2 shows what including them costs. Balance barely moves: for the comparison of violation cases the mean absolute standardised difference is 0.033 under the main set against 0.028 with every indicator added. Effective sample size collapses: from 0.44 of the nominal sample to 0.14. The sparse indicators therefore buy no balance and spend most of the information available to estimate the effect.

Restricting the model does not restrict the diagnostics. Balance is always assessed on the full set, including the indicators the model was not asked to balance, so the main specification is held to the wider standard it is being compared against. Both arms are estimated on both covariate sets and reported together throughout; the all-covariate results appear in Appendix F.4.

One column of Table F2 needs a word. "Relaxed" counts the samples whose selected method qualified only after the no-serious-warning gate was dropped—that is, where the optimizer reported it had not reached a stable solution. It is negligible everywhere except the weighting arm of the donation design, at 11 of 12 samples, and that caveat is carried through to the results in Appendix F.4 rather than treated as a reason to withhold them.

Selected methods. The method retained for each sample is reported in Table F3 for the violation-case design and Table F4 for the donation design.

Table F2: **Covariate sets compared, penalty analysis.** Medians across samples for the method selected in each specification.

	Mean |SMD|	Share > 0.10	ESS ratio	Relaxed
Comparison of violation cases (ownership origin)				
Main covariates, matching	0.033	0.06	0.44	0/12
Main covariates, weighting	0.054	0.16	0.42	4/10
All covariates, matching	0.028	0.06	0.14	0/16
All covariates, weighting	0.062	0.19	0.42	10/20
Protection via campaign donations				
Main covariates, matching	0.067	0.23	0.24	1/12
Main covariates, weighting	0.064	0.22	0.23	**11/12**
All covariates, matching	0.067	0.24	0.19	10/96

Notes: Balance statistics are computed on the full diagnostic covariate set under every specification. "ESS ratio" is effective sample size as a share of the nominal sample. "Relaxed" counts samples whose selected method was admitted only after the no-serious-warning gate was dropped.

Table F3: **Selected preprocessing methods, comparison of violation cases (main covariates).**

Comparison	Subsample	Matching	Weighting
Foreign vs. In-State	Democratic Stronghold	`optimal_1`	`optweight_05`
	Swing State	`quick`	`gbm`
	Republican Stronghold	`full_caliper_02`	`optweight_10`
Out-of-State vs. In-State	Democratic Stronghold	`optimal_1`	`gbm`
	Swing State	`optimal_1`	`gbm`
	Republican Stronghold	`optimal_3`	`gbm`

Notes: Each row reports the preprocessing method the selection rule of Appendix E.1 retained for that sample: methods failing the convergence, degeneracy, or treated-retention screen are excluded, survivors are ranked on covariate balance, and ties are broken on effective sample size. Method names are those used by `MatchIt` and `WeightIt`.

Table F4: **Selected preprocessing methods, donation design (main covariates).**

Ownership	Subsample	Matching	Weighting
Foreign-Owned	Democratic Stronghold	`optimal_1`	`optweight_03`
	Swing State	`optimal_3`	`super`
	Republican Stronghold	`nearest_3`	`optweight_10`
Out-of-State Domestic	Democratic Stronghold	`cardinality`	`gbm`
	Swing State	`optimal_1`	`cbps`
	Republican Stronghold	`nearest_1`	`gbm`

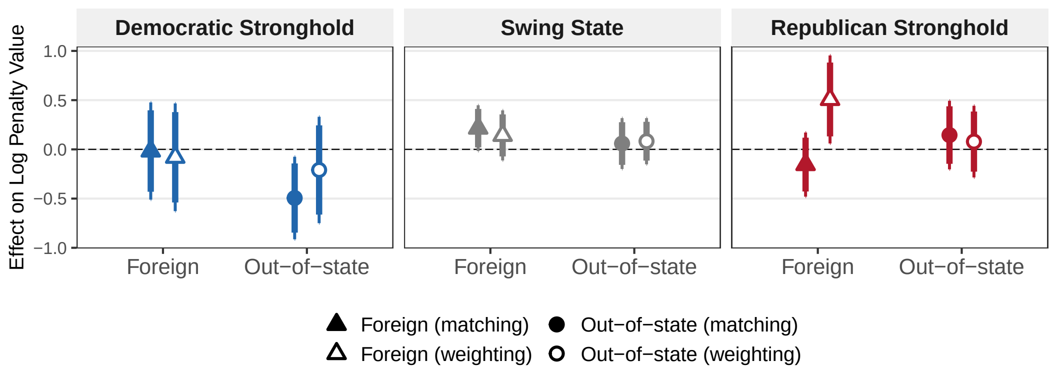
Notes: Each row reports the preprocessing method the selection rule of Appendix E.1 retained for that sample: methods failing the convergence, degeneracy, or treated-retention screen are excluded, survivors are ranked on covariate balance, and ties are broken on effective sample size. Method names are those used by `MatchIt` and `WeightIt`. Treatment is a donation to state candidates in the five years before the violation, the definition used throughout; the selection was run separately for each treatment definition estimated, and the retained methods differ little across them.

Outcome regression. The outcome regression takes the same form as in the greenfield analysis, without the age interaction, since this design conditions on a violation rather than following facilities across their life cycle. Outcome regressions are estimated on the preprocessed sample with industry-by-year, state-by-year, and TRI-status fixed effects, weighted by the matching or weighting weights, with standard errors clustered by facility. No matched-pair effect is included. Clustering at the facility rather than at the state-by-year level reflects that a single facility contributes multiple violation cases, which are the dependent observations here; the state-by-year dimension is already absorbed by the corresponding fixed effect. Estimates whose magnitude exceeds the observed range of the outcome in the estimation sample are reported in the tables and flagged, but omitted from the coefficient figures, where they would compress the scale of every other interval; such estimates arise in subsamples with weak overlap and should be read as diagnostics of that weakness rather than as effects.

F.4 Results

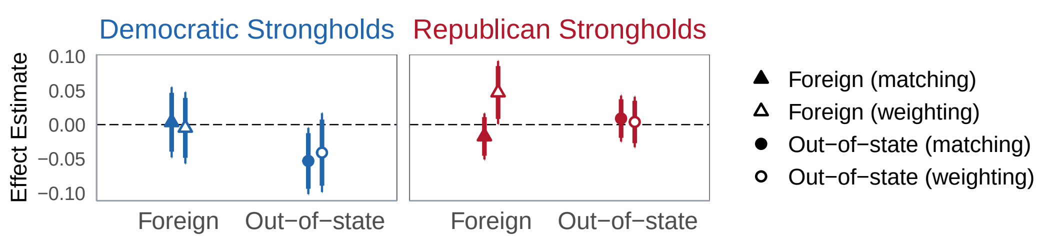
F.4.1 Comparison of Violation Cases

Figure F4: **Effect of external ownership on log penalty value, with swing states shown separately.**



Notes: As Figure 7, with swing states as a third group. Filled markers are the matching arm, hollow the weighting arm; thick bars are 90 percent intervals, thin bars 95 percent.

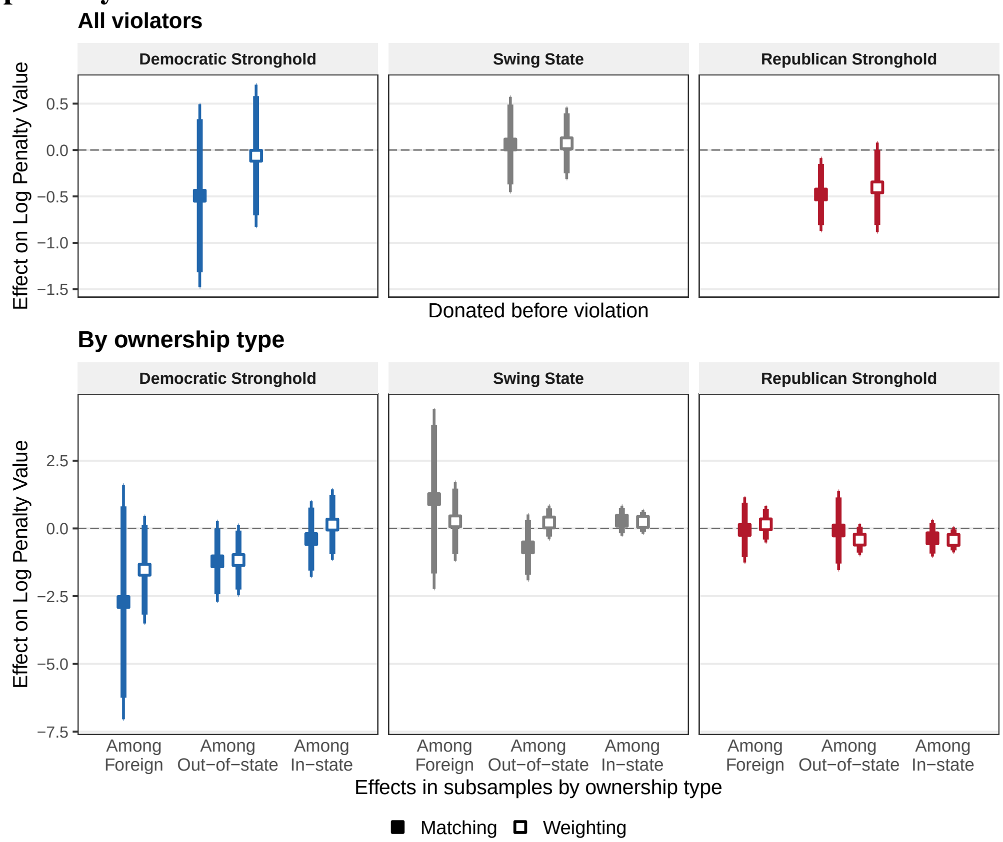
Figure F5: **Effect of external ownership on receiving any monetary penalty.**



Notes: Details follow Figure 7.

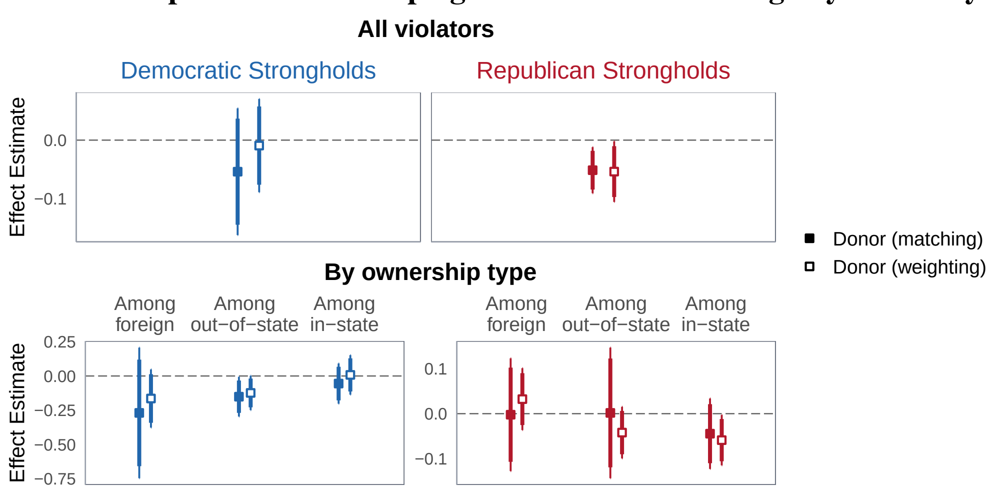
F.4.2 Securing Protection via Campaign Donations

Figure F6: **Effect of pre-violation campaign donation on log penalty value, with swing states shown separately.**



Notes: As Figure 8, with swing states as a third group. Vertical scales differ across panels. Filled markers are the matching arm, hollow the weighting arm; thick bars are 90 percent intervals, thin bars 95 percent.

Figure F7: **Effect of pre-violation campaign donation on receiving any monetary penalty.**



Notes: Details follow Figure 8.

F.4.3 Results from Alternative Specifications

Both analyses are re-estimated under both covariate sets described in Appendix F.3, and the estimates are reported numerically rather than as coefficient plots so that the two arms can be compared row by row. Table F5 gives the comparison of violation cases and Table F6 the donation analysis; Panel A of each is the matching arm and Panel B the weighting arm.

Table F5: **Effects of external ownership on enforcement outcomes, across specifications.**

	Log penalty value			Any formal action		
Specification	Dem.	Swing	Rep.	Dem.	Swing	Rep.
Panel A. Matching						
Foreign vs. in-state ownership						
Main covariates	−0.017	0.216^{+}	−0.155	0.015	0.011	−0.017
	(0.252)	(0.119)	(0.166)	(0.026)	(0.012)	(0.017)
All covariates (categories)	0.169	0.064	0.022	0.033	−0.016	−0.011
	(0.210)	(0.114)	(0.144)	(0.024)	(0.012)	(0.016)
All covariates (codes)	0.547*	0.075	0.056	0.060*	−0.009	−0.012
	(0.239)	(0.114)	(0.141)	(0.025)	(0.012)	(0.017)
Out-of-state vs. in-state ownership						
Main covariates	−0.494*	0.059	0.145	−0.027	−0.003	−0.002
	(0.214)	(0.132)	(0.178)	(0.026)	(0.014)	(0.018)
All covariates (categories)	−0.649*	0.058	−0.083	−0.060*	0.001	−0.023^{+}
	(0.282)	(0.125)	(0.109)	(0.029)	(0.012)	(0.013)
All covariates (codes)	−0.489*	0.078	−0.191	−0.048^{+}	−0.006	−0.027*
	(0.241)	(0.114)	(0.123)	(0.028)	(0.012)	(0.013)
Panel B. Weighting						
Foreign vs. in-state ownership						
Main covariates	−0.080	0.142	0.507*	−0.003	−0.001	0.039
	(0.279)	(0.130)	(0.228)	(0.026)	(0.013)	(0.025)
All covariates (categories)	0.014	0.175	0.466*	0.004	0.002	0.035
	(0.289)	(0.142)	(0.226)	(0.026)	(0.014)	(0.024)
All covariates (codes)	−0.080	0.101	0.288	−0.003	−0.005	0.013
	(0.279)	(0.112)	(0.205)	(0.026)	(0.011)	(0.023)
Out-of-state vs. in-state ownership						
Main covariates	−0.209	0.083	0.079	−0.039	−0.004	−0.002
	(0.276)	(0.120)	(0.186)	(0.028)	(0.013)	(0.020)
All covariates (categories)	−0.022	0.037	−0.015	−0.013	−0.010	−0.012
	(0.340)	(0.133)	(0.188)	(0.034)	(0.014)	(0.020)
All covariates (codes)	−0.048	−0.017	0.061	−0.012	−0.015	−0.005
	(0.341)	(0.123)	(0.195)	(0.035)	(0.013)	(0.021)

Notes: Each row is a separate specification estimated on the same violation cases; "main covariates" is the specification reported in the main text. The all-covariate variants add the full set of sparse program and pollutant indicators, coded either as pollutant categories or as individual codes. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table F6: **Effect of pre-violation campaign donation on enforcement outcomes, across specifications.**

	Log penalty value			**Any formal action**		
Specification	Dem.	Swing	Rep.	Dem.	Swing	Rep.
Panel A. Matching						
Main covariates	-0.493	0.060	-0.479^{*}	0.008	0.012	-0.048^{*}
	(0.502)	(0.262)	(0.201)	(0.056)	(0.027)	(0.021)
All covariates (categories)	-0.345	-0.055	-0.187	0.001	-0.027	-0.046^{+}
	(0.473)	(0.316)	(0.268)	(0.052)	(0.035)	(0.028)
All covariates (codes)	-0.931^{+}	0.149	-0.497^{*}	-0.063	0.006	-0.055^{**}
	(0.479)	(0.307)	(0.205)	(0.051)	(0.034)	(0.020)
Panel B. Weighting						
Main covariates	-0.061	0.072	-0.403	0.012	-0.000	-0.055^{*}
	(0.391)	(0.197)	(0.246)	(0.044)	(0.021)	(0.026)
All covariates (categories)	0.003	0.108	-0.355^{*}	0.018	0.003	-0.032^{*}
	(0.397)	(0.202)	(0.153)	(0.045)	(0.021)	(0.016)
All covariates (codes)	-0.084	0.084	-0.218	0.013	0.002	-0.022
	(0.396)	(0.201)	(0.159)	(0.045)	(0.021)	(0.017)

Notes: Each row is a separate specification; "main covariates" is the specification reported in the main text. Standard errors clustered by facility in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.