Borders Bear the Brunt: State Environmental Review and Severed Agglomerations

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January 25, 2026

Abstract

We study how state environmental review laws (SEPAs) shape local business activity. Using a stacked, border-pair event design around staggered SEPAs, we compare adjacent counties on either side of state lines. SEPA-side border counties experience slower post-adoption growth that cumulates into persistent level losses over two decades, with no mirror-image gains across the border and no broad underperformance in state interiors. Effects are strongest for SEPA-treated counties that border large metro areas, when SEPAs have substantive provisions, and when states devote more resources to environmental protection, and they fall disproportionately on pollution-intensive sectors and small firms. In sum, local environmental regulations can erode local agglomerations, adversely affecting local small business growth.

JEL classification: Q58, R11, R58, H73, L51

Keywords: Real effects of regulation; State environmental review; SEPA; border discontinuity; business formation; small firms; agglomeration economies.

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1 Introduction

Economic theory suggests that environmental policy can add value by reducing pollution externalities but also imposes economic costs.[1] Empirical work on major federal regulations—most prominently the Clean Air Act—documents large health and amenity benefits alongside meaningful compliance and adjustment costs (e.g., Greenstone, 2002; Shapiro and Walker, 2018; Walker, 2013; Currie and Walker, 2019). By contrast, the economic impacts of *subnational* environmental regulations in the U.S. typically appear small or difficult to detect, with leading state-level studies finding limited effects on manufacturing location and output (Levinson, 1996; Henderson and Millimet, 2005; Millimet and List, 2004).[2] Whether this dearth of evidence reflects genuinely low real effects of state policies, offsetting costs and benefits, or limitations of prior research designs is an open and important question. If economic costs of state interventions are low or offset by health, amenity, or innovation gains (Chay and Greenstone, 2005; Isen, Rossin-Slater, and Walker, 2017; Porter and Linde, 1995), the optimal level of subnational protections is higher than if they significantly depress local business activity.

In this paper, we offer new evidence on how state environmental review laws affect local business growth and composition. We focus on State Environmental Protection Acts (SEPAs)—state "mini-NEPAs" that extend the procedural requirements of the National Environmental Policy Act (NEPA) to state and local decisions. Unlike NEPA, which is typically triggered only for large projects involving federal funding, permits, or land (Mandelker, Glicksman, Aughey, McGillivray, and Doelle, 2024), SEPAs embed environmental review into *routine* permitting, zoning, and public funding. Any private project that requires a local permit, land-use approval, or public support can face additional documentation, mitigation conditions, and litigation risk. This feature makes SEPAs economically salient

[1] Lots of cites here, e.g., Coase (1960); Copeland and Taylor (1994).

[2] We focus on U.S. policy impacts; international evidence on the real effects of environmental regulation is also mixed (e.g., He, Wang, and Zhang, 2020; De Simone, Naaraayanan, and Sachdeva, 2024).

where marginal business decisions are made. The localized geography of SEPAs also changes the mix of channels through which policy affects outcomes: when regulation is local, the relevant counterfactual for many projects is "just across the line," making cross-border relocation cheaper and raising the risk of severing otherwise integrated agglomerations of firms, workers, and suppliers (Greenstone, Hornbeck, and Moretti, 2010; Redding and Rossi-Hansberg, 2017; Monte, Redding, and Rossi-Hansberg, 2018).

Our empirical strategy exploits these features to identify where and how state environmental policies matter for local businesses. SEPAs were adopted in a staggered fashion by sixteen states, mostly in the early 1970s, generating sharp policy discontinuities at borders between counties in SEPA states and their neighbors in non-SEPA states. Because adjacent border counties share geography, infrastructure, and access to regional markets but face different permitting frictions once a SEPA is in force, changes in their relative outcomes offer a clean measure of the real effects of state environmental review. We implement a stacked border-pair event design centered on SEPA adoption: for each adopting state, we form matched pairs of adjacent counties on either side of the border and follow them before and after enactment. The specification includes county fixed effects and county-pair-by-year fixed effects, so identification relies purely on within–border-pair differences in each year and is robust to staggered-adoption concerns highlighted by Borusyak, Jaravel, and Spiess (2024).

A key ingredient to our analysis is a newly extended panel dataset on county business establishments that dates back to the mid-1960s, prior to the earliest SEPA adoption. Building on recent work reconstructing early County Business Patterns (CBP) files (Eckert, Lam, Mian, Müller, Schwalb, and Sufi, 2022), we extend county-level information on establishment counts, employment, payroll, industry, and size back to the mid-1960s and follow counties for several decades after SEPA adoption. This allows us to study the immediate dynamics of the local economy surrounding SEPA adoption and the decades-long economic impacts of SEPA adoption, both in aggregate and across firm size and industry distributions.

Our empirical analysis begins by documenting the immediate impact of SEPA adoption on local business activity at borders. Using an event window around each adoption, we compare the evolution of establishments and employment in counties on the SEPA side of a border to their adjacent non-SEPA neighbors. In the five years after adoption, SEPA-border counties experience substantially slower growth than non-SEPA border counties, and over the subsequent two decades the level of activity on the SEPA side remains markedly lower. The pattern is widespread across most of the sixteen SEPA-adopting states and is especially pronounced where states devote more resources to environmental protection, consistent with more intensive SEPA implementation. These initial findings rule out the view that state environmental review laws have negligible economic impact.

On its own, however, a wedge between SEPA and non-SEPA border counties does not reveal why outcomes diverge. In principle, the gap could reflect (i) deterrence and delay of marginal projects when review raises fixed and timing costs (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991), (ii) relocation across the border if projects remain profitable but move into the less-regulated regime (Levinson, 1996; List, Millimet, Fredriksson, and McHone, 2003), or (iii) broader state-level shocks unrelated to SEPA. To separate these channels, we bring in three additional test samples. Within SEPA states, we compare border counties to interior counties to test whether declines concentrate at the cross-state margin where "just across the line" counterfactuals are salient. We also run the analogous comparison within non-SEPA states to ask whether SEPA-side losses show up as gains next door. Finally, we compare interior counties of SEPA and non-SEPA states away from borders to probe for broader state-level shocks. Together, these analyses help triangulate the mechanism through which SEPAs impact local business activity.

We find that, following SEPA adoption, business growth in SEPA border counties lags that of the interiors of their own states. Growth in non-SEPA border counties does not systematically outpace (and if anything grow less than) their interiors, and there is similar growth in the interiors of SEPA and non-SEPA states. In short, the SEPA-induced decline

in business growth is localized to SEPA border counties, ruling out statewide shocks and large contemporaneous relocation as first-order explanations. Instead, the evidence is most consistent with a cut-off-agglomerations mechanism whereby SEPAs deter some investments from nearby businesses across the border (e.g., firms choosing not to expand across the line into a SEPA regime), and the loss of these projects shrinks nearby supplier, labor, and knowledge networks (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017; Monte et al., 2018). Further bolstering this interpretation, we find that the adverse effects of SEPAs on border counties are more pronounced when the SEPA-treated border county is adjacent to a large metropolitan area, and especially so when the SEPA-treated county itself is not in as large of a metropolitan area.

We then ask whether these localized declines persist or are offset over time. Comparing a pre-adoption window to a window two decades after enactment, SEPA border counties remain substantially smaller in establishments and employment than adjacent non-SEPA borders, while SEPA and non-SEPA interiors again look similar. We do not see the recovery one would expect if amenity gains dominated in the long run (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018), nor sustained outperformance on the non-SEPA side that would signal large, persistent relocation. Instead, the long-run evidence indicates that the initial border-localized deterrence is not fully undone by amenity rebalancing and that agglomeration erosion remains consequential for decades.

We next examine who bears the brunt of these changes, since environmental review falls more heavily on pollution-intensive sectors and fixed compliance costs load disproportionately on small and mid-sized establishments.[3] Consistent with our predictions, the SEPA-induced contraction concentrates in pollution-intensive sectors, which show especially large relative declines in establishments and employment (Dechezleprêtre and Sato, 2017; Hafstead and Williams III, 2018). At the same time, we find negative effects across most major sectors,

[3]See, e.g., Jaffe, Peterson, Portney, and Stavins (1995); Greenstone (2002) on exposure of dirty sectors and Barth and McNichols (1994); Ryan (2012) on fixed and timing costs for smaller firms.

indicating that SEPA impacts are not confined to a narrow set of regulated industries. On the firm side, the decline in establishments is driven overwhelmingly by small and, to a lesser extent, medium-sized firms—those for whom fixed compliance and delay costs represent a large share of expected returns (Joshi, Krishnan, and Lave, 2001; Ryan, 2012). Large firms, by contrast, appear largely insulated, consistent with their greater capacity to absorb compliance costs and reorganize projects (Becker and Henderson, 2000).

Summary and Contributions. State environmental review laws have sizable, uneven, and spatially sharp real effects. SEPAs reduce business activity where policy regimes meet low relocation costs and dense local linkages—at state borders—yet these losses are not offset by gains next door or by broad shifts in state interiors. The pattern fits a deterrence-and-agglomeration mechanism better than relocation or statewide shocks, implying that the economic footprint of environmental regulation can be larger and more geographically concentrated at the state level than suggested by earlier aggregate studies.

Our empirical design leverages the fine geography of borders through stacked adjacent-county event comparisons around SEPA adoption, with county and county-pair-by-year fixed effects. Auxiliary contrasts within SEPA states (border vs. interior), within non-SEPA states, and across interiors rule out statewide shocks and simple reshuffling, while speaking directly to interjurisdictional competition and the sensitivity of local activity to policy differentials where cross-state substitution is cheap (Holmes, 1998; Dube, Lester, and Reich, 2010; Oates and Schwab, 1988; Suárez Serrato and Zidar, 2016).

The results connect to the spatial-shock and agglomeration literature by showing persistent, border-localized losses with no mirror-image gains across the line. When marginal projects are deterred at the boundary, the surrounding supplier, labor, and knowledge networks shrink, and the local economy re-levels at a lower scale (Greenstone et al., 2010; Combes, Duranton, and Gobillon, 2012; Redding and Rossi-Hansberg, 2017; Monte et al., 2018). This mechanism is consistent with local multipliers and network propagation, where small changes

at the project margin can transmit through buyer-supplier and commuting links (Moretti, 2010; Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012). Because SEPAs operate through routine permitting and land-use processes, the bite falls on precisely the incremental expansions that sustain dense urban networks, which helps explain why we observe sharp effects at borders rather than broad state-wide declines (Mandelker et al., 2024; Renz, 1984).

We also contribute to the environmental real-effects and incidence literatures by linking who bears the burden to basic corporate-finance margins. Procedural review raises fixed and timing costs and compresses expected cash flows, reducing the set of positive-NPV projects for smaller establishments, which lack scale to amortize compliance and delay (Barth and McNichols, 1994; Joshi et al., 2001; Ryan, 2012). Consistent with this mechanism, pollution-intensive sectors contract most (Greenstone, 2002; Berman and Bui, 2001; Dechezleprêtre and Sato, 2017; Hafstead and Williams III, 2018), and the establishment margin is driven overwhelmingly by small and, to a lesser extent, medium-sized firms, while large firms appear comparatively insulated (Becker and Henderson, 2000). The subnational setting further amplifies finance-relevant channels: low relocation costs enable regulatory arbitrage and cross-border sorting (Becker and Henderson, 2000; Bartram, Hou, and Kim, 2022), and stakeholder forces shape firms' environmental responses in ways that can reinforce incidence on smaller, local businesses (Schiller, 2018; Duchin, Gao, and Xu, 2025). These patterns complement federal studies documenting substantial benefits and adjustment costs (Greenstone, 2002; Walker, 2011, 2013; Shapiro and Walker, 2018) while clarifying that state-level procedural review operates on more local, finance-salient margins than national rules.

Finally, our findings highlight the differential implications of state versus national policy. Where relocation across borders is cheap, and agglomerations are local, state review generates sharp, durable border effects that national, geographically diffuse rules may not reveal. The incidence is likely heterogeneous across states: smaller states, or those with a larger share of activity near borders, face stronger exposure to otherwise similar review regimes. Our estimates are intentionally local and complement rather than substitute for federal evidence on

health, amenity, and innovation gains (Roback, 1982; Porter and Linde, 1995; Isen et al., 2017; Shapiro and Walker, 2018). Together, the border design, agglomeration-based mechanism tests, and incidence by sector and firm size provide a unified account of how and where state environmental review reshapes the private economy, and why its real effects can systematically differ from those of national policies.

2 Motivation: Environmental policy & business growth

Environmental policy can mitigate negative externalities and improve health and amenities, but it also changes the private returns to operating a business. Broad environmental review frameworks—prominently NEPA and the state "mini-NEPAs" (SEPAs)—impose both direct costs (consultants, studies, legal) and indirect costs (longer timelines, uncertainty, litigation risk) that can lower project values at the margin (Becker and Henderson, 2000; Pindyck, 1991; Ryan, 2012). At the same time, cleaner air and water and reduced environmental risk can raise local quality of life and stimulate consumer-facing activity (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). Because these forces operate in opposite directions and on different horizons, the net effect on local business activity is ambiguous *a priori*.

In the U.S., these tradeoffs most directly meet everyday business decisions at the state level. Unlike NEPA—which is triggered mainly when federal action is involved (Mandelker et al., 2024)—SEPAs extend similar procedural requirements to *state and local* permitting, zoning, and public funding, sweeping in the typical private establishment that needs a building permit, land-use approval, or utility connection. This broader reach means SEPAs can impose fixed compliance costs and timing risk on small and mid-sized projects (Ryan, 2012; Bartram et al., 2022), exactly where marginal NPV decisions are made. Yet systematic state-level evidence remains surprisingly thin: canonical work either studies federal policy or state differences that do not map cleanly onto SEPA-style review (e.g., Clean Air Act attainment or

manufacturing location) (Greenstone, 2002; Levinson, 1996; Henderson and Millimet, 2005). This gap is noteworthy given how often states—not the federal government—govern the permitting frictions and timelines that shape local business formation.

State policy also operates on more localized geographies. When relocation costs are low—as they often are across a state line—the relevant counterfactual for marginal projects is "just across the line," so even modest fixed or timing costs can tip decisions and re-sort activity (Levinson, 1996; Henderson and Millimet, 2005). At the same time, agglomeration gains and losses are intensely local: trimming borderline projects on one side can thin nearby supplier, labor, and customer networks without producing a mirror-image boom next door (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017). These features make a border-based design particularly informative for isolating the real effects of state environmental policies and underscore that incidence can differ when relocation margins are salient and cheap—precisely the case for SEPAs, less so for federal rules (Mandelker et al., 2024; Greenstone, 2002).

2.1 Testable tension at state borders

The business impact of SEPAs is an empirical question. Procedural review and policy uncertainty can deter marginal projects or slow firm expansion (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991), while improved environmental quality can raise amenity values and bolster local demand—especially for consumer-facing activity (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). State borders provide a clean setting to study this tradeoff since adjacent counties share geography and market access but face different permitting frictions once SEPA is in force (Levinson, 1996; Henderson and Millimet, 2005; Greenstone, 2002). If compliance costs dominate, business growth should be lower on the SEPA side. If, instead, amenity gains are salient, level differences may attenuate, and the composition of activity may shift toward industries that benefit from a cleaner environment. We use timing (short- vs. long-run) and space (border vs. interior) to separate the empirical

relevance of these mechanisms and shed new light on how state environmental policy affects local business growth.

2.1.1 Primary mechanisms (immediate impact channels)

SEPAs can change outcomes on the SEPA side relative to the non-SEPA side through three conceptually distinct channels:

(i) Deterrence/delay. Procedural review and litigation risk raise fixed and timing costs, pushing borderline projects below profitability. *Prediction:* an immediate contraction in business formation and employment on the SEPA border, especially for permitting-intensive, project-based establishments (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991).

(ii) Relocation across the line. If projects remain profitable but are mobile, activity shifts into adjacent non-SEPA border counties. *Prediction:* a contemporaneous dip on the SEPA border and an uptick on the non-SEPA border relative to non-SEPA interiors (Levinson, 1996; List et al., 2003; List, McHone, and Millimet, 2004).

(iii) Weak local bite. If firms anticipate or absorb review costs, SEPA may have little immediate effect. *Prediction:* muted short-run differences at the border, with outcomes tracking closely on both sides (Henderson and Millimet, 2005; Millimet and List, 2004; Been, Knafo, and Hacohen, 2021).

2.1.2 Modulators (amenities and agglomeration across horizons)

Amenity and agglomeration effects can amplify or offset these immediate impacts. These modulators may emerge as important drivers of SEPA's business impact either quickly or gradually, but they are unlikely to exist in the absence of any immediate economic impact of SEPAs. For example, if no projects are relocated or deterred then agglomerations will be unaffected and there will be no shift in amenities. Below we outline predictions regarding the

manner in which amenities and agglomerations effects will modulate SEPA's business impact.

Amenities. If SEPA improves environmental quality that households value, local demand can rise—sometimes quickly via expectations and place branding, and more slowly via migration and housing adjustment. *Prediction:* attenuation of initial level gaps and a composition shift toward cleaner, consumer-facing sectors on the SEPA side (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). Operationally, we expect attenuation in levels and a rising share of clean, consumer-facing industries on the SEPA side.

Agglomeration. Losing borderline projects can shrink nearby supplier, labor, and knowledge networks immediately (canceled contracts, thinner customer flow) and continue to do so as scale economies erode over time. *Prediction:* persistent or slowly decaying differences concentrated in SEPA border counties, weak differences in SEPA interiors, and no systematic uplift on the non-SEPA border relative to its interiors—consistent with localized external returns to scale and limited short-run absorptive capacity across the line (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017; Monte et al., 2018).

2.1.3 Putting the pieces together: predictions by mechanism mix

Combining primary mechanisms with modulators yields several plausible impact patterns:

- *Deterrence plus agglomeration cut-offs:* immediate SEPA-border declines; over time, effects remain localized to SEPA borders; SEPA interiors remain closer to non-SEPA interiors; non-SEPA borders do not systematically outpace non-SEPA interiors (Becker and Henderson, 2000; Ryan, 2012; Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017).

- *Deterrence with amenities offset:* a near-term dip on the SEPA border that attenuates; composition tilts toward cleaner, consumer-facing sectors; the long-run level gap narrows (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018).

- *Relocation reinforced by spatial sorting:* an immediate SEPA-side dip paired with a contemporaneous non-SEPA border uptick; over time, relocation intensifies as agglomeration and amenity forces concentrate compatible industries on either side of the line. Interiors across states need not differ; long-run SEPA-side levels need not remain depressed if capacity expands across the border (Levinson, 1996; List et al., 2003; Suárez Serrato and Zidar, 2016).

- *Weak local bite (amenities/agglomeration muted):* little short-run movement at the border and limited longer-run divergence; any changes reflect broader trends rather than border-specific mechanisms (Henderson and Millimet, 2005; Millimet and List, 2004).

2.2 Who is most exposed? Dirty industries and small firms

Heterogeneity by industry and size clarifies the mechanism. Environmental review and mitigation requirements fall most heavily on pollution-intensive sectors, where compliance raises fixed and timing costs and often requires abatement investment or process redesign (Jaffe et al., 1995; Dechezleprêtre and Sato, 2017; Hafstead and Williams III, 2018; Greenstone, 2002; Fowlie, 2010). We therefore expect a relative decline in "dirty" activity following SEPA adoption—even if cleaner sectors exhibit resilience or growth as amenities improve (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018; Addoum, Gounopoulos, Gustafson, Lewis, and Nguyen, 2023).

Firm size is an equally important margin. Because much of the regulatory burden is fixed—consulting, legal, documentation, and the option value of time under uncertainty—small establishments face disproportionately higher effective costs per dollar of expected revenue (Barth and McNichols, 1994; Joshi et al., 2001; Ryan, 2012). Larger firms can amortize these costs, rely on dedicated compliance capacity, and better withstand delays; in some settings they may even gain as marginal competitors exit (Becker and Henderson, 2000). We therefore

anticipate that any overall decline in establishment counts is driven primarily by small (and especially small-dirty) firms, with muted effects for large incumbents.

3 Identification Strategy

Existing evidence on the business impact of sub-national environmental regulations is mixed, often finding insignificant or heterogeneous effects of environmental policy (Levinson, 1996; Currie and Walker, 2019; Bartram et al., 2022; Henderson and Millimet, 2005). Our empirical strategy is designed to focus on regulatory frameworks that are both comprehensive and binding at the local level, where business decisions are made. We identify such settings by examining the staggered adoption of State Environmental Protection Acts (SEPAs)—state-level analogs to the federal National Environmental Policy Act (NEPA)—and by exploiting the sharp contrasts at state borders between SEPA and non-SEPA jurisdictions.

3.1 SEPAs and Their Business Impact

The National Environmental Policy Act (NEPA), enacted in 1969, is an "action-forcing" procedural statute requiring federal agencies to assess and disclose the environmental impacts of major projects. NEPA does not set substantive pollution limits; rather, it mandates a structured process of information gathering, documentation, and public disclosure. Its central requirement is a "detailed statement" describing environmental consequences of major federal actions (Mandelker et al., 2024). In practice, NEPA is triggered by federal funding, federal permits, or federal land—for example, infrastructure projects near wetlands or developments on public lands.

The process is tiered. Projects categorically deemed to have no significant environmental effects are exempt. Others undergo an *Environmental Assessment* (EA) to determine significance. If impacts are not significant, the agency issues a *Finding of No Significant*

Impact (FONSI); otherwise, a full *Environmental Impact Statement* (EIS) is required, including analysis of alternatives, mitigation plans, and public comment periods that can mobilize opposition and spur litigation (Singerman and Kersten, 2023). From a business perspective, NEPA imposes both *direct compliance costs*—consultant and legal fees, technical studies, documentation—and *timing/uncertainty costs* that delay completion, erode NPV, and raise abandonment risk (Ryan, 2012; Pindyck, 1991). Because NEPA applies mainly when federal action is involved, most small and mid-sized businesses operating solely under state and local permits rarely trigger it; its bite is concentrated in large or federally tied projects.

Following NEPA, many states enacted "mini-NEPAs," or State Environmental Policy Acts (SEPAs), extending similar procedural requirements to state and local governments and, crucially, to private projects requiring state or municipal permits, zoning approvals, or public funds. Figure 1 maps the sixteen SEPA-adopting states and adoption years (clustered in the early 1970s, with California's CEQA in 1970 as a prominent early example). Unlike NEPA, SEPAs reach a much broader range of private economic activity because nearly every new commercial build or business expansion requires some form of local permit or land-use approval. A small hotel, restaurant, or warehouse may trigger SEPA review if building permits, traffic assessments, or public-utility connections are implicated. As a result, SEPAs are far more relevant for the *everyday* business environment and provide a powerful setting to study the economic effects of environmental regulation.

The mechanisms through which SEPAs affect firms mirror NEPA's but are more pervasive. Direct costs arise from consultant fees, legal expenses, and preparation of environmental documents; indirect costs stem from longer review timelines, public participation, and litigation risk. Together, these steps introduce uncertainty that can deter investment and disproportionately discourage smaller firms from expansion. Figure A.I reproduces a CEQA EIR page listing potential impacts and required mitigation measures before construction can begin, while Figure A.II illustrates the open, participatory process via a public-hearing notice. Public comment solicits scrutiny from community groups and environmental advocates,

heightening reputational and political risks and increasing the likelihood of delay or suit. These examples show how procedural mandates translate into concrete financial and operational burdens for private businesses.

Although the procedural blueprint is similar, SEPAs apply to a much broader set of projects than NEPA, effectively integrating environmental review into the *standard* local permitting process for commercial and industrial development. Because most projects require land-use, zoning, utility, or occupancy approvals, SEPA review can be a routine step rather than a rare federal contingency. This broader scope means that SEPAs shape day-to-day decisions about entry, expansion, and location in ways that are directly relevant for local business formation and employment.

The flow of a SEPA review also parallels NEPA but operates at a more granular scale. Figures 2 and 3 depict CEQA as an example. The initial screening phase determines whether a proposal is a "project," assesses exemptions, and, if needed, conducts an Initial Study. The full EIR pathway then proceeds through a Notice of Preparation, public scoping, Draft EIR circulation, comment and response, and final certification or denial. Each step carries administrative cost and delay and creates additional opportunities for public challenge. While designed to promote transparency and environmental stewardship, these procedures can create meaningful frictions for local businesses.

From a corporate-finance perspective, SEPAs magnify the same economic frictions identified for NEPA but apply them to a vastly larger set of firms. The combination of compliance expenses and timing risk reduces expected project returns, particularly for small, leveraged, or time-sensitive investments, pushing marginal projects below profitability and occasionally leading to cancellation. In short, SEPAs operationalize NEPA's informational mandate at the state and local levels, converting disclosure requirements into pervasive local costs. Figures A.I, A.II, 2, and 3 together illustrate how environmental review evolves from a high-level federal framework into a routine, local permitting layer. These institutional details

motivate our empirical strategy, which exploits staggered SEPA adoption across states and sharp border discontinuities between SEPA and non-SEPA jurisdictions to estimate causal effects on local business activity.

3.2 Empirical Specification

Our empirical design exploits the staggered adoption of State Environmental Protection Acts (SEPAs) across states and examines how local business activity evolves in counties located along SEPA–non-SEPA state borders. Because adjacent border counties share geography, climate, infrastructure, and regional economic conditions—but face different permitting procedures once a SEPA takes effect—changes in their relative performance offer a clean measure of how environmental review affects everyday business activity.

We implement an event-study framework centered on each state's SEPA adoption year. For each adoption event, we build a matched border pair: a county on the SEPA side of the border and its immediately adjacent county across the line. All border pairs across adopting states are then combined ("stacked") so that each adoption event contributes a localized comparison. The stacked event design ensures that estimates reflect only neighboring-county contrasts at each adoption date, rather than long-term average differences across distant places.

To isolate local, time-varying differences within each pair, we include *county* and *border-pair-by-year* fixed effects. These absorb all common shocks—economic cycles, regional booms and busts, shared labor markets, or national policy changes—and force the identifying variation to come from changes in relative outcomes *within* each border pair at each point in time. Because comparisons are always local and contemporaneous, the design is robust to staggered policy timing, heterogeneous county characteristics, and persistent cross-state differences.

Formally, for each outcome $Y_{i,j,t}$ (establishments, employment, sectoral composition, etc.)

for county i in border pair j at time t, we estimate:

$$Y_{i,j,t} = \alpha_i + \gamma_{j,t} + \beta_1\big(SEPA_i \times Post_{j,t}\big) + \epsilon_{i,j,t}, \tag{1}$$

where α_i are county fixed effects and $\gamma_{j,t}$ are border-pair-by-year fixed effects. In addition to the baseline specification above where we measure the relative shift in outcomes on the SEPA side of the border after SEPA adoption with β_1, we also conduct separate event-time analyses where we trace how relative differences evolve in the years leading up to and following SEPA adoption. Standard errors are clustered at the border-pair level.

Two features of this specification are central. First, county fixed effects absorb all permanent differences across counties (size, industrial structure, demographics). Second, border-pair-by-year fixed effects absorb all annual shocks shared by both sides of a border—business cycle fluctuations, natural disasters, regional infrastructure investments, or contemporaneous state policies. As a result, identification comes entirely from changes in *relative* economic activity across a thin geographic boundary where the only sharp institutional change is the onset of SEPA review.

This approach provides the benefits normally associated with quasi-experimental border comparisons—sharp institutional contrasts, shared local conditions, and minimized unobservables—without relying on the language of "treated" or "control" units. Instead, the interpretation is purely comparative: when environmental review becomes more stringent on one side of a shared local economy, how do differences in establishment growth, employment, or business composition evolve?

3.3 Estimation Samples

Section 3.2 delivers border-pair estimates centered on SEPA adoption. Guided by Section 2, we read these estimates through two complementary lenses: (i) *short-run* responses that

speak most directly to on-impact channels (deterrence/delay, relocation, or weak local bite), and (ii) *long-run* differences that reveal whether amenities offset early losses or whether agglomeration forces entrench them. A set of within-state and inner-county comparisons then isolates where the cross-border gap originates—at the border margin versus statewide—and whether any decline on the SEPA side is mirrored across the line.

3.3.1 Event Windows

We estimate our short-run window impacts using an eleven-year event window—five years before adoption ($t-5$ to $t-1$) and five years after ($t+0$ to $t+5$)—using growth outcomes (annual percent changes in establishments and employment) as dependent variables. County fixed effects absorb time-invariant differences, and county-pair-by-year fixed effects restrict identification to within–border-pair contrasts in each year; standard errors are clustered at the county-pair level.

The short-run window is designed to reveal which on-impact channel dominates when a SEPA comes into force: deterrence/delay, relocation across the border, or a weak local bite. Because these channels operate where the policy applies most directly, border counties are the primary laboratory, and within-state contrasts indicate whether effects are localized to the border margin or reflect broader statewide forces.

We also conduct longer-window analyses. The long-run window assesses whether early movements fade, persist, or translate into spatial re-sorting after transitional dynamics have played out. For these tests, we compare levels between a five-year pre-period ($t-5$ to $t-1$) and a five-year window two decades after adoption ($t+20$ to $t+25$), estimating equation (1) with the natural logarithm of establishments and employment as outcomes. As in the short-run analysis, we include county fixed effects and county-pair-by-year fixed effects and cluster at the county-pair level. By omitting the transitional years, this "late post" window targets the new steady state and the cumulative divergence attributable to SEPA.

3.3.2 Test Samples

To understand the mechanisms behind any observed analyses we employ three test samples, in addition to our baseline analysis using county pairs that straddle the state border. First, we use a within-SEPA state comparison of border versus interior counties to test whether effects concentrate at the cross-state margin; a larger decline on the SEPA border than in SEPA interior counties is consistent with mechanisms acting where the "just-across-the-line" counterfactual is salient, rather than with statewide shocks. Second, a within–non-SEPA comparison of border versus interior counties serves as a placebo for relocation: a positive border–interior differential on the non-SEPA side indicates inflows from across the line, whereas a flat non-SEPA profile is inconsistent with simple reallocation of SEPA-side losses. Finally, comparing SEPA interior to non-SEPA interior counties away from the border tests for broad state-level shocks; weak or null differences there align with border-localized mechanisms. Taken together, these diagnostics ensure the immediate border-pair effect is not misread as a statewide disturbance and establish whether any short-run dip on the SEPA side is offset by contemporaneous gains next door.

3.4 Sample Construction

Our empirical analysis relies on a newly constructed county-level panel that exploits spatial and temporal variation in SEPA adoption. The sample design is central to our analysis for two main reasons.

First, because most SEPAs were enacted in the early 1970s, our framework requires a long historical dataset that captures several pre-adoption years and a multi-decade post-adoption horizon. To achieve this, we extend the *County Business Patterns* (CBP) data back to 1965, creating one of the most comprehensive county-level panels available to date. The resulting dataset spans 1965–2020 and includes annual information on establishments, employment, firm size, and industry composition. To our knowledge, this is the first study to compile and

employ a continuous CBP series covering nearly sixty years. Details of data construction, variable definitions, and consistency checks are provided in Appendix C.

Second, our empirical design exploits discontinuities at state borders, comparing counties that are geographically adjacent but subject to different regulatory regimes once a SEPA is adopted. This approach isolates the effect of SEPA-related procedural costs while holding constant local geography, demographics, and market access. Border counties are ideal for this comparison: relocation and migration costs are low, making them the most sensitive margin for regulatory frictions. Classical predictions from the *Pollution Haven Hypothesis* suggest that firms may relocate to jurisdictions with fewer environmental constraints, while residents may move to capture the amenity benefits of cleaner environments (Chay and Greenstone, 2005; Isen et al., 2017).

We begin with the universe of all U.S. counties from 1965 to 2020. A county is classified as a *treated county* if it lies within a SEPA-adopting state and shares a physical border with a county in a non-SEPA state. A *control county* is defined as the neighboring county across the border in a state that has not adopted a SEPA. This design ensures that control counties serve as credible counterfactuals for treated counties: adjacent border counties share similar baseline economic conditions, industrial structures, and infrastructure networks, differing primarily in their exposure to SEPA review.

The core of our dataset consists of unique treated–control "county pairs." Because border relationships are many-to-many, each treated county can border multiple control counties, and vice versa. We systematically generate all possible border pairs and then retain only unique combinations to avoid duplication. Figure A.VIIa illustrates this pairing logic using the Indiana–Illinois border, where Vigo and Sullivan counties in Illinois (treated) are matched with Clark and Crawford counties in Indiana (controls). The dashed lines represent unique treated–control pairs, capturing local economic contrasts across the regulatory boundary while preserving geographic adjacency.

To prevent large multi-border counties from dominating the regressions, each county pair is weighted by the treated county's total employment divided by the number of pairs it forms. For instance, if a treated county borders three control counties, each pair receives one-third of that county's employment as its analytical weight. This weighting ensures that each treated county contributes proportionally to the estimation, yielding representative average effects across all border economies.

Figures 1 and 4 summarize the geographic scope of the analysis. Figure 1 identifies the sixteen SEPA-adopting states and their respective years of enactment, while Figure 4 highlights the resulting network of border-pair counties. At the baseline year preceding SEPA adoption ($T-1$), the final analytical sample includes 502 unique treated–control county pairs, comprising 262 treated and 248 control counties drawn from 15 SEPA states and 23 non-SEPA states. On average, SEPA border counties host approximately 1,300 establishments employing about 21,000 workers, while non-SEPA border counties host around 1,500 establishments employing roughly 27,000 workers. The mean dirty-industry share of establishments is 23–24%, and small firms (fewer than 50 employees) account for more than 95% of all establishments in both groups.

Overall, these characteristics confirm that SEPA and non-SEPA border counties are economically comparable prior to adoption, supporting the validity of the border-pair framework. By focusing on adjacent counties that differ only in regulatory exposure, our design provides a geographically balanced setting to examine how environmental permitting regimes reshape local business activity, employment, and industrial composition.

3.5 Descriptive Evidence on Identifying Assumptions

Our key identifying assumption is that, in the absence of SEPA enactment, the economic outcomes of SEPA counties and adjacent non-SEPA on the other side of the state border would have followed parallel trends. Although this counterfactual is unobservable, we can

provide circumstantial evidence on its validity by examining the pre-treatment characteristics and trends of the two groups.

We begin with descriptive statistics on the economic conditions in counties on either side of the border of states in the years before they adopt a SEPA. Table A.I shows that counties on either side of the border are similar in terms of the main economic outcomes used throughout the analysis. We find no economically or statistically significant pre-existing differences in measures of overall economic scale, including the natural logarithm of establishments and employees. For instance, in the year prior to SEPA adoption (Year Y-0), the average log number of establishments is 6.094 for the treated group and 6.167 for the control group. This balance extends to the firm size distribution as well; the counts of small (fewer than 50 employees), medium (50-499), and large (500+) establishments, along with their respective employee counts, are highly comparable across the border. The pre-treatment industrial composition of the two groups is also statistically indistinguishable. For example, the 'Dirty Mix'—our primary measure of industrial structure, defined as the share of establishments in polluting industries—is nearly identical at 23.7% in treated counties versus 24.9% in control counties. This similarity also holds when measuring the mix by the share of employees.

Figure 6, as well as Table A.I, more directly sheds light on the plausibility of our identifying assumptions by showing the evolution of business activity in counties on the border of states that are about to adopt SEPA and those on the other side of the state border in non-SEPA states. Figure 5 indicates is little evidence of pre-existing differential trends in establishment growth rates.

Figure A.III plots the pre-SEPA trends in other outcomes of interest used throughout the paper, including the number of small establishments, the number of employees, and the percentage of establishments in dirty industries. Across all four panels, we find little evidence of a systematic difference in trends between to-be SEPA and non-SEPA states in the four years leading up to the SEPA adoption event. These findings support our identifying

assumptions and suggest that any differential shift in the economic conditions of SEPA and non-SEPA counties following SEPA adoption can be reasonably attributed to the SEPA itself.

4 Main Results

We begin by documenting how local business activity evolves around the implementation of SEPA review requirements. Table 2 reports the comparative evolution of establishments and employment in border counties where one side of the state line becomes subject to SEPA while the adjacent county does not. The estimates reveal a pronounced and persistent divergence between these otherwise similar local economies. Within five years of SEPA enactment, the level of establishments is about 5% lower relative to the pre-adoption period. Over the longer horizon—comparing the average of the five years prior to adoption with the 20–25 year steady-state window—the difference deepens to nearly 15% ($\exp(-0.201) - 1$). These magnitudes indicate that SEPA's procedural review requirements impose economically meaningful frictions that hamper local business formation in directly affected counties.

Figure 6 presents the dynamic evolution of these patterns in an event-study framework. Both groups of border counties follow nearly identical trends in the years prior to SEPA adoption, consistent with the high degree of economic integration typically found among adjacent counties straddling a state border. After enactment, however, the number of establishments on the SEPA side declines sharply and persistently. The gap widens over the subsequent decade and remains large even twenty years after adoption. This time path aligns with the mechanisms outlined in Section 2: SEPA's review requirements deter marginal or time-sensitive projects in the short run, and the resulting contraction in project pipelines gradually weakens the agglomeration structures—local suppliers, contractors, specialized services, and labor-market connections—that ordinarily support business activity in border regions. The lack of a corresponding increase in establishment activity on the non-SEPA side of the border suggests that relocation across jurisdictions is limited; instead, the regulation

primarily disrupts the local growth processes on the SEPA side.

The agglomeration channel is examined more directly in Table 3, which investigates whether SEPA's effects are amplified when a treated county relies economically on a nearby metropolitan area located across the border in a non-SEPA state. The results show that declines in establishment activity are substantially larger when the SEPA county is not part of a major metropolitan area, and they are even more pronounced when the adjacent non-SEPA county *is* a major metro area. These patterns indicate that SEPA disproportionately harms counties with weaker internal agglomeration forces and stronger cross-border dependencies. When a non-metropolitan SEPA county relies on a neighboring metro for suppliers, customers, or labor flows, the introduction of SEPA frictions undermines those cross-border linkages, producing a larger contraction than in more self-sufficient urban counties. This evidence reinforces the conclusion that SEPA's economic footprint reflects the weakening of localized agglomeration economies rather than large-scale movement of firms to unregulated jurisdictions.

Employment patterns closely mirror the establishment results. As shown in Columns 3–4 of Table 2, total employment on the SEPA side is roughly 4% lower within the first five years after adoption and nearly 11% lower two decades later ($\exp(-0.111) - 1$). The similarity in magnitude between the establishment and employment responses indicates that these changes represent genuine contractions in local economic activity rather than compositional shifts in reporting or firm size. Together, these patterns suggest that SEPA's procedural requirements not only suppress new business formation but also erode the scale economies that sustain existing employers in border regions.

Taken together, Table 2 to 4, and Figure 6, provide consistent evidence that SEPA adoption induces sizable and persistent reductions in business activity concentrated on the regulated side of state borders. The absence of compensating gains in neighboring non-SEPA counties points to localized project deterrence and weakened agglomeration linkages, rather

than firm relocation or cyclical reallocation, as the dominant mechanisms. In the next section, we examine where these effects are most pronounced across industries, firm sizes, and local economic structures.

4.1 Just a Border Effect?

A natural question is whether the contraction in local business activity following SEPA adoption is confined to the immediate border region or diffuses more broadly within the state. This distinction is central to interpreting the operative mechanism: whether the observed declines reflect firms relocating across the state line to escape SEPA review, or whether they represent localized disruptions to the agglomeration forces that ordinarily sustain economic activity in border-adjacent areas.

Table 4 provides evidence on this spatial gradient by comparing counties one layer inside SEPA states with their interior counterparts in non-SEPA states, as well as by contrasting border and interior counties within each side of the border. Columns (1)–(2) show that interior counties in SEPA states exhibit no differences relative to interior counties in non-SEPA states. Columns (3)–(4), however, reveal that SEPA border counties underperform their own in-state interior neighbors, while Columns (5)–(6) show that non-SEPA border counties do not experience any corresponding gains relative to non-SEPA interior counties. In combination, these patterns indicate that the contraction induced by SEPA is geographically concentrated near the regulatory discontinuity and does not extend uniformly across the state.

The localization of these effects aligns closely with the framework outlined in Section 2. When one side of a shared local economy becomes subject to more stringent and uncertain project-review procedures, marginal investments are deterred, and the network of suppliers, service providers, and workers that depends on those projects begins to thin out. Because these agglomeration forces operate at close geographic range—through labor-market pooling,

customer–supplier proximity, and knowledge spillovers—their erosion remains confined to the counties directly exposed to SEPA review rather than propagating broadly across the state. A reduction in project activity on the SEPA side therefore lowers the "external returns to scale" in that area without generating an offsetting increase just across the border.

The broader spatial and labor-market literatures offer additional support for this pattern. Work on local multipliers shows that job creation in tradable sectors generates secondary employment in non-tradables within the same commuting zone (Moretti, 2010); when SEPA frictions suppress tradable-sector investment, these induced jobs never materialize, producing a wider local slowdown. Similarly, shocks propagate imperfectly across regional networks: adjustments in commuting flows, supplier relationships, and factor allocations are gradual and capacity constrained (Monte et al., 2018; Acemoglu et al., 2012). As a result, nearby non-SEPA counties do not experience a compensating boom, and the shock remains spatially asymmetric.

Table A.II reinforces this interpretation by showing a similar attenuation pattern for employment outcomes. The largest and most persistent employment declines are concentrated in SEPA border counties, while interior counties on both sides exhibit no effects. The absence of gains in non-SEPA border counties further rules out cross-border relocation as the primary driver. Instead, SEPA appears to disrupt the localized business ecosystems that tie together firms and workers in border regions—an effect that can be described as a "suffocated-agglomeration" response.

These findings carry clear policy implications. Border regions—where firms can most readily compare regulatory environments and where cross-state supply chains are most tightly woven—bear the brunt of SEPA's procedural frictions. These counties face the dual burden of direct compliance and uncertainty costs, along with the indirect erosion of local agglomeration advantages. Policymakers concerned about maintaining economic vitality in border communities may therefore consider harmonizing permitting standards,

improving inter-state coordination, or providing transitional support to firms most exposed to regulatory-induced spatial frictions.

4.2 Heterogeneity Across States

The patterns in Table 2 mask substantial variation across SEPA-adopting states. Figure 7 decomposes the main border estimates by state and shows that nearly all SEPA states experience relative declines in establishment activity, but the magnitudes vary considerably. States such as California, Washington, and Minnesota exhibit large and persistent contractions, while others, including Virginia and New Jersey, display little measurable change. This heterogeneity indicates that the economic consequences of SEPA adoption are not uniform, but instead depend on institutional and legal characteristics that shape how burdensome the environmental review process is in practice.

Two features of SEPA implementation are particularly informative. First, SEPAs differ in their *substantive authority*. Some states, most notably California under CEQA, allow agencies to condition or even deny projects on environmental grounds. Other SEPAs are purely procedural and require disclosure but not conditioning or denial. Second, SEPA states vary dramatically in their underlying administrative capacity. A state with a historically large environmental bureaucracy—staff, technical expertise, legal resources—can conduct more rigorous and time-consuming reviews than one with a thin administrative apparatus. Both factors—legal authority and bureaucratic capacity—shape how binding SEPA requirements are for private investment.

Table 5 formalizes these ideas by interacting SEPA exposure with two measures of institutional strength. The first is a *substantive-provision* dummy equal to one for states whose SEPA statutes authorize agencies to deny or substantially condition projects. The second is the *log difference* in 1970 environmental and natural-resource spending between the SEPA county and its cross-border non-SEPA counterpart. This measure captures the

relative strength of each county's regulatory apparatus *prior* to SEPA adoption and reflects both political commitment and administrative capacity.

The results show that both sources of heterogeneity matter. Counties in substantive-provision SEPA states experience significantly larger long-run declines in establishments, confirming that regulatory "teeth" amplify the procedural deterrence channel. Similarly, the negative and significant coefficient on the triple interaction with the 1970 spending difference indicates that SEPA effects are stronger where the SEPA county had a more robust environmental bureaucracy relative to its neighbor. Quantitatively, a larger historical spending advantage on the SEPA side predicts a substantially deeper post-adoption contraction in local business activity. These patterns are consistent with the idea that SEPA's economic bite scales with the rigor, enforceability, and administrative depth of the review process.

Together, Figure 7 and Table 5 demonstrate that SEPA's impact depends not merely on the existence of an environmental review statute, but on *how powerful and well-resourced* that statute is within a given state. SEPA adoption imposes the largest economic burdens where regulators possess both the legal authority and institutional capacity to conduct intensive reviews. These same institutional strengths are also those most likely to generate long-run environmental and amenity gains, highlighting the dual-edged nature of procedural environmental governance.

5 Most Affected Firm Types

The previous sections show that SEPA adoption is followed by a sizable and persistent contraction in local business activity, concentrated near the regulatory boundary. We now turn to identifying which types of firms and industries account for these aggregate declines. The objective is not simply descriptive heterogeneity, but to illuminate the mechanisms described in Section 2: how fixed compliance costs, agglomeration linkages, and long-run

amenity forces interact to shape the distributional footprint of SEPA across the local economy.

Our analysis proceeds along two complementary dimensions: *industry type* and *firm size*. Variation across industries reveals how SEPA reshapes the sectoral composition of local economies by discouraging activity in pollution-intensive industries and, over time, reallocating demand toward cleaner or consumer-facing sectors. Variation across firm size captures how SEPA's procedural frictions differentially affect establishments with varying capacity to absorb regulatory delays, documentation requirements, and compliance uncertainty.

Section 5.1 examines heterogeneity by industry. Using Figure 8 and Tables 6 and A.III, we show that the immediate economic burden of SEPA falls disproportionately on "dirty" industries—manufacturing, construction, mining, and other pollution-intensive sectors—where the fixed-cost and timing components of environmental review are most binding. Cleaner sectors, particularly retail and restaurants, display relative resilience and even long-run expansion consistent with an amenity-driven reallocation of local economic activity.

Section 5.2 investigates heterogeneity by establishment size. Drawing on Tables 7 and 8, we find that the decline in business activity is overwhelmingly concentrated among small, which lack the scale, in-house expertise, or administrative capacity to navigate lengthy and uncertain environmental review processes. Large establishments, in contrast, are largely insulated. The contraction is especially pronounced among small firms in dirty industries, linking the size and industry channels into a unified fixed-cost mechanism.

Taken together, these results reveal that SEPA-induced restructuring is highly uneven. Rather than generating uniform declines across the local economy, SEPA primarily deters activity among small, pollution-intensive firms and gradually shifts the economic base toward larger, cleaner, and more service-oriented establishments. This pattern underscores that SEPA's economic impact operates through compositional transformation—mediated by firm characteristics and local agglomeration structures—rather than through a proportional contraction across all firms and industries.

5.1 Disproportionate Effect on Dirty Industries

We begin our heterogeneity analysis by examining how SEPA affects different industries, focusing on which sectors bear the brunt of procedural and compliance costs. Figure 8 provides an initial visualization by plotting relative changes in establishment counts for three broad industry cohorts: dirty industries, retail and restaurants, and other clean sectors. The divergence across sectors is pronounced. Establishments in dirty industries experience an immediate and sharp decline after SEPA adoption, falling by more than 15% within five years. While the decline in other clean industries is statistically insignificant, the retail and restaurant establishments stay relatively at pre-SEPA level. These patterns indicate that the short-run burden of SEPA review falls disproportionately on pollution-intensive sectors, whereas consumer-oriented sectors are comparatively insulated.

Table 6 formalizes these compositional adjustments using the "dirty-industry mix"—the share of establishments in pollution-intensive industries. Columns (5)–(6) show that the dirty-establishment share falls by roughly 0.9 percentage points in the short run and by about 1.1 percentage point over the long run. Columns (1)–(4) examines the establishments in dirty and clean industries separately, and finds that after five years from SEPA adoption, the drop in establishments in dirty industries are about three times higher than the drop in clean industries. Because specifications include county and county-pair-by-year fixed effects, these estimates reflect divergences within matched border regions rather than differences in long-standing industrial composition.

Although dirty sectors contract sharply, other parts of the local economy expand. Table A.III examines the share of establishments and employment in retail and restaurant sectors—proxies for local, non-tradable activity. Across both short- and long-run specifications, the retail share rises meaningfully. Within five years of SEPA enactment, the retail-establishment share increases by roughly 1.5 percentage points; twenty years after adoption, the increase reaches 2.3 points. These increases exceed what would be expected

mechanically from dirty-sector contraction alone and instead point to an amenity-driven channel. As environmental quality improves or is perceived to improve, households and consumers who value cleaner environments generate additional demand for local services. This mechanism is consistent with spatial-equilibrium models in which amenities are capitalized into local economic activity (Roback, 1982) and with empirical evidence linking improved air quality to higher housing values and inflows of skilled workers (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). Recent studies showing the opposite effect—declines in retail activity after negative environmental shocks such as wildfire smoke (Addoum et al., 2023)—further reinforce this interpretation.

Taken together, Figure 8 and Tables 6–A.III paint a coherent picture of SEPA-induced structural change. In the short run, SEPA imposes procedural and timing costs that deter investment in pollution-intensive sectors, particularly in border-adjacent regions where firms are most sensitive to regulatory differences. As dirty sectors contract, local agglomeration linkages weaken, deepening the downturn. Over the long run, however, cleaner amenities stimulate growth in local service sectors, gradually shifting the industrial base toward less polluting and more household-oriented activities. The overall pattern is one of *reallocation rather than uniform decline*: SEPA compresses dirty-sector activity while helping to reconfigure local economies toward cleaner, service-intensive composition.

5.2 Disproportionate Effect on Small Firms

We next examine heterogeneity by establishment size to identify which firms absorb the procedural and compliance burdens introduced by SEPA review. As described in Section 3, environmental permitting requirements impose largely fixed costs—fees for environmental assessments, legal review, and documentation, as well as the indirect costs of delays and regulatory uncertainty. These costs scale weakly with project size but strongly with administrative complexity, making them disproportionately burdensome for small-sized enterprises

(SMEs). Smaller firms are also more credit constrained and less able to absorb timing risk, whereas large firms can spread compliance costs across multiple projects and may even gain market share as smaller rivals exit.

Table 7 summarizes how establishment counts evolve by firm size over the long run, comparing the five years preceding SEPA adoption to the 20–25 years following it. The results are striking: the decline only exists among small establishments (fewer than 50 employees), while medium-sized establishments (50–499 employees), and large establishments (500 or more employees) experience no declines. The coefficient of -0.168 for small firms corresponds to an 15.5% decline ($\exp(-0.168) - 1$) in their number. The coefficients for medium and large establishments are near zero and statistically insignificant. These magnitudes remain stable across alternative long-run windows such as (+15, +20). The size-specific decline is exactly what the fixed-cost deterrence mechanism predicts: SEPA's procedural and timing requirements selectively discourage investment by marginal, resource-constrained firms.

This asymmetric response reshapes the size distribution of local business activity. Smaller firms face the largest proportional shock from the added compliance costs and delays associated with SEPA review, while larger firms—often equipped with in-house permitting expertise or established legal teams—internalize or outsource these requirements more efficiently. Over time, these frictions skew local industrial structure toward larger incumbents and away from small, entrepreneurial entrants that typically drive local job creation and innovation. In agglomeration terms, the loss of small establishments reduces local supplier diversity and weakens downstream linkages, amplifying the initial deterrence effect through local network contraction.

To connect these size patterns with industrial composition, Table 8 decomposes changes in the dirty-industry mix by establishment size. The results again tell the same story: virtually the entire reduction in the dirty-industry share arises from small firms. Interestingly, Column (2) shows that dirty-mix of medium sized firms actually goes up, probably because of the

consolidation of the small dirty firms. Column (3) show negligible effects for large firms. Columns (4)–(6) replicate this pattern using dirty-employment shares: only small firms exhibit a significant decline (-0.015), while medium and large establishments remain largely unaffected. These parallel findings across establishment and employment measures underscore that SEPA's economic bite falls overwhelmingly on small, pollution-intensive firms.

Conceptually, these results illustrate how SEPA's primary mechanism—fixed-cost deterrence—interacts with agglomeration and industry channels described in Section 2. Procedural frictions raise the effective threshold for project viability, pushing small dirty firms below profitability and prompting exits that erode local supplier networks. Larger dirty firms, in contrast, maintain operations, consolidating market power and potentially benefiting from reduced competition. The resulting post-SEPA landscape is cleaner but also more concentrated, with fewer establishments overall but surviving firms that are larger and more capital intensive.

Taken together, Tables 7 and 8 highlight a sharp asymmetry in how SEPA reshapes local economies. The procedural costs of environmental review fall most heavily on small dirty firms, which face both financial and informational barriers to compliance. Larger firms absorb or adapt to the new requirements, leaving a post-SEPA economy characterized by fewer, larger, and cleaner establishments. This structural transformation underscores a key trade-off embedded in procedural environmental regulation: while SEPAs may promote long-run environmental and amenity gains, they do so at the cost of small-firm dynamism and local entrepreneurial depth.

6 Conclusion

The passage of state environmental review laws materially reshape local economic geography at the margins where policy regimes meet, both immediately and over several

decades. Exploiting staggered adoption of State Environmental Protection Acts (SEPAs) in a stacked border-pair event design and a long county-level panel, we show that counties on the SEPA side of state borders end up persistently smaller than otherwise similar neighbors just across the line. We find no significant gains on the non-SEPA side of the border and no broad underperformance in the interior of SEPA-adopting states. These localized losses are larger when SEPA border counties border major metropolitan areas and where states devote more resources to environmental enforcement, and they fall disproportionately on pollution-intensive sectors and small establishments. Taken together, the evidence suggests that local environmental policies can sever local agglomerations to the detriment of small business growth.

Our findings imply that procedural environmental review at the state level has significant, but uneven, effects on the local business environment. By design, our estimates are local to border economies and we do not attempt a full welfare evaluation, but our setting sheds new light on how embedding environmental review in routine state and local decisions can adversely affect cross-border agglomerations and tilt the business landscape away from small, pollution-intensive firms. Future work could link our design to data on emissions, air quality, property values, and migration to quantify environmental and amenity gains, or embed SEPA-style review in structural spatial models to map these local shocks into general-equilibrium outcomes. More broadly, the analysis underscores that the real effects of regulation depend not only on the content of the regulation but also on the geographic scale of the policy variation. For instance, in the case of environmental review our findings highlight the substantial economic impact of reviews passed at the state level, but suggest a more muted impact if the same review requirements were national in scope.

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US States that Enacted State Environmental Policy Act (SEPA)

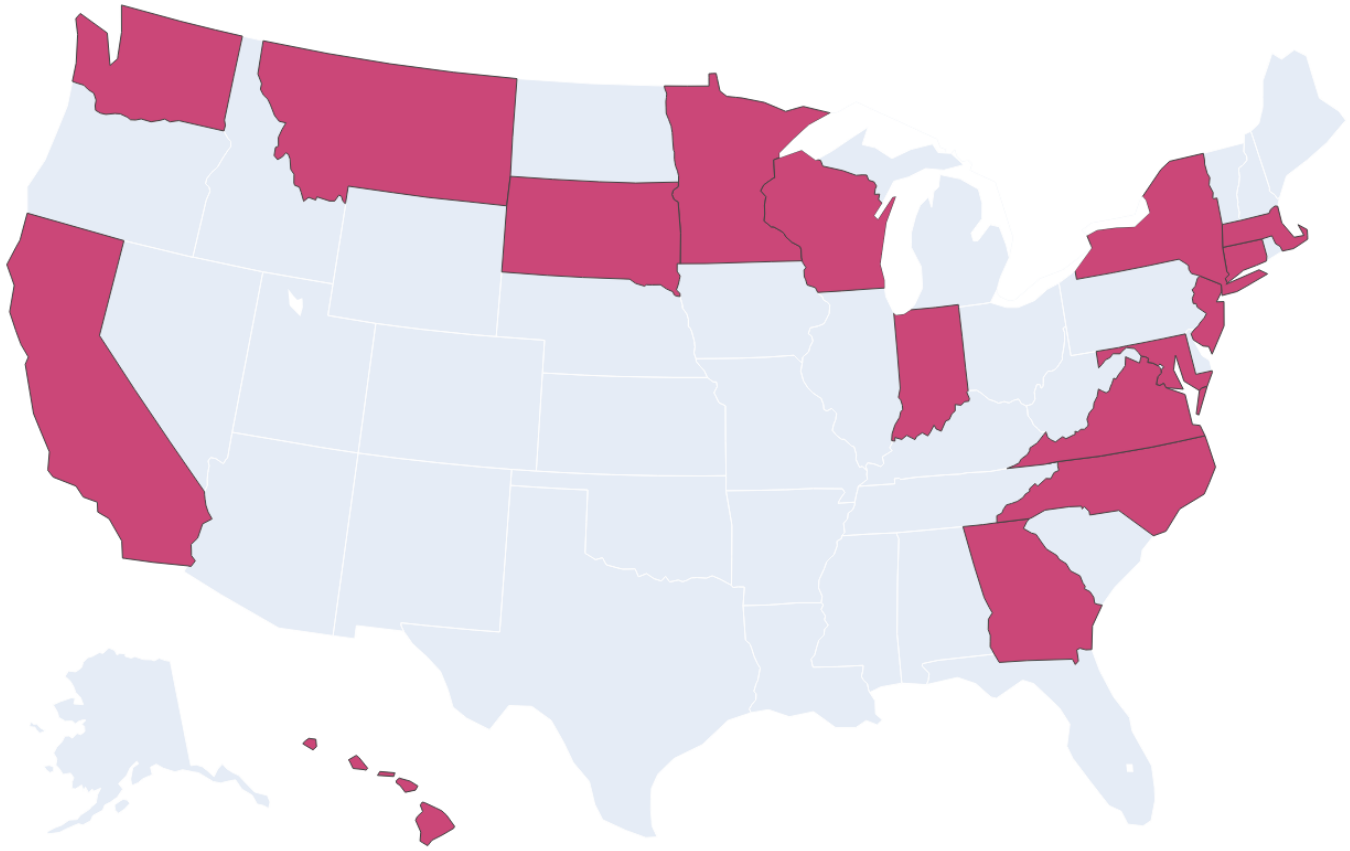


Figure 1: US States that Enacted State Environmental Policy Acts (SEPAs). This figure shows the sixteen U.S. states that have passed a SEPA. These states, with their year of enactment in parentheses, are: California (1970), Connecticut (1973), Georgia (1991), Hawaii (1974), Indiana (1972), Maryland (1973), Massachusetts (1977), Minnesota (1973), Montana (1971), New Jersey (1989), New York (1978), North Carolina (1971), South Dakota (1974), Virginia (1973), Washington (1971), and Wisconsin (1974). Our empirical analysis utilizes 15 of these 16 states; Hawaii is excluded because our border-pair research design requires adjacent U.S.-based control counties, which Hawaii lacks.

California SEPA (CEQA) Process Flowchart (Part 1): Initial Review and Streamlined Paths

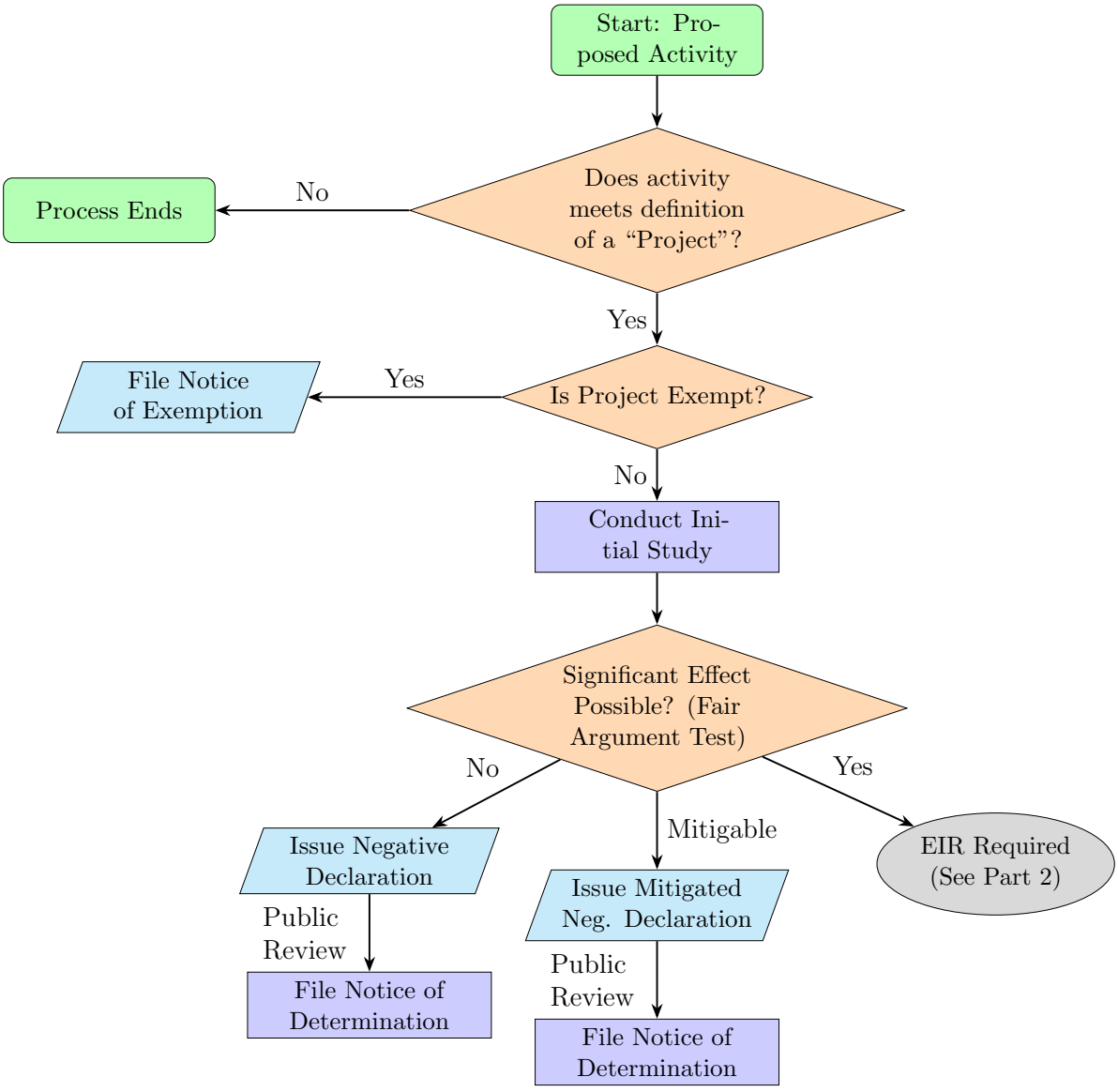


Figure 2: California SEPA (CEQA) Process Flowchart (Part 1): Initial Screening and Streamlined Paths. Taking California's SEPA as an example, this flowchart details the initial screening process for a proposed project. An activity is first evaluated to determine if it meets the legal definition of a "Project" and if it qualifies for a statutory or categorical exemption, both of which would end the review. If it proceeds, the lead agency conducts an Initial Study to determine whether the project may have a significant environmental effect. Based on this determination, the project can be approved via a streamlined path with a Negative Declaration (or Mitigated Negative Declaration), or it must proceed to the more comprehensive Environmental Impact Report (EIR) process detailed in the next figure.

California SEPA (CEQA) Process Flowchart (Part 2): The Environmental Impact Report (EIR) Track

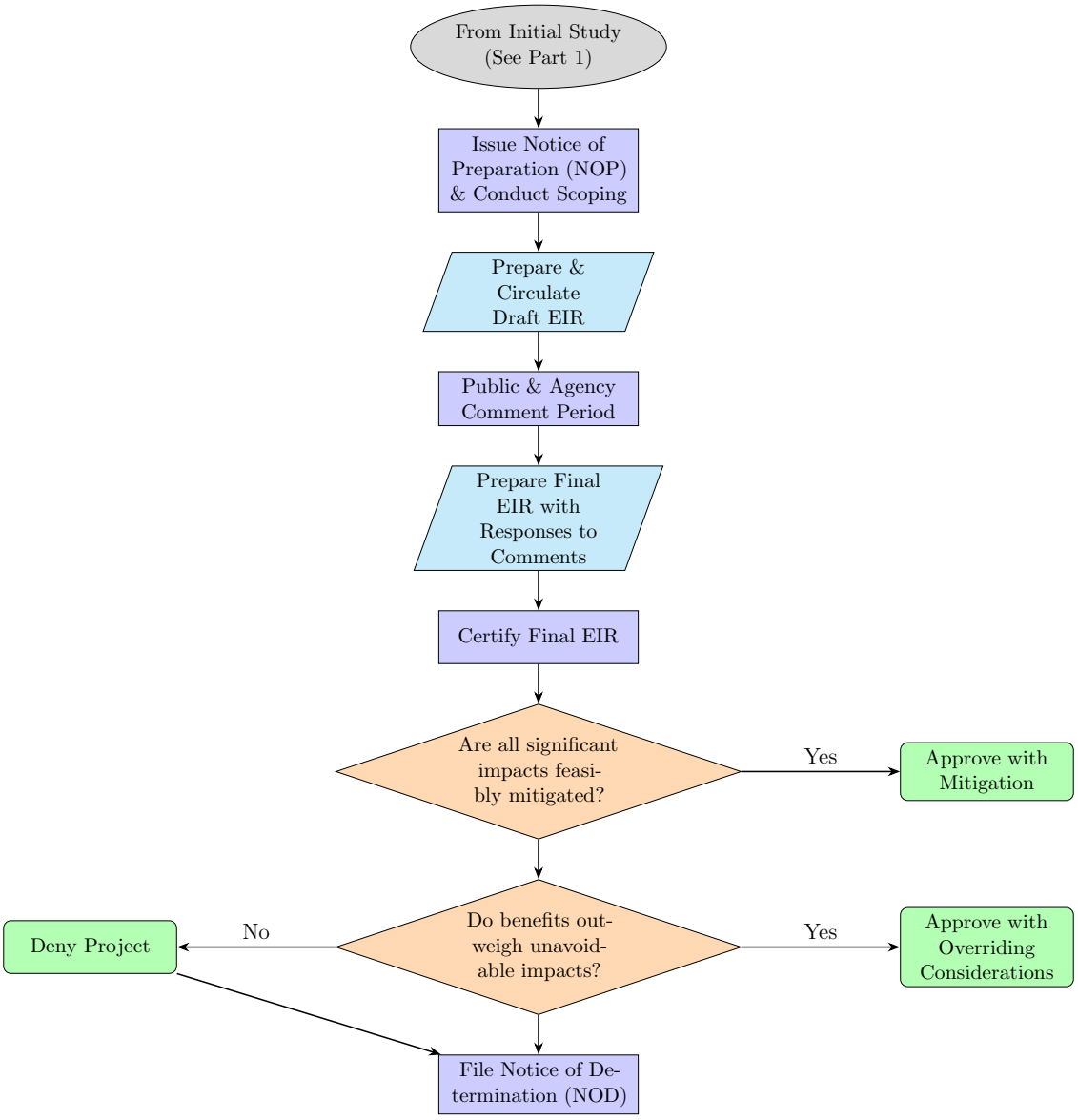


Figure 3: California SEPA (CEQA) Process Flowchart (Part 2): The Environmental Impact Report (EIR) Track. Taking California's SEPA as an example, this flowchart details the comprehensive EIR process that is required once a project is determined to have a potentially significant environmental effect. This legally intensive path includes mandatory public scoping, the preparation and circulation of a Draft EIR for public comment, and the preparation of a Final EIR with formal responses to all substantive comments. Critically, the process concludes with a substantive agency decision, where the agency has the authority to approve the project (either with full mitigation or with a "Statement of Overriding Considerations"), or to deny the project altogether on environmental grounds.

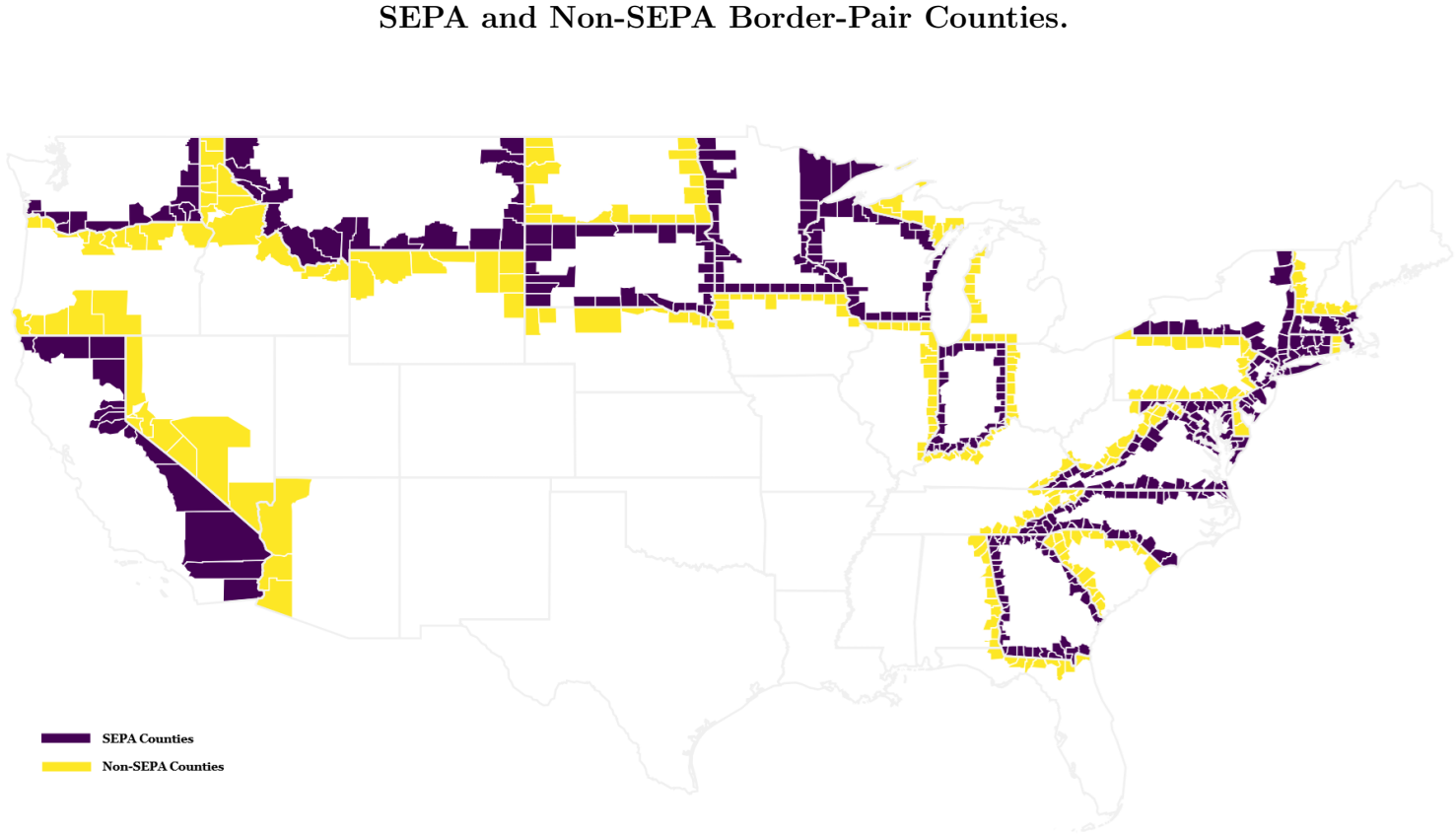
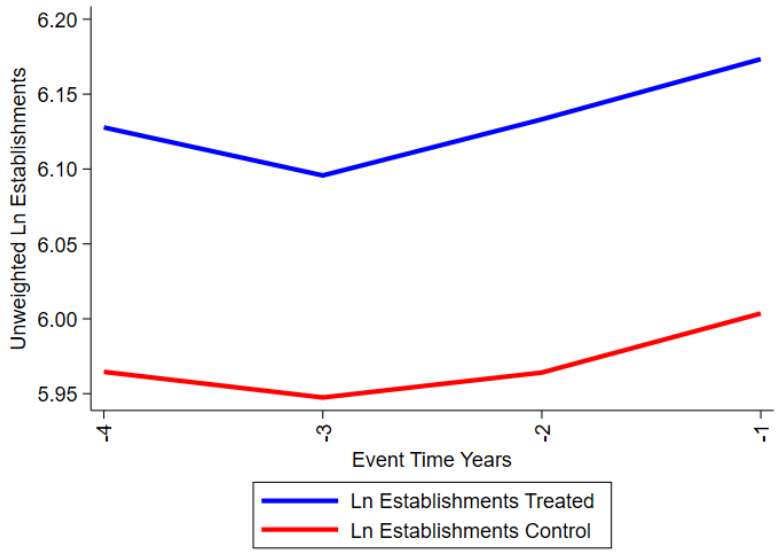
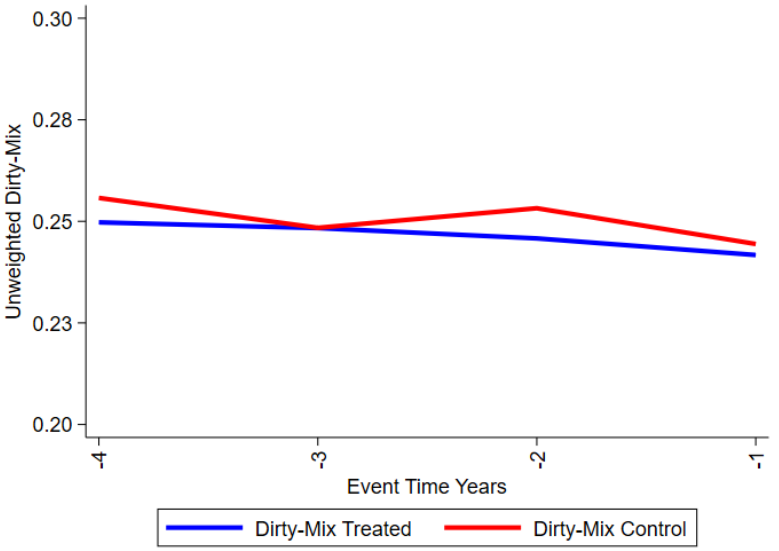


Figure 4: SEPA and Non-SEPA Border-Pair Counties. This figure illustrates our border pair matching strategy. The sample consists of all counties in a SEPA-adopting state that share a border with a county in a non-SEPA state (shown in purple), and those corresponding non-SEPA border counties (shown in yellow). The matching algorithm is many-to-many. For example, the treated county of San Bernardino, CA, is paired with three distinct control counties: La Paz, AZ; Mohave, AZ; and Clark, NV. Conversely, a single control county, such as Klamath, OR, can be paired with multiple treated counties (in this case, Siskiyou, CA and Modoc, CA). While the initial matching is many-to-many, our final sample is constructed from unique county pairs, where each pair consists of one treated (SEPA) county and one adjacent control (non-SEPA) county.



(a) Unweighted Log of Total Establishments



(b) Unweighted Dirty-Mix of Establishments

Figure 5: **Pre-Trends in Establishment Counts and thiere Dirty-Mix Prior to SEPA Adoption.** This figure plots the unweighted average log number of total establishments for treated (SEPA) and control (non-SEPA) border counties over the four years preceding SEPA adoption in Panel (a) and Dirty-Mix of Establishments in Panel (b). Dirty-Mix is defined as total establishments in the dirty industries divided by the total establishments for a given county for a given year. Dirty industries are all industries with two-digit SIC codes below fifty. The blue line represents SEPA (treated) counties and the red line represents non-SEPA (control) counties. The two series move in parallel throughout the pre-adoption period, indicating that treated and control counties exhibit similar baseline trends in establishment growth as well as their dirty-mix prior to SEPA implementation.

Natural Logarithm of Establishments in All Industries Pre and Post SEPA Enactments

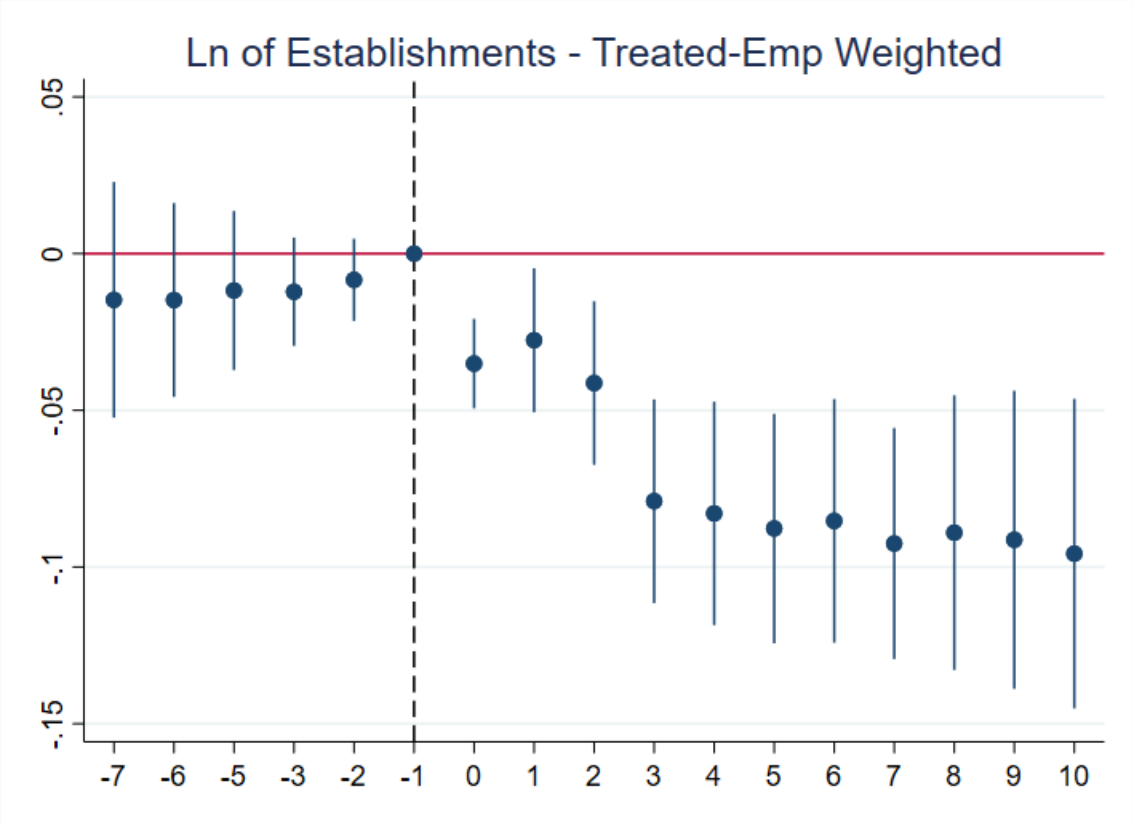


Figure 6: Event-Study Analysis of SEPA Enactment on the Number of Establishments. The figure shows the estimated year-by-year effect of a SEPA enactment on the log number of business establishments in treated border counties relative to their control-county neighbors. The model includes county-pair-by-year fixed effects. The plot demonstrates the absence of pre-trends in the years prior to the event. In the post-enactment period, there is a sharp and statistically significant decline, with the effect ranging between -5% and -10% in years 1 through 4 and exceeding -10% by year 5. Vertical bars represent 95% confidence intervals.

Estimation of Our Main Specification by States

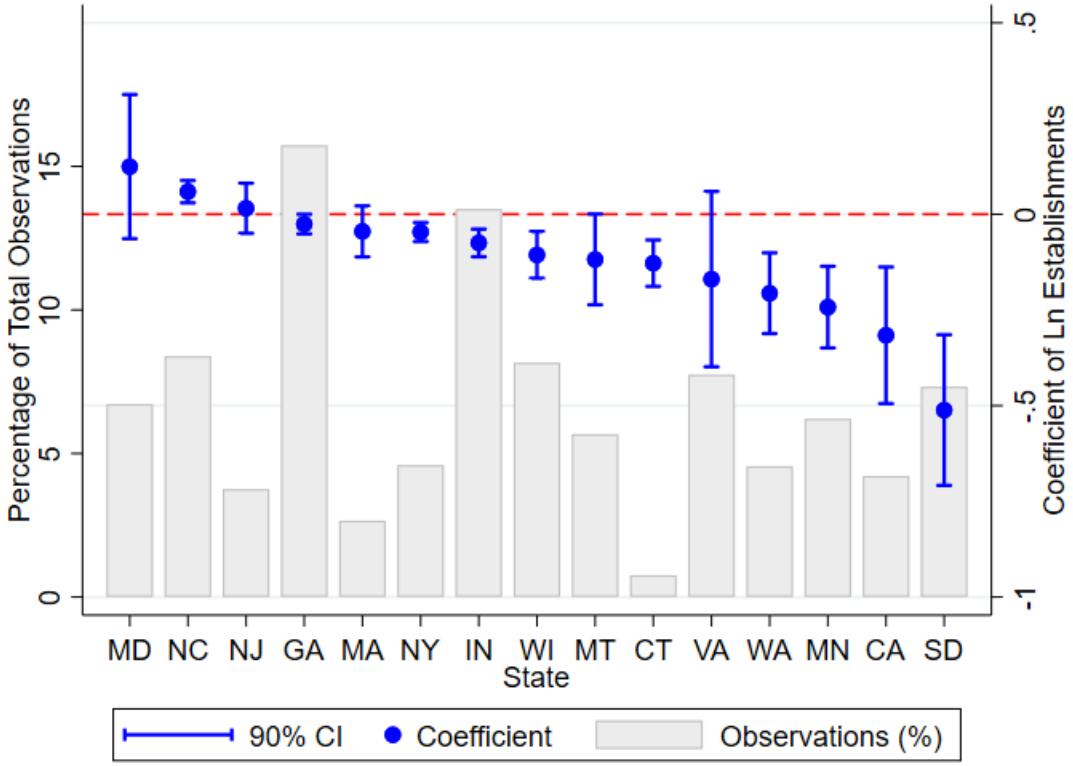


Figure 7: Heterogeneity in the Short-Run Impact of SEPA Adoption by State. This figure disaggregates the average treatment effect by plotting the state-specific DiD coefficient for the log number of establishments from our short-run (-5, +5 year) specification. The blue dots show the point estimate for each state, with the red dashed line representing a null effect. The results reveal significant heterogeneity in the policy's impact. Consistent with a strong regulatory burden, states with notably substantive SEPA statutes such as California (CA), Washington (WA), and Minnesota (MN) exhibit large and statistically significant negative effects on business formation. In contrast, the effect is statistically insignificant in several states with more procedural statutes, such as Virginia (VA) and New Jersey (NJ). The gray bars provide context by showing each state's share of total sample observations, illustrating the relative contribution of each state's border pairs to our pooled estimate.

Natural Logarithm of Establishments in Dirty, Retail, and Clean excluding Retail Industries Pre and Post SEPA Enactments

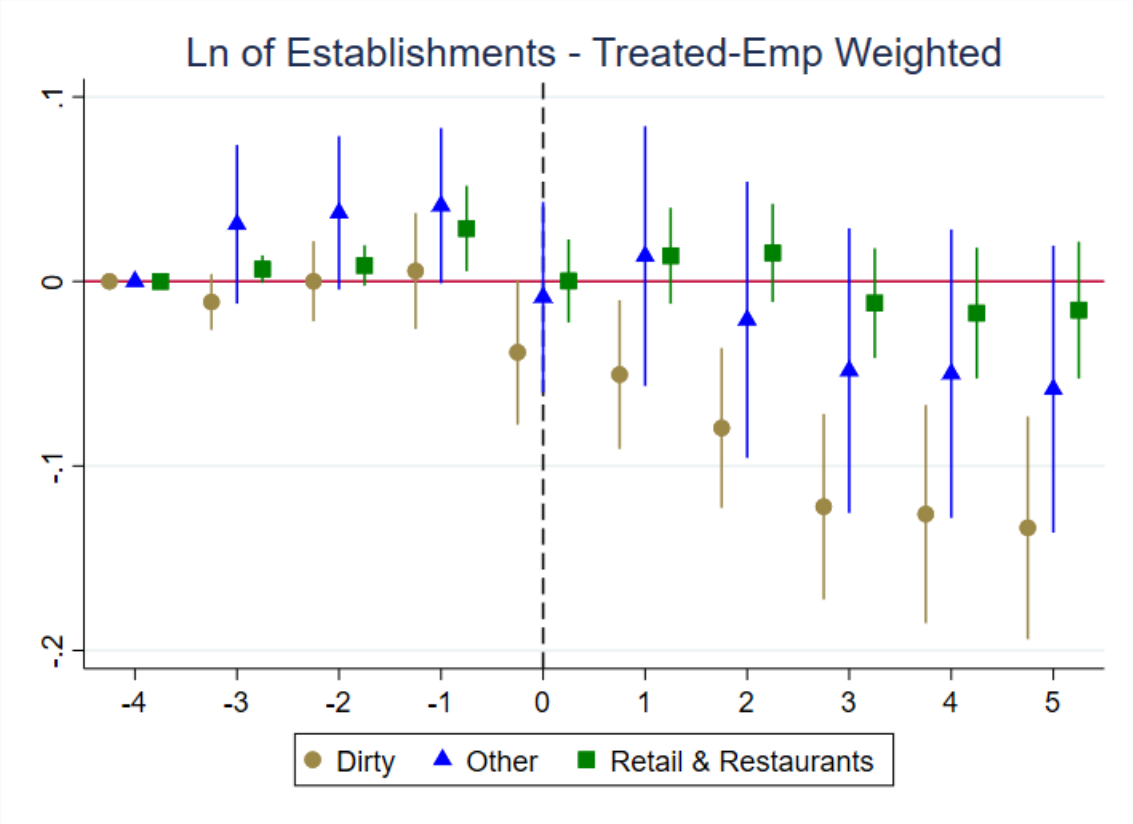


Figure 8: Heterogeneous SEPA Effects: Event Study by Industry Cohort. This figure disaggregates the main treatment effect by plotting the dynamic DiD coefficients for three industry cohorts: 'Dirty,' 'Retail & Restaurants,' and 'Other' (all non-dirty, non-retail firms). The results show a clear ordering of impacts. The 'Dirty' industry cohort has the strongest and most immediate response, declining by close to 15% within five years of SEPA enactment. In contrast, the 'Retail & Restaurants' cohort, composed largely of small service-oriented enterprises, exhibits almost no response. Other industries consisiting clean industries excluding retail show about 5% decline in five years but it is not statistically significant even at 10% level. All models include county-pair-by-year and county fixed effects. Vertical bars represent 90% confidence intervals.

Table 1: **Summary Statistics.**
This table reports summary statistics for the variables used in the analysis. *Total Establishments* and *Employees* denote the total number of establishments and employees, respectively, in the CBP data for a given county-year. *Dirty* and *Clean Establishments* represent establishments in industries classified as dirty (two-digit SIC codes < 50) and clean (SIC codes ≥ 50), respectively. *Small*, *Medium*, and *Large Establishments* refer to establishments with fewer than 50, between 50 and 499, and 500 or more employees, respectively. *Dirty* and *Clean Employees* are defined analogously based on industry classification. *Dirty Mix* is the share of establishments in dirty industries, calculated as the number of dirty establishments divided by total establishments for each county-year. The sample period spans 1964–2021, and the statistics reported here represent a snapshot at $T-1$, the year immediately preceding SEPA adoption. Panel A reports statistics for treated (SEPA-side) counties, and Panel B reports statistics for control (non-SEPA-side) counties. Variable definitions are provided in Appendix A.

	Treated Sample						Control Sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	N	mean	SD	P5	P50	P95	N	mean	SD	P5	P50	P95
Total Establishments	262	1,293	2,643	32	376	5,983	248	1,500	5,702	17	327.5	5,217
Total Employees	262	20,827	46,887	313	5,238	112,855	248	27,108	126,816	274	4,460	93,033
Dirty Mix	262	0.226	0.110	0	0.227	0.377	248	0.240	0.139	0	0.231	0.415
Small Establishments	262	1,226	2,502	27	360	5,634	248	1,416	5,332	15	321.5	4,855
Medium Establishments	262	61.86	136.8	1	14	325	248	77.99	342.0	0	13.50	294
Large Establishments	262	4.431	10.85	0	1	23	248	6.101	30.18	0	1	21
Dirty Establishments	262	311.9	674.3	0	88.50	1,387	248	351.1	1,337	0	82.50	1,255
Clean Establishments	262	980.8	1,977	26	285	4,643	248	1,149	4,383	14	249.5	3,922
Dirty Employees	262	9,702	22,186	0	2,692	42,965	248	12,866	62,745	0	2,311	48,545
Clean Employees	262	11,125	25,897	180	2,233	60,318	248	14,242	65,791	66	1,787	52,557

Table 2: **Effect of SEPA Enactments on Local Business Activity.**
This table reports county-level regressions of local business activity on SEPA adoption indicators. Columns (1)–(3) use the logarithm or annual percentage change in the number of establishments as the dependent variable, while Columns (4)–(6) use the corresponding measures of total employment. Panel A presents estimates for border counties located on either side of SEPA–non-SEPA state borders, and Panel B reports results for interior counties one layer inside each state. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Dependent Variable	Ln Estabs	Ln Estabs	Ln Emp	Ln Emp
Post x Treated	-0.050***	-0.163***	-0.039**	-0.111*
	(-2.755)	(-3.342)	(-2.144)	(-1.910)
CntyPair#Year FE	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES
Observations	10,538	12,926	10,538	12,926

Table 3: **SEPA Enactment and the Weakening of Agglomeration Economies**
This table reports county-level regressions examining how dependence on cross-border metropolitan areas influences the local economic impact of SEPA adoption. Dependent variable is natural logarithm of total establishments. *Non Metro Treated* equals one if the treatment (SEPA) county is not classified as part of a large metropolitan area under the 1974 Rural–Urban Influence Codes published by the U.S. Department of Agriculture. *Metro Control Side Only* equals one when the control county belongs to a large metropolitan area but the corresponding treated (SEPA) county does not, indicating that the treated county's local economy relies on access to the adjacent metropolitan market across the state border. A negative coefficient on this interaction term indicates that SEPA adoption has stronger adverse effects where treated counties depend economically on neighboring metro areas. The sample spans 1964–2021, excluding 1974 due to changes in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. Standard errors are clustered at the county-pair level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	Ln Establishments			
	(1)	(2)	(3)	(4)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated x Non Metro Treated	-0.241***	-0.603***		
	(-5.280)	(-6.288)		
Post x Treated x Metro Control Side Only			-0.176***	-0.475***
			(-4.090)	(-4.677)
Post x Treated	0.159***	0.364***	-0.057***	-0.181***
	(3.723)	(4.203)	(-3.860)	(-4.581)
CntyPair#Year FE	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES
Observations	10,538	12,926	10,538	12,926

Table 4: **Effect of SEPA Enactments on Local Business Activity.**
This table reports county-level regressions of local business activity on SEPA adoption indicators. Columns (1)–(3) use the logarithm or annual percentage change in the number of establishments as the dependent variable, while Columns (4)–(6) use the corresponding measures of total employment. Panel A presents estimates for border counties located on either side of SEPA–non-SEPA state borders, and Panel B reports results for interior counties one layer inside each state. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(SEPA vs. Non-SEPA) Inner		(SEPA) Boarder vs. Inner		(Non-SEPA) Boarder vs. Inner	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	-0.038	-0.041	-0.061**	-0.235**	-0.023	-0.033
	(-0.363)	(-0.175)	(-2.124)	(-2.582)	(-1.484)	(-0.672)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	Yes	YES	YES	YES	YES	YES
Observations	15,757	19,555	8,552	9,450	8,976	9,942

Table 5: **Effect of SEPA Enactments on Establishments Interacted with SEPA Stiffness**

This table reports county-level regressions of local business activity on SEPA adoption indicators interacted with a measure of SEPA enforcement capacity. Columns (1)–(3) use the logarithm or annual percentage change in the number of establishments as the dependent variable, while Columns (4)–(6) use the corresponding measures of total employment. The first interaction term, *Post* × *Treated* × *Substantive Provision*, captures the SEPAs with authority to deny the project, or the substantive provisions. It is a dummy variable that is equal to 1 for states with substantive provisions in their SEPA. The second interaction term *Post* × *Treated* × *Environmental Spending*, captures the differential impact of SEPA adoption in states with higher baseline per-capita environmental and natural-resource spending in 1970, which proxies for regulatory enforcement capacity. Environmenal Spending is the log of difference in environmental spending by the treatement county versus the control county. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Dependent Variable	Ln Estabs	Ln Estabs	Ln Estabs	Ln Estabs
Post x Treated x Substantive Provision	-0.119***	-0.274**		
	(-2.706)	(-2.547)		
Post x Treated x Environmental Spending			-0.065***	-0.180***
			(-4.880)	(-5.396)
Post x Treated	-0.036*	-0.128**	-0.008	-0.021
	(-1.931)	(-2.375)	(-0.473)	(-0.529)
CntyPair#Year FE	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES
Observations	10,538	12,926	10,384	11,562

Table 6: **Effect of SEPA Enactments on Dirty and Clean Industries**

This table reports county-level regressions examining the effect of SEPA adoption on establishment counts in pollution-intensive (*Dirty*) and non-polluting (*Clean*) industries. The dependent variable is the natural logarithm of total establishments (*Ln Establishments*) for each county-year. Columns (1)–(2) present results for dirty industries (two-digit SIC codes < 50), and Columns (3)–(4) present results for clean industries (SIC codes ≥ 50). The dependent variables are the shares of establishments and employment in pollution-intensive ("dirty") industries relative to total activity. The short-run specification uses an event window of $(-5, +5)$ years around SEPA adoption, and the long-run specification compares $(-5, -1)$ and $(+20, +25)$ windows. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dep Var: Ln Establishments				Dep Var: DirtyMixEstabs	
	Dirty Industries		Clean Industries			
	(1)	(2)	(3)	(4)	(5)	(6)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	-0.091***	-0.219***	-0.034*	-0.143***	-0.009**	-0.011**
	(-3.818)	(-4.390)	(-1.867)	(-2.861)	(-2.448)	(-2.238)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES	YES
Observations	10,014	12,392	10,366	12,760	10,538	12,926

Table 7: **Effect of SEPA Enactments on Small, Medium and Large Establishments**
This table reports county-level regressions examining the effect of SEPA adoption on establishments of different sizes. The dependent variable is the natural logarithm of the number of establishments, categorized as *Small* (fewer than 50 employees), *Medium* (50–499 employees), and *Large* (500 or more employees). Panel A presents estimates for border counties located on either side of SEPA–non-SEPA state borders, and Panel B reports results for interior counties one layer inside each state. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Three Size Groups of Establishments

	Small Estabs		Medium Estabs		Large Estabs	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	-0.052***	-0.168***	0.000	-0.067	-0.018	-0.019
	(-2.828)	(-3.427)	(0.016)	(-1.496)	(-0.467)	(-0.159)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES	YES
Observations	10,538	12,926	10,538	12,926	10,538	12,926

Panel B: Small Firms in Inner Counties

	(SEPA vs. Non-SEPA) Inner		(SEPA) Boarder vs. Inner		(Non-SEPA) Boarder vs. Inner	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	-0.210	-0.350	-0.058**	-0.230***	-0.024	-0.036
	(-1.243)	(-1.411)	(-2.082)	(-2.606)	(-1.546)	(-0.740)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES	YES
Observations	15,711	19,515	8,478	9,384	8,962	9,928

Table 8: **Effect of SEPA Enactments on County's Dirty vs Clean Establishments Mix**

This table reports county-level regressions examining the effect of SEPA adoption on the composition of dirty versus clean establishments by firm size. The dependent variables are the shares of establishments and employment in pollution-intensive ("dirty") industries, calculated within each size category. Columns (1)–(3) report results for the dirty-industry share of establishments, and Columns (4)–(6) report results for the dirty-industry share of employment. *Small*, *Medium*, and *Large* establishments are defined as those with fewer than 50, between 50 and 499, and 500 or more employees, respectively. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dirty Mix Establishments			Dirty Mix Employees		
	Small	Medium	Large	Small	Medium	Large
	Sample Years Relative to Event Years: (-5,-1) & (+20,+25)					
Post x Treated	-0.013***	0.019*	0.035	-0.015**	0.008	0.034
	(-2.738)	(1.849)	(1.282)	(-2.539)	(0.819)	(1.251)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES	YES
Observations	12,864	12,152	5,280	12,864	12,152	5,280

Appendix A Variable Definitions

In this appendix, we provide a comprehensive explanation of how we calculate our variables used in our analyses.

Total Establishments: The total number of business establishments in a county-year, as reported in the U.S. Census Bureau's County Business Patterns (CBP) data.

Employees: The total number of paid employees across all establishments in a county-year, as reported in the CBP.

Ln Establishments: The natural logarithm of the total number of establishments in a county-year.

Ln Employees: The natural logarithm of total employment in a county-year.

Dirty Establishments: The number of establishments in pollution-intensive industries, defined as two-digit SIC codes less than 50 (e.g., manufacturing, mining, construction, and utilities).

Clean Establishments: The number of establishments in non-polluting industries, defined as two-digit SIC codes greater than or equal to 50 (e.g., trade, finance, services, and professional sectors).

Ln Dirty Establishments: The natural logarithm of the number of dirty establishments in a county-year. Dirty industries are defined as the industries with SIC two-digit code below 50.

Ln Clean Establishments: The natural logarithm of the number of clean establishments in a county-year. Clean industries are defined as the industries with SIC two-digit code 50 or above.

Small Establishments: The number of establishments with fewer than 50 employees, aggregated from CBP establishment-size bins.

Medium Establishments: The number of establishments with between 50 and 499 employees.

Large Establishments: The number of establishments with 500 or more employees.

Ln Small Establishments: The natural logarithm of the number of small establishments in a county-year.

Ln Medium Establishments: The natural logarithm of the number of medium establishments in a county-year.

Ln Large Establishments: The natural logarithm of the number of large establishments in a county-year.

Dirty Employees: The number of employees working in pollution-intensive industries (two-digit SIC codes < 50).

Clean Employees: The number of employees working in non-polluting industries (two-digitSIC codes ≥ 50).

Ln Dirty Employees: The natural logarithm of the number of employees in dirty industries (two-digit SIC codes < 50).

Ln Clean Employees: The natural logarithm of the number of employees in clean industries (two-digitSIC codes ≥ 50).

Dirty Mix (Establishments): The share of establishments in dirty industries, calculated as the number of dirty establishments divided by total establishments in a county-year.

Dirty Mix (Employees): The share of total employment in dirty industries, calculated as the number of employees in dirty industries divided by total employees in a county-year.

Retail Mix (Establishments): The share of establishments in retail and restaurant industries, defined as two-digit SIC codes 52–59, divided by total establishments in a county-year.

Retail Mix (Employees): The share of total employment in retail and restaurant industries, defined as two-digit SIC codes 52–59, divided by total employees in a county-year.

Payroll: Total annual payroll (in millions) for all establishments in a county-year, as reported in the CBP.

Ln Payroll: The natural logarithm of total annual payroll for each county-year.

Δ% Establishments: The annual percentage change in the total number of establishments relative to the previous year.

Post: An indicator variable equal to one for all years after a state adopts a State Environmental Policy Act (SEPA) and zero otherwise.

Treated: An indicator variable equal to one for counties located within SEPA-adopting states that border non-SEPA states, and zero for adjacent counties across the border in non-SEPA states.

Post $\times$ Treated: The interaction term identifying counties exposed to SEPA review after its enactment; captures the differential post-adoption effect in treated counties relative to adjacent non-SEPA counties.

PCSpend1970: Per-capita state government expenditures on environmental and natural-resource programs in 1970, used as a proxy for baseline environmental enforcement capacity.

Post $\times$ Treated $\times$ PCSpend1970: The triple interaction term capturing how the local economic effect of SEPA adoption varies with pre-existing state enforcement capacity.

Border Counties: Counties located along the boundary between SEPA-adopting and non-SEPA states.

Interior Counties: Counties located one layer inside a state, not sharing a direct border with another state.

Dirty Industries: Industries classified as pollution-intensive, typically including manu-

facturing (SIC 20–39), mining (SIC 10–14), construction (SIC 15–17), and utilities and transportation (SIC 40–49).

Clean Industries: Industries with low environmental impact, defined as SIC codes ≥ 50, including wholesale and retail trade, finance, insurance, real estate, and service sectors.

Retail and Restaurant Industries: Industries with SIC codes 52–59, capturing consumer-facing, non-tradable sectors such as retail stores, food services, and restaurants.

SEPA (State Environmental Policy Act): A state-level procedural environmental review statute that extends National Environmental Policy Act (NEPA)-style assessment requirements to state and local permitting, zoning, and public-funding decisions.

Event Windows: Periods centered on SEPA adoption used for analysis: short-run $(-5, +5)$ years around adoption and long-run $(-5, -1)$ vs. $(+20, +25)$ years for steady-state comparisons.

Appendix B Additional Plots & Tables

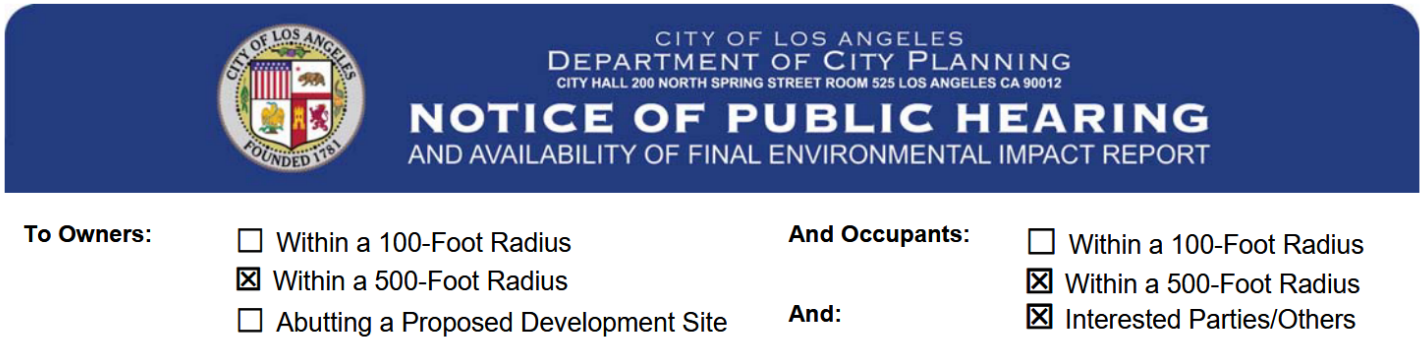
Requirement for Extensively Detail Environmental Mitigation Summary for a Hotel in CA

Impact	Mitigation Measures	Original Impact / Residual Impact with Mitigation			
		Alternative A	Alternative B	Alternative C	Alternative D
Section 3.2. Land Resources					
Topography – Development could result in changes to site topography from grading activities	No mitigation required.	LS	LS	LS	NI
Seismic Conditions – Construction near an active fault could yield adverse effects associated with seismic activity	The use of BMPs would minimize impacts related to seismic conditions. No mitigation required.	LS	LS	LS	NI
Soil Characteristics – Construction and grading activities could cause soil erosion	The use of BMPs would minimize impacts related to soil erosion. No mitigation required.	LS	LS	LS	NI
Mineral Resources – No known mineral resources would be impacted	No mitigation required.	NI	NI	NI	NI
Section 3.3. Water Resources					
Surface Water – Impacts related to surface water could include:					
1) **Construction Impacts** – Construction activities could increase the discharge of sediment and pollutants to surface waters	The use of BMPs would minimize impacts related to surface water quality during construction. No mitigation required.	LS	LS	LS	NI
2) **Stormwater Runoff** – Project alternatives could alter stormwater quantity, quality, and/or drainage patterns	The use of BMPs would minimize impacts related to stormwater runoff during operation. No mitigation required.	LS	LS	LS	NI
3) **Floodplain** – Development within a floodplain could cause adverse impacts due to inundation.	Building components intentionally sited outside of regulatory floodway. Any earthwork within floodplain would be balanced to prevent impacts to regulatory floodway. No mitigation required.	LS	LS	LS	NI

NI = No Impact LS = Less than Significant PS = Potentially Significant S = Significant BI = Beneficial Impact

Figure A.I: **Example of a Detailed Environmental Mitigation Table under CEQA.** This figure shows a single page from a 50-page mitigation summary table, part of a nearly 400-page Environmental Impact Statement for a proposed hotel project in California. The table illustrates the granular level of analysis required by SEPA, where dozens of specific potential environmental impacts (e.g., soil erosion, stormwater runoff) must be identified. For each impact, the project proponent must propose a corresponding "Mitigation Measure" designed to reduce the impact's severity to a "Less than Significant" (LS) level. This document is a primary example of the complex and costly procedural requirements that contribute to the high fixed costs of SEPA compliance.

Notice of Public Hearing for a Proposed Project in California



CITY OF LOS ANGELES
DEPARTMENT OF CITY PLANNING
CITY HALL 200 NORTH SPRING STREET ROOM 525 LOS ANGELES CA 90012

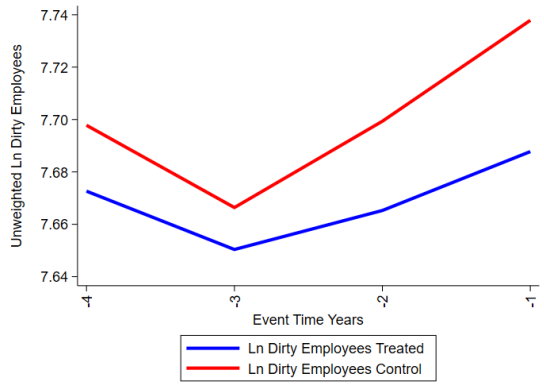
NOTICE OF PUBLIC HEARING
AND AVAILABILITY OF FINAL ENVIRONMENTAL IMPACT REPORT

To Owners:	☐ Within a 100-Foot Radius	**And Occupants:**	☐ Within a 100-Foot Radius
	☒ Within a 500-Foot Radius		☒ Within a 500-Foot Radius
	☐ Abutting a Proposed Development Site	**And:**	☒ Interested Parties/Others

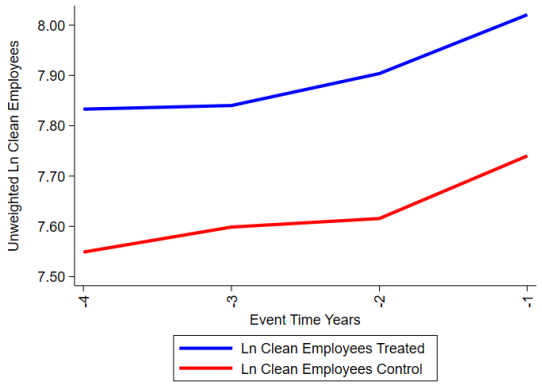
This notice is sent to you because you own property or are an occupant residing near a site for which an application was filed with the Department of City Planning. All interested persons are invited to attend the public hearing where you may listen, ask questions, and/or present testimony regarding the project. The environmental document, will be among the matters considered at the hearing. The hearing officer or decision-maker may consider all the testimony presented at the hearing, written communications received prior to or at the hearing, and the merits of the project as it relates to existing environmental and land use regulations. **Please note that your attendance at the hearing is optional.**

Project Site: 1240-1260 S. Figueroa Street & 601 W. Pico Boulevard, Los Angeles, CA 90015

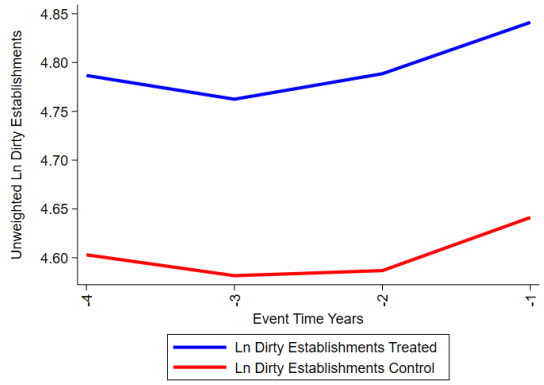
Figure A.II: **Example Notification for a Public Hearing Under CEQA.** This figure shows a standard "Notice of Public Hearing" for a proposed hotel project in Los Angeles, California. The notice illustrates the broad public engagement mandated by the SEPA process. Critically, notification is not limited to immediate neighbors; the city provides notice to residents within a 500-foot radius and explicitly to "Interested Parties/Others." As stated in the text, the hearing is an open forum where any interested person may "listen, ask questions, and/or present testimony regarding the project." This open-invitation format exposes the project to a wide range of potential opponents, including those whose motivations may be unrelated to the project's direct environmental impacts, creating a significant source of political and litigation risk.



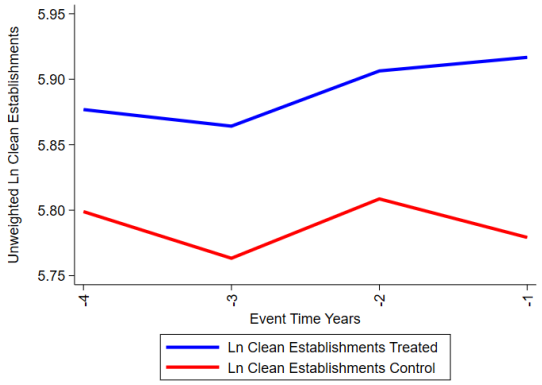
(a) Dirty Employees



(b) Clean Employees



(c) Dirty Establishments



(d) Clean Establishments

Figure A.III: **Pre-Trend Dirty vs Clean Establishment and Employees Prior to SEPA Adoption.** This figure illustrates unweighted pre-adoption trends in the main variables used in the analysis for treated (SEPA) and control (non-SEPA) border counties. Each subplot displays the unweighted average logarithm of a county-level outcome over the four years preceding SEPA adoption, with blue lines representing SEPA counties and red lines representing non-SEPA counties. Panel (a) and (b) show total and dirty-industry and clean-industry employment, respectively. The final panels, (c) and (d), plot clean and dirty establishments, respectively. Across all variables, treated and control series move in parallel throughout the pre-SEPA window, indicating that counties on either side of the state border followed similar trajectories before the introduction of environmental review requirements. These balanced pre-trends support the validity of the comparative border framework employed in the main analysis. The patterns confirm that SEPA and non-SEPA border counties were on comparable economic paths prior to enactment, validating the use of their differential post-adoption evolution to study the localized effects of state-level environmental regulation.

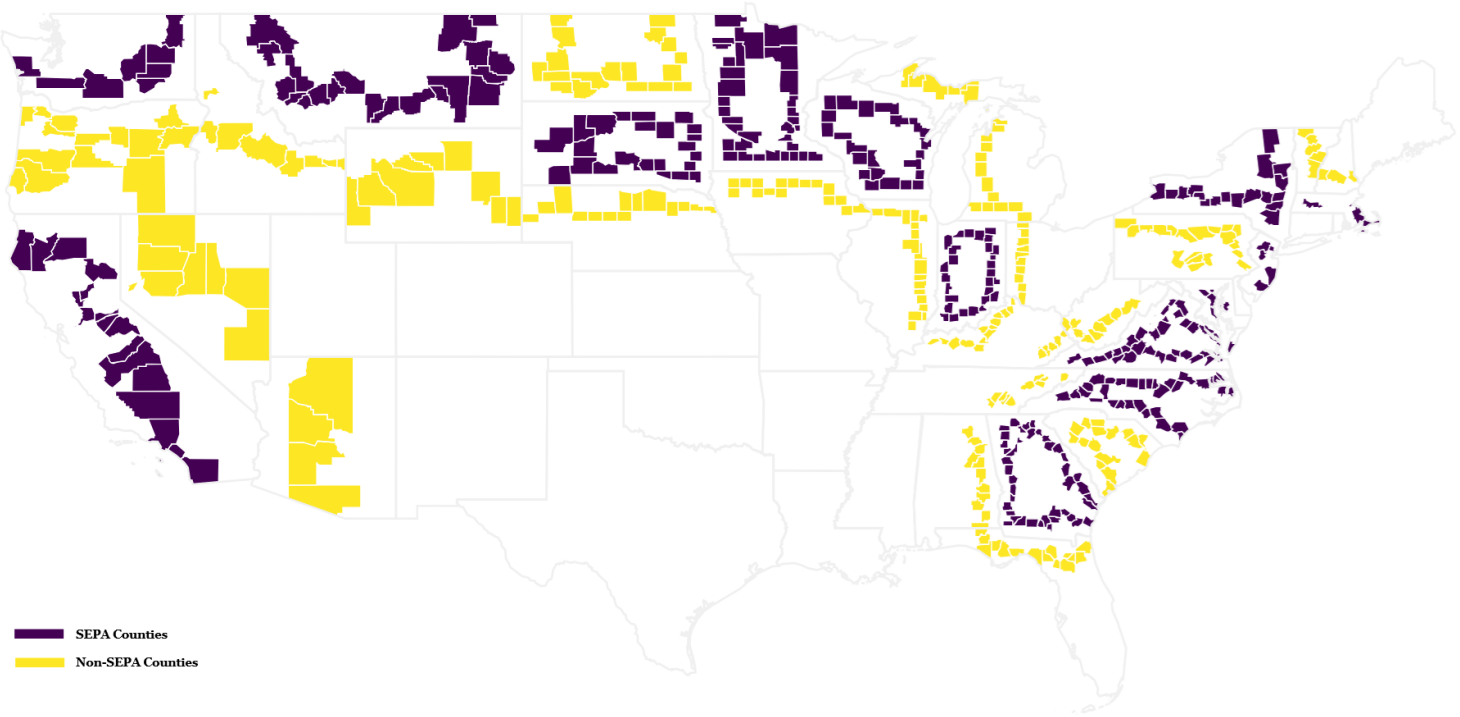


Figure A.IV: **SEPA vs. Non-SEPA Inner-State County Groups.** This figure illustrates the construction of the "inner-state" county groups used for the placebo-style robustness tests designed to assess the spatial reach of SEPA effects. The example uses counties along the Indiana–Illinois border. Clay and Greene counties in Indiana (SEPA state, shown in red) are matched with Coles, Cumberland, and Jasper counties in Illinois (non-SEPA state, shown in yellow), forming the primary border treatment–control group. To create the inner-state groups, we identify each county's immediate in-state neighbors that do not directly border the other state. The SEPA-side group therefore includes interior Indiana counties adjacent to Clay and Greene, while the non-SEPA-side group consists of interior Illinois counties adjacent to Coles, Cumberland, and Jasper. Unlike the main border-pair analysis, which relies on one-to-one county pairs, this grouping method produces many-to-many sets of geographically proximate but non-bordering counties. This design allows us to test whether the observed SEPA impacts are strictly localized to border regions or whether they diffuse into nearby interior counties through regional agglomeration and supply-chain linkages.

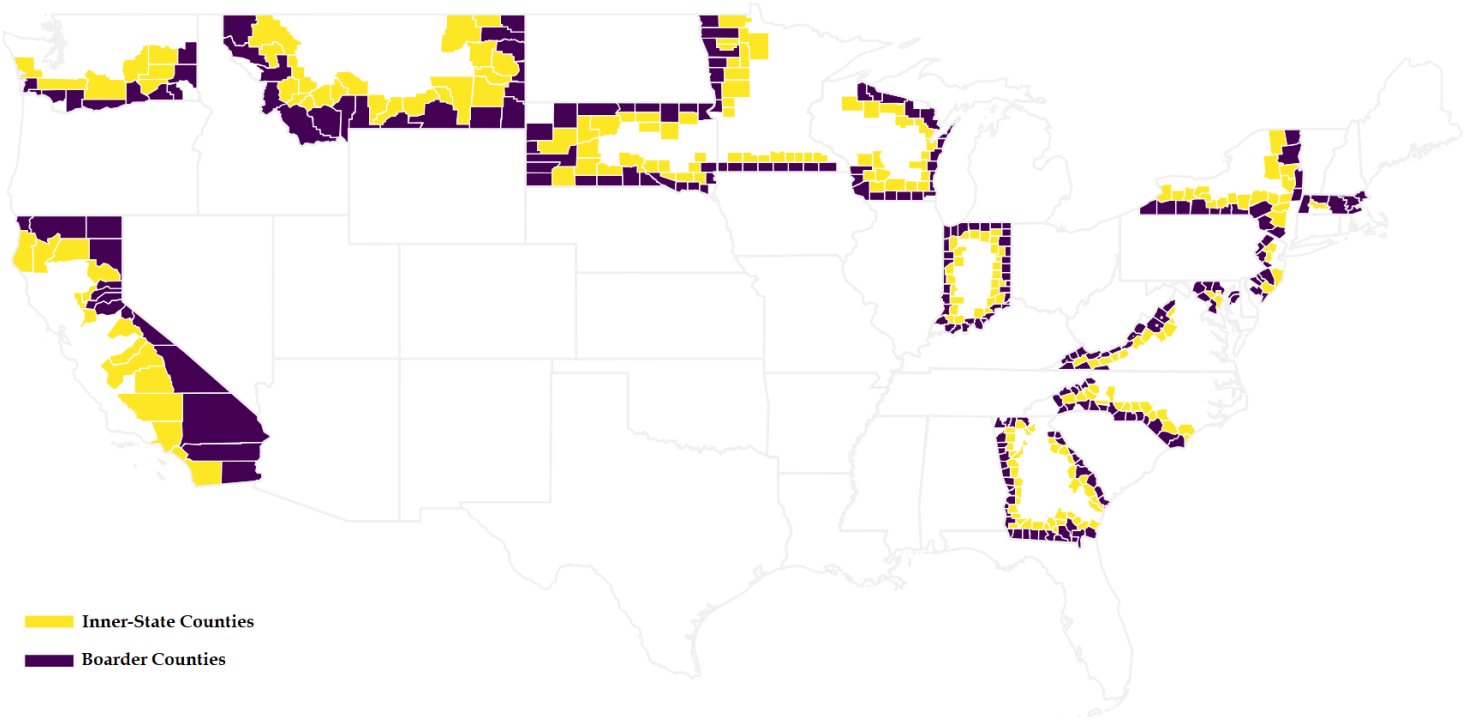


Figure A.V: **Within-SEPA State County Pairs.** This figure illustrates the construction of the within-state county pairs used to examine whether SEPA impacts are concentrated near the state border or diffuse throughout SEPA-adopting states. The example uses Indiana, a SEPA state. The yellow-shaded counties (Vigo and Sullivan) represent border counties, while the red-shaded counties (Parke, Clay, and Greene) are their immediate interior neighbors. Each dashed line denotes a unique within-state pair connecting a border county to an adjacent inner-state county—for example, (Vigo, Parke), (Vigo, Clay), (Sullivan, Clay), and (Sullivan, Greene). This pairing procedure mirrors the main border-pair design but is restricted to counties within the same SEPA state. By comparing border and interior counties exposed to the same regulatory regime, this analysis tests whether the economic effects of SEPA adoption are localized to border regions or extend more broadly through intra-state agglomeration and network channels.

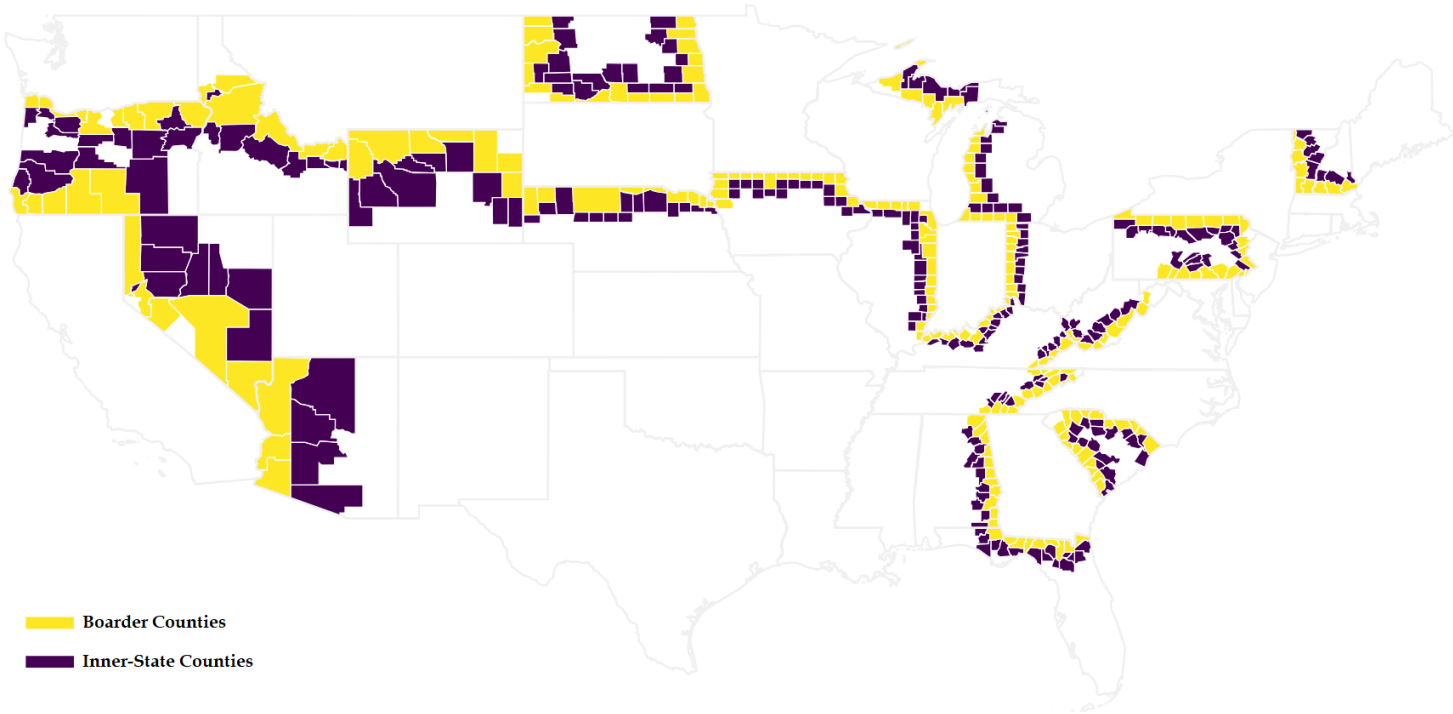


Figure A.VI: **Within-Non-SEPA State County Pairs.** This figure illustrates the construction of the within-state county pairs used for the placebo analysis in non-SEPA states. The example uses Illinois, a non-SEPA state bordering SEPA-adopting Indiana. The yellow-shaded counties (Clark and Crawford) represent border counties adjacent to SEPA jurisdictions, while the red-shaded counties (Cumberland, Coles, Jasper, and Richland) represent their immediate interior neighbors. Each dashed line indicates a unique within-state pair connecting a border county to an adjacent inner-state county—for example, (Clark, Cumberland), (Clark, Coles), (Crawford, Jasper), and (Crawford, Richland). This pairing procedure mirrors the within-SEPA analysis shown in Figure A.V, but is restricted to counties in non-SEPA states. By comparing border and interior counties exposed to the same unregulated policy environment, this analysis serves as a placebo test. The absence of significant differences between border and inner counties in non-SEPA states strengthens the causal interpretation that the negative effects observed in SEPA regions are driven by environmental regulation rather than general border or regional economic trends.

Table A.I: **Treated vs Control Counties.**

This table reports summary statistics for treated (SEPA-side) and control (non-SEPA-side) counties in the years surrounding SEPA adoption. The variables include the natural logarithm of total establishments and employees (*Ln Establishments* and *Ln Employees*) based on County Business Patterns (CBP) data for each county-year. *Ln Dirty Establishments* and *Ln Clean Establishments* denote the natural logarithm of establishments in pollution-intensive (two-digit SIC codes < 50) and non-polluting (SIC codes ≥ 50) industries, respectively. *Ln Small*, *Ln Medium*, and *Ln Large Establishments* correspond to establishments with fewer than 50, between 50 and 499, and 500 or more employees, respectively. *Dirty Mix* is the share of establishments in dirty industries, calculated as the ratio of dirty establishments to total establishments for each county-year. $\Delta\%$*Establishments* denotes the annual percentage change in total establishments relative to the previous year. The sample spans 1964–2021, and the statistics reported here represent snapshots at $T-2$, $T-1$, and $T-0$ relative to each SEPA adoption year. Panel A presents unweighted values, while Panel B reports statistics weighted by the treated county's employment. Variable definitions are provided in Appendix A.

Panel A: Unweighted Numbers

	Treated Year-4	Control Year-4	Treated Year-3	Control Year-3	Treated Year-2	Control Year-2	Treated Year-1	Control Year-1	Avg. Diff (Trt - Ctrl)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total Establishments	6.132	5.965	6.105	5.947	6.070	5.964	6.109	6.004	-0.134
Total Employees	8.294	8.161	8.300	8.186	8.246	8.200	8.324	8.201	-0.104
Dirty Mix	0.242	0.256	0.241	0.248	0.238	0.253	0.235	0.244	0.011
Small Establishments	6.100	5.950	6.071	5.968	6.037	5.948	6.076	5.975	-0.111
Medium Establishments	2.927	2.831	2.988	2.825	2.945	2.891	2.999	2.916	-0.099
Large Establishments	1.547	1.345	1.542	1.365	1.440	1.353	1.494	1.381	-0.145
Dirty Establishments	4.813	4.603	4.792	4.582	4.755	4.587	4.792	4.641	-0.185
Clean Establishments	5.892	5.799	5.873	5.763	5.844	5.809	5.869	5.779	-0.082
Dirty Employees	7.670	7.698	7.641	7.666	7.570	7.699	7.624	7.738	0.074
Clean Employees	7.813	7.549	7.837	7.599	7.824	7.616	7.925	7.740	-0.224

Panel A: Treated-Employee-Weighted Numbers

	Treated Year-4	Control Year-4	Treated Year-3	Control Year-3	Treated Year-2	Control Year-2	Treated Year-1	Control Year-1	Avg. Diff (Trt - Ctrl)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total Establishments	8.738	7.086	8.700	7.072	8.658	7.075	8.659	7.080	-0.134
Total Employees	11.428	9.490	11.378	9.515	11.321	9.514	11.327	9.502	-0.104
Dirty Mix	0.240	0.254	0.236	0.250	0.231	0.248	0.233	0.252	0.011
Small Establishments	8.684	7.036	8.647	7.033	8.603	7.022	8.605	7.028	-0.111
Medium Establishments	5.666	3.967	5.622	3.990	5.613	4.030	5.622	3.991	-0.099
Large Establishments	3.248	1.812	3.229	1.825	3.128	1.844	3.128	1.868	-0.145
Dirty Establishments	7.290	5.686	7.240	5.658	7.176	5.652	7.185	5.673	-0.185
Clean Establishments	8.478	6.821	8.430	6.797	8.394	6.799	8.409	6.788	-0.082
Dirty Employees	10.720	9.117	10.644	9.119	10.589	9.113	10.562	9.110	0.074
Clean Employees	10.808	8.912	10.804	8.959	10.807	9.005	10.808	9.037	-0.224

Table A.II: **Inner County Results for Employees**

This table reports county-level regressions examining the effect of SEPA adoption on employment in interior and border counties. The dependent variable is the natural logarithm of total employment (*Ln Employees*) for each county-year. Columns (1)–(2) compare SEPA and non-SEPA interior counties, Columns (3)–(4) compare SEPA border counties to interior counties within SEPA states, and Columns (5)–(6) compare non-SEPA border counties to their interior counterparts. The short-run specification uses an event window of $(-5, +5)$ years around SEPA adoption, and the long-run specification compares $(-5, -1)$ and $(+20, +25)$ windows. The sample spans 1964–2021, excluding 1974 due to a change in the U.S. Census Bureau's data collection methodology. Each county pair is weighted by the treated county's employment divided by the number of cross-border matches it forms. For example, the weight for San Bernardino, CA equals its employment divided by three, reflecting its three adjacent non-SEPA counties (La Paz and Mohave, AZ; Clark, NV). Standard errors are clustered at the county-pair level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(SEPA vs. Non-SEPA) Inner		(SEPA) Boarder vs. Inner		(Non-SEPA) Boarder vs. Inner	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	0.029	-0.049	-0.087**	-0.314*	-0.050	-0.030
	(0.329)	(-0.244)	(-2.429)	(-1.745)	(-1.498)	(-0.525)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	Yes	YES	YES	YES	YES	YES
Observations	15,757	19,555	8,552	9,450	8976	9,942

Table A.III: **Effect of SEPA Enactments on County's Retail vs Other Establishments**
This table shows shows the county-level OLS regression of (a) the share of Retail and Restaurant industries' establishments in the total number of establishments for a given county in columns (1) and (3) and (b) the share of employees in Retail and Restaurant industries' establishments in the total number of employees for a gvien country in columns (2) and (4) on the POST, TREATED and POST*TREATED variables. We exclude 1974 due to the change in data collection schemes by the U.S. Census Bureau in 1974 compared to that before 1974. Retail and Restaurants industries include the SICs 52 through 59. In the regression, each county pair is weighted by the treated county's employment divided by its number of control (out-of-state neighboring) counties. For example, San Bernardino, CA's weight equals its employment divided by three, reflecting the three control counties (La Paz, AZ; Mohave AZ; Clark, NV). We cluster the standard errors at the county-pair level. The ***, **, * denote 1%, 5%, and 10% statistical significance, respectively.

	(1)	(2)	(3)	(4)
	Retail Ln Establishments		Retail Mix Establishments	
Year Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)
Post x Treated	-0.014	-0.140***	0.015***	0.023***
	(-0.965)	(-2.891)	(3.277)	(3.014)
Observations	10,340	12,716	10,538	12,926

Table A.IV: **Summary of SEPA Legislations**
This table summarizes the principal statutory features of State Environmental Policy Acts (SEPAs) across adopting U.S. states. Panel A provides narrative summaries of key legislative provisions, including the scope of application, procedural requirements, mitigation and disclosure standards, and judicial review mechanisms, as documented in Mandelker et al. (2024). Panel B presents a comparative cross-state overview of environmental-justice provisions, cumulative-impact mapping, substantive requirements, public-participation procedures, and whether each SEPA applies at the state or local level, based on (Rolle and Snape, 2022). Together, these panels highlight the substantial heterogeneity in SEPA design, enforcement scope, and implementation across jurisdictions.

Panel A: Summary of SEPA Provisions from Mandelker et al. (2024)

State	**Summary**
CA	Requires environmental impact report similar to federal statement, including mitigation measures and growth-inducing effects. Applies to state agencies and local governments. Detailed provisions governing preparation of impact report and judicial review. State agency to prepare Guidelines. Statutory terms defined.
CT	State agencies to prepare environmental impact evaluations similar to federal impact statement, including mitigation measures and social and economic effects. Actions affecting environment defined.
DC	Mayor, District agencies and officials to prepare impact statements on projects or activities undertaken or permitted by District. Impact statement to include mitigation and cumulative impact discussion. Action to be disapproved unless mitigation measures proposed or reasonable alternative substitute to avoid danger.
GA	Applies to projects proposed by state agencies for which it is probable to expect significant effect on the natural environment. Limited primarily to land-disturbing activities and sale of state land. Decision on project not to create cause of action.

Continued on next page

Table A.IV: Summary of SEPA Legislation (continued)

State	Summary
HI	State agencies and local governments to prepare impact statements on use of public land or funds and land uses in designated areas. Statements must be "accepted" by appropriate official. Judicial review procedures specified.
IN	Similar to NEPA. Applies to state agencies.
MD	State agencies to prepare environmental effects reports covering environmental effects of proposed appropriations and legislation, including mitigation measures and alternatives.
MA	State agencies and local authorities to prepare environmental impact reports covering environmental effects of actions, mitigation measures and alternatives. Must specify feasible measures to avoid damage to environment or mitigate or minimize damage to maximum extent practicable.
MN	State agencies and local governments to prepare environmental impact statements covering environmental effects of actions, mitigation measures and economic, employment and sociological effects. Procedures for preparation of statements and for judicial review specified. State environmental quality board may reverse or modify state actions inconsistent with policy or standards of statute.
MT	Similar to NEPA. Applies to state agencies.
NY	State agencies and local governments to prepare impact statements similar to federal impact statement and including mitigation measures and growth-inducing and energy impacts. Statutory terms defined. Procedures for preparing statement specified. State agency to adopt regulations on designated topics.

Continued on next page

Table A.IV: Summary of SEPA Legislation (continued)

State	Summary
NC	Similar to NEPA. Applies to state agencies. Local governments may also require special-purpose governments and private developers of major development projects to submit impact statement on major developments. Certain permits and public facility lines exempted.
PR	Similar to NEPA. Applies to Commonwealth agencies and political subdivisions.
SD	State agencies "may" prepare environmental impact statements similar to federal impact statement and adding mitigation measures and growth-inducing "aspects." Statutory terms defined. Ministerial and environmental regulatory measures exempt.
VA	Similar to NEPA. Applies to state agencies for major state projects. Impact statements also to consider mitigation measures and impact on farmlands.
WA	State agencies and local governments to prepare impact statements identical to federal statement but limited to "natural" and "built" environment. Proposal may be denied if it has significant impacts or mitigation measures insufficient. Judicial review procedures specified. State agency to adopt regulations on designated topics.
WI	Similar to NEPA. Applies to state agencies. Statements also to consider beneficial aspects and economic advantages and disadvantages of proposals.

Panel B: Summary of SEPA Provisions from (Rolle and Snape, 2022)

State	EJ Provision	Cumulative Impacts	EJ Mapping	Substantive Provision	Public Participation	Permitting	SEPA Applies	
							State Only	State & Local
California		X	X	X	x	x	x	
Connecticut		X	X		x			
DC		X		X	x		x	
Georgia		X	X		x			
Guam							x	
Hawaii		X			x		x	
Indiana								
Maryland			X					
Massachusetts	x		X	X	x	x	x	
Minnesota		X	X	X	x		x	
Montana		X		X	x		x	
New Jersey	x	X	X	X	x		x	
New York	x	X	X	X	x		x	
North Carolina		x	X					
Puerto Rico					x	x	x	
South Dakota		x					x	
Virginia		x			x			
Washington		x	X		x		x	
Wisconsin		x			x		x	

Appendix C Data Description

As we discuss in Section 3.4, we operationalize our difference-in-differences analysis using a sample of county-level economic outcomes starting in 1964. In this section, we discuss the construction of this sample as well as provide descirpitve statistics to provide evidence on our key identifying assumptions.

Appendix C.1 Institutional Detail of Census Data

The County Business Patterns (CBP) data, published by the U.S. Census Bureau, represent a cornerstone for economic research, offering a detailed view of the United States' industrial structure at the county level. Initiated in 1946, and published annually since 1964, with some intermittent releases between 1946 and 1962, the CBP provides statistics on employment, payroll, and the number and size of business establishments by industry and county.

The foundation of the CBP data lies in the U.S. Census Bureau's Business Register (BR), a comprehensive database compiled from the administrative tax records of all private, non-farm employer establishments in the United States. This register serves as the underlying frame for all economic data collected by the Census Bureau. To enhance the BR data, the Census Bureau incorporates additional information from various sources, including its Economic Censuses, Annual Surveys, Current Business Surveys, and Company Organization Surveys, as well as data from other agencies such as the Bureau of Labor Statistics and the Social Security Administration. Since 1974, the establishment has been the primary unit of analysis for CBP tabulations. Employment is assigned to specific industries and locations by aggregating the employment reported by establishments sharing the same industry and location code. Prior to 1974, multi-location employers outside of manufacturing were permitted to aggregate their employment to a central reporting office. The CBP program employs both automated and analytical edits to the raw data to identify and remove anomalies and to ensure the validity

of geographic coding, addresses, and industry classifications

Our sample period goes from 1964 to upto 2020, depending on the type of analysis. In some specifications, we are analyzing the effects of SEPA enactments on business formations 30 years from the enactment dates. Before 1964, the CBP did not cover all counties individually; smaller adjacent counties were sometimes grouped together for publication purposes, particularly in states with a high number of counties. After 1964, data for all U.S. counties became available.

Appendix C.2 Data Work and Variable Construction

For each year, we download CBP data from ICPSR. Each row of the data consists of a long string variable with all values from state and county fips to employees to establishments in different buckets, and all lumped into one long string variable. The length of the string variable differs each year depending on how they treat the null variable. In some years, it changes because they introduce a new variable or new bin for establishments (e.g., establishments with 0 to 3 employees vs establishments with 1 to 4 employees). The files do not contain the variable information nor tell you which variable is in which position in the string.

We parse our variables using the guidance from Eckert et al. (2022), manual visual inspection, trial and error, and analyzing any abnormal jumps in any variable to ensure we assign the correct values to the right variables. The data files are most accurate at the county aggregate level. Each county-level number is further broken down by one-digit SICs, and then each one-digit SIC code value is further broken into digits, SIC code values, two digits to three, and three digits to four. However, as we go down to the granular level, data becomes very spotty. The four-digit values do not necessarily sum up to three, the three-digit values do not necessarily sum up to two digits, and so on.

One of the primary motives for our study is to look at changes in the industry composition of the business due to enactments of SEPAs. As SEPAs increase costs more for brown or

dirty industries than for clean industries, splitting establishments into brown vs. clean is very natural and critical for our research question. Hence, we decided to use the two-digit SIC code level numbers. As shown in [Figure (XYZ)], numbers provided at two-digit SIC codes sum up to [80%] of the aggregate county number. So, even though we do not use every establishment from CBP data, we are confident we are using the lion's share of the information provided.

There has been some shift in establishments' bins over time. Within our sample period, from 1964 to xyz, there are eight bins based on the number of employees: n0_3, n4_7, n8_19, n20_49, n50_99, n100_249, n250_499, and n500. Names of each bucket provide the employees with the minimum and maximum number of employees. For example, n8_19 gives us the number of establishments with eight to nineteen employees. All establishments with 500 or more employees are lumped into the last bucket. Starting in 1974, the Census changed the construction of these establishment bins. They now report establishments in thirteen different bins: n1_4, n5_9, n10_19, n20_49, n50_99, n100_249, n250_499, n500_999, n1000, n1000_1, n1000_2, n1000_3, and n1000_4. Definitions of these bins are the same as before. However, the bin that reports the establishments with the most employees is n1000, which has all the establishments with one thousand or more employees. The buckets n1000_1 through n1000_4 give further detail on large establishments but are part of the n1000 count.

Appendix C.3 Small, Medium and Large Establishments

The second dimension we wanted to explore is the effect of SEPA on different firm sizes. Extant literature primarily focuses on large and, in many cases, even public firms to analyze the impact of climate regulations. However, we wanted to understand the effect on the grass root levels. Hence, CBP benefits us by providing information on the tiny mom-and-pop shops with fewer than four employees. Even though the Census changed the bins before 1974 to 1974 or after, these are not that vastly different. When we aggregate those granular bins into

three major groups – small, medium, and large – those differences were almost immaterial to us. Our small firm group aggregates the establishment numbers from 0 to 49 employees and aggregates the bins n0_3, n4_7, n8_19, and n20_49 for the years 1964-1973 and the bins n1_4, n5_9, n10_19, and n20_49 for the year 1974 or after. Our medium firm group aggregates the establishment numbers from 50 to 499 employees and aggregates bins n50_99, n100_249, and n250_499 for all the years from 1964. As n1000 bins are unavailable before 1974, our large firm group uses n500 for 1964-1973 and n500_999 and n1000 for 1974 and after.

Appendix C.4 Industry Classification: Dirty, Clean, and Retail & Restaurants

To evaluate how SEPA adoption differentially affects local economic activity across sectors, we classify industries into three mutually exclusive groups: dirty, clean, and retail & restaurants. This classification is based on the two-digit Standard Industrial Classification (SIC) codes reported in the County Business Patterns (CBP) data.

Dirty industries are defined as those with SIC codes below 50, which encompass pollution- and resource-intensive activities. These industries are typically characterized by higher emissions, energy consumption, and direct exposure to environmental regulation. Examples include manufacturing (SIC 20–39, such as food processing, chemical production, fabricated metal products, and machinery), mining (SIC 10–14, such as coal, oil, and natural gas extraction), construction (SIC 15–17), and utilities and transportation (SIC 40–49, such as electric power generation, water supply, trucking, and warehousing). These sectors are the most directly affected by environmental review requirements because their operations often involve land alteration, emissions, or resource use subject to SEPA oversight.

Clean industries include all sectors with SIC codes of 50 and above, representing economic activities with relatively low environmental externalities and minimal direct compliance burdens under SEPA. Clean industries cover a broad range of activities, including wholesale

trade (SIC 50–51), finance, insurance, and real estate (SIC 60–67), professional and business services (SIC 70–89, such as law, accounting, education, healthcare, and entertainment), and other non-manufacturing service activities. While SEPA review may indirectly affect these industries through slower construction approvals or local spillovers, they are not directly targeted by the regulation.

Within the clean category, retail and restaurant industries are separately identified as those with SIC codes between 52 and 59. These sectors represent the core of the local, non-tradable, consumer-facing economy, including general merchandise stores (SIC 53), food stores (SIC 54), automotive dealers and service stations (SIC 55), apparel and accessory stores (SIC 56), furniture and home furnishing stores (SIC 57), and eating and drinking establishments (SIC 58–59). These industries are of particular interest because they capture the potential for amenity-driven growth that may arise from improvements in environmental quality following SEPA adoption.

This classification framework captures the main dimensions of heterogeneity relevant to environmental policy: the regulatory exposure of pollution-intensive sectors, the relative insulation of clean service industries, and the potential for amenity-led expansion in consumer-facing sectors. Using this structure, we analyze how SEPA-induced regulatory changes reshape local industrial composition and identify which parts of the economy bear the costs or benefit from environmental policy.

Appendix C.5 Employees for small, medium, and large firms

Even though the Census provides the establishment numbers in different buckets based on the employee size, they do not provide the employee breakdown by buckets. It is just one lump sum number by county by SIC, one to four digits. Along with establishments, we also wanted to understand the impact of SEPA on employment. So, we estimate the employees by each establishment bucket by multiplying the number of establishments in the bucket by the

average number of employees. For example, we estimate the employees in the establishment bucket n1_4 by multiplying establishment numbers by 2.5.

Appendix C.5.1 SEPA vs Non-SEPA Inner-State County Groups

In additional analyses, and to validate the hypothesis that the economic effects of SEPA adoption are geographically concentrated near state borders, we conduct a complementary exercise using a sample of counties located one layer inside the border on each side of the SEPA–non-SEPA divide. The premise is that if the adverse effects we observe in border counties truly stem from the localized costs of regulation and potential relocation opportunities, then those effects should dissipate rapidly as we move further inland. In contrast, if the observed effects reflect broader state-level shocks or unobserved macroeconomic differences, we would expect to find similar declines among these interior counties.

Figure A.VIIb illustrates how these "inner-state" groups are formed, using the example of Illinois (a non-SEPA state) and Indiana (a SEPA state). The construction begins with an existing border treatment–control group—such as SEPA counties Clay and Greene in Indiana paired with non-SEPA counties Coles, Cumberland, and Jasper in Illinois. For each county in this border group, we identify its immediate in-state neighbors that do *not* share a direct border with the other state. These in-state neighbors constitute the "inner counties." On the SEPA side, Clay and Greene's immediate in-state neighbors are grouped together, while on the non-SEPA side, the inner neighbors of Coles, Cumberland, and Jasper are grouped to form the comparison group. Each dashed line in the figure represents a connection between the original border county and its associated inner-state neighbor.

Unlike our main border-pair analysis, which relies on one-to-one matches, this inner-county approach produces many-to-many *county groups*. Each group is seeded by a border-pair relationship but can include multiple counties on either side depending on local geography and adjacency patterns. For instance, in Figure A.VIIb, the SEPA-side group may include

several interior Indiana counties neighboring Clay and Greene, while the non-SEPA-side group includes interior Illinois counties neighboring Coles, Cumberland, and Jasper. We again ensure uniqueness by consolidating overlapping matches and weighting each group by the total employment of its associated border counties, divided by the number of interior matches to prevent overweighting of large counties.

The goal of this analysis is twofold. First, it serves as a spatial falsification test to confirm that the observed SEPA effects are driven by localized border discontinuities rather than by broader, statewide dynamics. If relocation or investment deterrence occurs mainly near the regulatory boundary, we expect inner-state counties to display muted or insignificant treatment effects. Second, this test helps us distinguish between alternative mechanisms. A pure relocation story would imply minimal impacts away from the border, whereas a broader agglomeration story—where regulatory shocks weaken regional supplier networks or labor markets—could produce mild spillovers even in interior regions.

By examining these SEPA versus non-SEPA inner-county groups, we provide an additional layer of evidence regarding the spatial extent of SEPA's economic effects. Finding that these inner-state groups show little to no change in business formation or employment relative to one another strengthens the interpretation that the economic consequences of SEPA adoption are highly localized—concentrated in border counties where agglomeration forces are most vulnerable to regulatory frictions.

Appendix C.5.2 SEPA-Only County Pairs

In addition to the border-pair sample that compares SEPA and non-SEPA states, we construct a second sample of *within-state county pairs* to examine whether the effects of SEPA adoption are concentrated along state borders or more broadly distributed across the state. This analysis focuses exclusively on counties located within SEPA-adopting states and pairs each border county with one or more of its immediate interior neighbors. The

process mirrors the border-pair matching procedure described above but is restricted to a single regulatory regime.

Figure A.VIIc illustrates this construction using the example of Indiana, a SEPA state. The yellow-shaded counties (Vigo and Sullivan) represent the border counties, while the red-shaded counties (Parke, Clay, and Greene) are their immediate inner-state neighbors. Each dashed line represents a unique treated–control pair within the state. For instance, Vigo County (border) is paired with Parke and Clay Counties (interior), and Sullivan County (border) is paired with Clay and Greene Counties. This many-to-many pairing process yields four unique SEPA-only county pairs: (Vigo, Parke), (Vigo, Clay), (Sullivan, Clay), and (Sullivan, Greene). As with our main sample, we remove duplicate combinations and weight each pair by the treated (border) county's total employment divided by the number of interior matches it forms.

The motivation for this within-SEPA pairing is twofold. First, it serves as a robustness check by comparing counties exposed to the same overall policy and macroeconomic environment, thereby isolating whether the observed economic effects are localized to the state border or more uniformly spread throughout the state. Second, the SEPA-only analysis helps identify the role of spatial spillovers and agglomeration dynamics. If SEPA's effects are driven mainly by cross-border relocation, we would expect little difference between border and interior counties within the same state. Conversely, if border counties still experience larger declines relative to nearby interior counties, this would suggest that the primary mechanism is local agglomeration disruption rather than relocation across state lines.

This SEPA-only county pairing thus complements our main border-pair analysis by testing the internal spatial gradient of SEPA's economic effects. It allows us to determine whether the regulation's economic burden is truly confined to border areas or whether it diffuses more broadly within SEPA states through weakened regional linkages and investment networks.

Appendix C.5.3 Non-SEPA-Only County Pairs

We also construct a complementary sample of *within-state county pairs* for states that did not adopt SEPAs. This "non-SEPA-only" analysis serves as a placebo test, helping us assess whether the patterns observed in SEPA-adopting states are unique to the presence of environmental regulation or simply reflect broader regional dynamics. The procedure mirrors that used for SEPA-only pairs, but all counties in this case belong to states without SEPA statutes throughout the sample period.

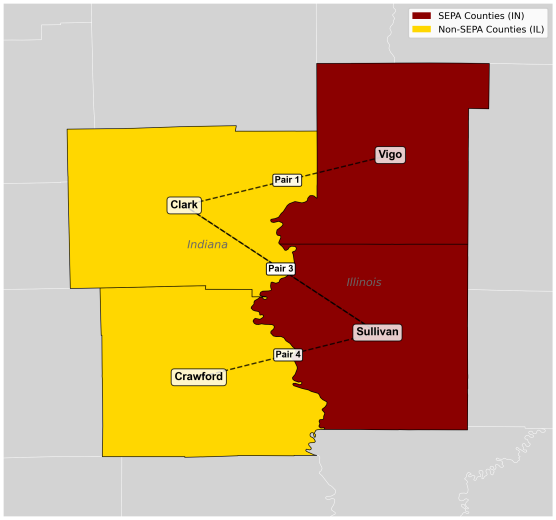
Figure A.VIId illustrates the construction of these pairs using the example of Illinois, a non-SEPA state. The yellow-shaded counties (Clark and Crawford) represent the border counties adjacent to SEPA states, while the red-shaded counties (Cumberland, Coles, Jasper, and Richland) are their immediate inner-state neighbors. Each dashed line represents a unique within-state pair connecting a border county to its neighboring interior county. For instance, Clark County (border) is paired with Cumberland and Coles Counties, while Crawford County (border) is paired with Jasper and Richland Counties. This process generates five unique non-SEPA-only county pairs: (Clark, Cumberland), (Clark, Coles), (Crawford, Jasper), (Crawford, Richland), and one additional overlapping border–interior combination as shown in the map. As in our other pairing procedures, we retain unique pairs and weight each by the border county's total employment divided by its number of matches.

The purpose of these non-SEPA-only pairings is to provide a *placebo benchmark*. Because these counties share similar geographic and economic characteristics but are all located in non-regulated states, any systematic differences between border and interior counties here would reflect underlying regional trends rather than environmental regulation. In contrast, if the estimated treatment effects appear only in SEPA and SEPA–non-SEPA border comparisons, this would confirm that our main results capture genuine policy-driven impacts rather than generic spatial or economic patterns.

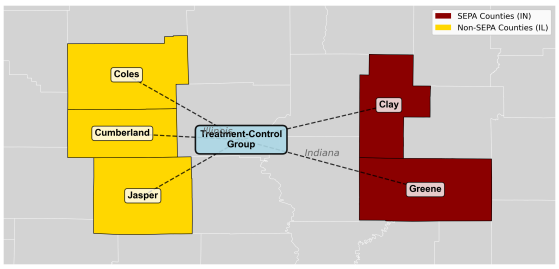
In addition, the non-SEPA-only analysis helps clarify whether border counties in un-

regulated states benefit from cross-border business inflows or agglomeration gains when neighboring SEPA states impose new regulations. If SEPA adoption in adjacent states prompted firm relocation across the border, we would expect to observe stronger growth in the border counties of non-SEPA states relative to their interior counterparts. Conversely, finding little or no difference between border and inner counties within non-SEPA states would suggest that relocation is minimal and that the primary mechanism underlying our results is the disruption of local agglomeration networks rather than pure cross-border migration of firms.

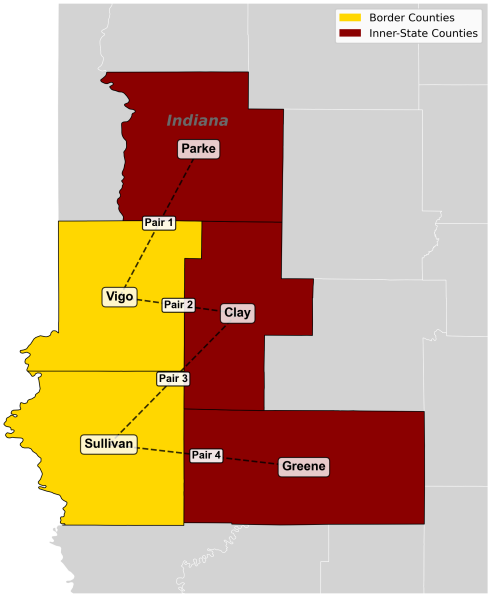
Overall, the non-SEPA-only county-pair analysis acts as a robustness and falsification exercise, reinforcing the causal interpretation of our border-pair findings. By demonstrating that comparable spatial patterns do not arise in states lacking SEPA regulation, we strengthen the argument that the documented economic contractions are indeed attributable to the implementation of state-level environmental policies.



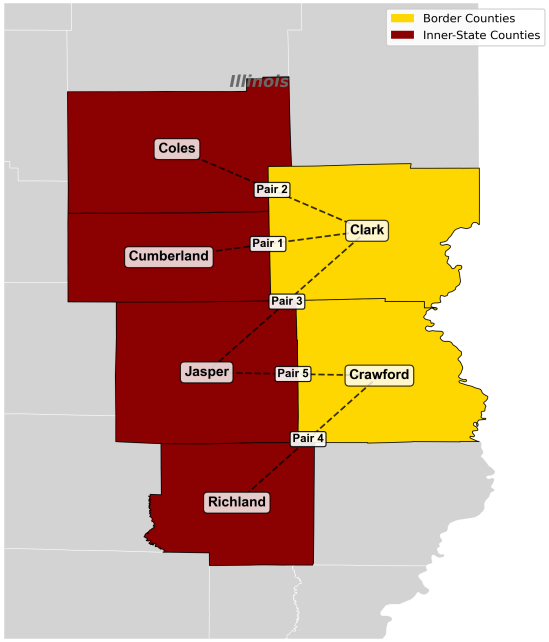
(a) SEPA vs non-SEPA Boarder County Pairs



(b) SEPA vs non-SEPA Inner-State County Groups



(c) Within SEPA States Border-Inner County Pairs



(d) Within non-SEPA States Border-Inncer County Pairs

Figure A.VII: **Examples of County Pair and Group Constructions.** This figure illustrates the four types of geographic pairings used in our analysis. (a) SEPA vs. non-SEPA border county pairs compare directly adjacent counties across state lines with and without SEPA regulation. (b) SEPA vs. non-SEPA inner-state county groups include counties one layer inland on each side of the border to test spatial attenuation. (c) Within-SEPA border–inner county pairs match SEPA border counties to their interior in-state neighbors to evaluate whether effects are localized within SEPA states. (d) Within-non-SEPA border–inner county pairs perform a placebo comparison for non-SEPA states to confirm that no similar patterns emerge in unregulated regions.