Borders Bear the Brunt: State Environmental Review and Severed Agglomerations

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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, with local banking structure shaping the magnitude of the contraction. In sum, local environmental regulations can erode local agglomerations, adversely affecting local small business growth.

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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.

SEPA-induced agglomeration effects operate through local financial networks. Using newly constructed measures of pre-existing banking depth and bank composition, we show that SEPA counties experience a significant long-run decline in the number of nationally chartered banks and a corresponding increase in the share of locally chartered institutions. Moreover, counties with deeper pre-period national-bank presence experience significantly smaller post-SEPA declines in establishments, indicating that stronger financial intermediation mitigates the real effects of regulatory frictions. Credit-market evidence further shows that consumer lending contracts following SEPA adoption, but the decline is attenuated where locally embedded banks account for a larger share of financial institutions. These patterns suggest that SEPA weakens cross-regional financial integration and that banking structure functions as a transmission channel for agglomeration effects, amplifying economic contraction in financially thinner regions while buffering it where local financial networks are stronger.

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, 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-

[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.

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 Conceptual Framework & Empirical Predictions

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

Both NEPA and SEPA are procedural statutes that trigger when a party engages with the government in question. Rather than setting substantive pollution limits, these rules require government agencies to assess and disclose the environmental impacts of major federal (NEPA) or state (SEPA) actions (Mandelker et al., 2024). In practice, NEPA is triggered by a federal interactions typically related to permits, funding, or federal land, so its scope is somewhat limited with respect to the everyday decisions of businesses. SEPAs, on the other hand, follow NEPA's structure by extending similar procedural requirements to state and local actions and—crucially—to private projects that require state or municipal permits, zoning approvals, or public funds. Because most commercial entry and expansion involves some combination of land-use, building, utility, or occupancy approvals, SEPAs can integrate environmental review into routine local permitting, attaching both compliance costs and timing/uncertainty costs to everyday business decisions (Ryan, 2012; Pindyck, 1991). Appendix A provides additional institutional detail and examples (including representative documents and process flow charts).

The broader reach of SEPAs means that they may 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 there is little systematic evidence that state-level environmental policies are economically relevant: canonical work often focuses on federal policy or on state differences that do not map cleanly onto SEPA-style procedural review (e.g., Clean Air Act attainment or manufacturing location) (Greenstone, 2002; Levinson, 1996; Henderson and Millimet, 2005). This gap is noteworthy given that states govern many of the land-use and permitting frictions that shape local business formation (Schuetz, 2023).

The localized geography within which state environmental policy operates has important implications for the economic impacts of these policies. When relocation costs are low, such as near a state border, the relevant counterfactual for marginal projects can be "just across the border," so even modest fixed or timing costs can re-sort business activity (Levinson, 1996; Henderson and Millimet, 2005; Greenstone, 2002). At the same time, agglomeration gains and losses are inherently local such that trimming borderline projects on one side can thin nearby supplier, labor, and customer networks without producing gains in adjacent counties (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 review and underscore how the economic impacts of environmental policy can differ when relocation margins are salient and cheap—precisely the case for SEPAs, and less so for many federal rules (Mandelker et al., 2024; Greenstone, 2002).

2.1 Business Impacts of SEPAs around State Borders

We next introduce a simple conceptual framework to clarify how a border-based empirical design can identify the business impact of SEPAs and help distinguish mechanisms. We proceed in three steps. We begin with the two most direct border-margin channels—deterrence/delay and relocation—which are the mechanisms that our border-pair design is best suited to detect. We then formalize these border-margin forces in a parsimonious static project-location model that yields sharp, testable implications. Finally, we step beyond the border model to address two issues that are central for interpretation: (i) how the statewide scope of SEPAs interacts with border amplification (i.e., whether the border exhibits additional effects beyond the statewide component), and (ii) whether longer-run dynamics reflect amenity rebalancing or agglomeration-driven persistence.

2.1.1 A Simple Model of Border-margin Mechanisms

We begin with a parsimonious framework that illustrates two key mechanisms through which state-level environmental review (i.e., SEPAs) may affect business activity. The first channel is *deterrence or delay.* Procedural review and policy uncertainty can deter marginal projects or slow firm expansion by raising fixed compliance costs and increasing timing risk (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991). The second channel, which is especially relevant at state borders where relocation costs are low, is *relocation.* If projects remain profitable but sufficiently mobile, activity can reoptimize toward the less-regulated side of the border (Levinson, 1996; Henderson and Millimet, 2005).

The model below illustrates how these channels generate localized predictions at the state line, and why they differ in whether a decline on the SEPA side is accompanied by "mirror-image" gains in adjacent non-SEPA counties.

Geography and projects. Consider a representative border pair with two adjacent counties: S on the SEPA side and N on the non-SEPA side. Each period, a mass Λ of potential projects arrives. Projects differ by type $k \in \{d, c\}$ (dirty/permitting-intensive vs. clean) and by size class $s \in \{\text{small}, \text{large}\}$.

Let M_S and M_N denote local economic scale (e.g., establishments/employment) in the two counties. We write the deterministic component of operating profits in county $\ell \in \{S, N\}$ as

$$\pi_{\ell k}(M_S, M_N) \;=\; \alpha_{\ell k} \;+\; \eta \log M_\ell \;+\; \phi \log(M_S + M_N), \tag{1}$$

where $\eta \geq 0$ captures within-county agglomeration and $\phi \geq 0$ captures the value of regional linkages that span the border (commuting, supplier/customer networks). The term $\alpha_{\ell k}$ includes baseline fundamentals and, potentially, amenities.

SEPA wedge and mobility costs. A project has baseline cost c_{ik} and idiosyncratic match shocks $\varepsilon_{iS}, \varepsilon_{iN}$. Locating across the line entails an idiosyncratic mobility/sorting cost $\kappa_i \geq 0$. We normalize the outside option (no entry) to zero.

SEPA affects only county S by imposing a wedge $w_{iks} \geq 0$ that summarizes fixed compliance costs and delay/uncertainty costs (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991). We allow w_{iks} to be larger for dirty projects and for smaller firms. Static net values are

$$U_{iS} = \pi_{Sk}(M_S, M_N) \; + \; \varepsilon_{iS} \; - \; c_{ik} \; - \; w_{iks}, \tag{2}$$

$$U_{iN} = \pi_{Nk}(M_S, M_N) \; + \; \varepsilon_{iN} \; - \; c_{ik} \; - \; \kappa_i. \tag{3}$$

The project enters in the location that maximizes $\{U_{iS}, U_{iN}, 0\}$.

2.1.2 Border Mechanisms and Long-run & Within-state Considerations

The static model is intentionally local: it formalizes border-margin mechanisms (deterrence/delay and relocation as well as the possibility of limited economic impact) that are most naturally identified by adjacent-county comparisons. We use it to interpret short-run border effects, and then step beyond it to organize how we interpret longer-run dynamics and within-state patterns in the data.

Short-run border mechanisms and potentially low impact. In the model, holding (M_S, M_N) fixed, increasing the SEPA wedge w_{iks} reduces the set of projects for which U_{iS} is the maximizer, lowering entry and slowing establishment/employment growth on the SEPA side (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991). Mirror-image gains on the non-SEPA side are *possible* but not automatic: entry in N rises only if a substantial mass of projects remains profitable and sufficiently mobile (low κ_i). If cancellation dominates relocation, or if profits depend importantly on local or regional thickness (high η and/or

ϕ), the adjacent county need not experience a commensurate boom (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017). These border-margin mechanisms also imply sharp heterogeneity: effects should be larger in permitting-intensive ("dirty") activity and among smaller establishments that face higher fixed compliance burdens per dollar of revenue, motivating Predictions 5–6. Appendix B provides formal statements for the border-mechanism claims above, corresponding to Predictions 1–2 below.

Longer-run dynamics: amenities versus agglomeration. The longer-run response is ambiguous because slow-moving forces can work in opposite directions. Improved environmental quality can raise amenities and local demand—especially for consumer-facing activity—potentially attenuating level differences over time (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). Conversely, because agglomeration forces are inherently local, an initial contraction can thin nearby supplier, labor, and customer networks and generate persistently low economic growth (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017). This motivates examining longer event-time windows and sectoral composition, as formalized in Prediction 3 below.

Within-state patterns: statewide effects and border amplification. Because SEPAs apply statewide, interior counties may respond even in the absence of cross-border substitution. Thus, similar movements in border and interior counties can reflect a statewide treatment effect of increased permitting frictions. The key additional question is whether effects are *amplified* near state borders, where cross-state relocation is most feasible (Levinson, 1996; Henderson and Millimet, 2005; Greenstone, 2002). We thus employ within-state comparisons to assess whether the border margin exhibits additional effects beyond the statewide component, consistent with cross-border substitution and/or cross-border network disruption, as formalized in Prediction 4 below.

2.2 Empirical Predictions

The discussion above yields a set of empirical predictions that triangulate mechanisms using *space* (border vs. interior; SEPA vs. non-SEPA states), *time* (short vs. long run), and *exposure* (industry pollution intensity and establishment size).

Prediction 1 (Border-pair effect on the treated side). If SEPA raises fixed and timing costs, outcomes in SEPA border counties should decline relative to their adjacent non-SEPA neighbors shortly after adoption (e.g., fewer establishments, slower employment growth, lower entry rates) (Becker and Henderson, 2000; Ryan, 2012; Pindyck, 1991). This is the core "treated-side" border discontinuity. Operationally, we estimate this as a post-adoption border-pair discontinuity between SEPA and adjacent non-SEPA counties.

Prediction 2 (Mirror-image gains next door indicate relocation). If relocation is quantitatively important, the adjacent non-SEPA border county should exhibit outperformance relative to comparable non-SEPA interior counties (a mirror-image gain) (Levinson, 1996; Henderson and Millimet, 2005). If instead deterrence/cancellation dominates, the non-SEPA side need not rise. Operationally, we test for mirror-image gains by comparing non-SEPA border counties adjacent to adopting states to non-SEPA interior counties within the same non-SEPA state.

Prediction 3 (Long-run dynamics: amenities vs. agglomeration). Over longer horizons, SEPAs can generate offsetting forces. If environmental review improves local environmental quality, amenities and local demand may rise over time—especially for consumer-facing activity—attenuating or reversing initial declines in business activity (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018). Alternatively, if local agglomeration forces are important, an initial contraction in entry and expansion can persist or widen as supplier, labor, and customer networks thin, reducing the profitability of subsequent projects and generating persistent scarring (Greenstone et al., 2010; Redding and Rossi-Hansberg, 2017). In this case, long-run losses need not be accompanied by mirror-image gains across the border, because persistence can arise even when displaced projects are canceled or delayed rather than relocated.

Prediction 4 (Border amplification conditional on cross-border integration). Because SEPAs apply statewide, interior counties may also respond through increased permitting frictions. Border areas can exhibit an *additional* response when cross-border substitution is feasible or when local activity relies on cross-border networks. Accordingly, effects should be more negative in counties that are both closer to the border and more integrated with the adjacent state (e.g., cross-border commuting zones or cross-state MSAs), and should attenuate with distance-to-border (Levinson, 1996; Henderson and Millimet, 2005; Greenstone, 2002). If impacts are instead primarily a statewide permitting channel with little cross-border component, border and interior responses should be more similar once conditioning on baseline economic structure.

Prediction 5 (Heterogeneity by regulatory exposure: dirty industries). If SEPA-induced procedural review raises fixed and timing costs primarily when environmental impacts and mitigation requirements are salient, effects should be stronger in pollution-intensive ("dirty") industries, where compliance may require mitigation, abatement investment, or process redesign (Jaffe et al., 1995; Dechezleprêtre and Sato, 2017; Hafstead and Williams III, 2018; Greenstone, 2002; Fowlie, 2010). Accordingly, following adoption, dirty-industry entry and employment should decline relatively more on the SEPA side of the border. Over longer horizons, composition may shift away from dirty activity even if cleaner sectors are comparatively resilient and/or benefit from amenity improvements (Chay and Greenstone, 2005; Isen et al., 2017; Shapiro and Walker, 2018; Addoum, Gounopoulos, Gustafson, Lewis, and Nguyen, 2023). Operationally, we implement this prediction by estimating border-pair event-time effects separately for dirty versus non-dirty industries (and/or by interacting the post-adoption indicator with an industry-level pollution-intensity measure).

Prediction 6 (Heterogeneity by fixed-cost burden: small establishments). 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 compliance 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 expect any overall decline in establishment counts and entry to be concentrated among small establishments, with muted effects for large incumbents, and potentially the strongest declines among "small-dirty" activity. Operationally, we test this by estimating effects by establishment size bins (and, where feasible, by interacting size with industry pollution intensity).

3 Identification Strategy

Section 2 delivers six empirical predictions about how SEPA adoption affects local business activity. Our identification strategy is built to estimate: (i) the post-adoption SEPA-side border discontinuity (Prediction 1), (ii) whether any SEPA-side losses are mirrored across the border in adjacent non-SEPA counties (Prediction 2), (iii) long-run level divergence consistent with amenity rebalancing versus agglomeration persistence (Prediction 3), (iv) whether the border margin exhibits additional effects beyond statewide permitting frictions (Prediction 4), and (v–vi) heterogeneity by industry pollution intensity and establishment size (Predictions 5–6). We implement these tests by exploiting staggered SEPA adoption and sharp contrasts at SEPA–non-SEPA state borders.

3.1 Border-Pair Design and Empirical Specification

Figure 1 maps the sixteen SEPA-adopting states and adoption years, which cluster in the early 1970s. For each SEPA adoption event, we form adjacent county pairs along the adopting state's borders with non-adopting states.[4] Because paired counties share local geography, infrastructure, and access to the same regional markets but face different permitting regimes once a SEPA becomes effective, changes in their relative outcomes isolate the local business impact of procedural review at the state line.

Baseline specification (Prediction 1). For outcome $Y_{i,j,t}$, for example the natural log of the total business establishments in county i within border pair j and year t, we estimate:

$$Y_{i,j,t} = \alpha_i + \gamma_{j,t} + \beta\big(SEPA_i \times Post_{j,t}\big) + \varepsilon_{i,j,t}, \tag{4}$$

where α_i are county fixed effects and $\gamma_{j,t}$ are border-pair-by-year fixed effects. $SEPA_i$ indicates the county lies on the adopting-state side of the pair, and $Post_{j,t}$ indicates years at or after the pair's adoption event. County fixed effects absorb permanent differences across counties, while border-pair-by-year fixed effects absorb all shocks common to the two counties in a given year (regional business cycles, shared labor market conditions, etc.). Identification therefore comes from within-pair changes in relative outcomes around adoption, which addresses similar econometric issues as the stacking procedures recommended in recent studies (Borusyak et al., 2024). Standard errors are clustered at the border-pair level.

We also employ dynamic specifications to trace the timing of effects and assess parallel trends. Here, we estimate stacked event-time specifications that replace $SEPA_i \times Post_{j,t}$ with a set of lead/lag indicators relative to adoption (normalized so $t = 0$ is the adoption year), retaining α_i and $\gamma_{j,t}$. We report pre-period coefficients and joint tests of pre-trends.

[4] Border relationships are many-to-many; we generate all cross-state adjacent pairs and retain unique combinations.

3.2 Estimation Windows and Tests Mapped to Predictions

We leverage this baseline specification by introducing long-run event windows, inner state county outcomes, and subsample analyses to directly test each of our empirical predictions.

Short-run impacts (Prediction 1). We estimate short-run effects on the percentage growth in local business activity (i.e., establishment or employee counts) over an eleven-year event window ($t-5$ to $t+5$). This window is designed to capture the immediate response to SEPA-induced fixed and timing costs emphasized by the deterrence/delay mechanism.

Mirror-image gains test (Prediction 2). To test whether SEPA-side losses reflect relocation rather than deterrence/cancellation, we examine outcomes in the adjacent non-SEPA border counties relative to interior counties within the same non-SEPA state. A positive border–interior differential on the non-SEPA side (a mirror-image gain) is consistent with relocation, while a flat profile is inconsistent with simple reallocation of SEPA-side losses.

Long-run divergence (Prediction 3). To assess whether early effects attenuate or persist, we estimate long-run divergence in levels by comparing outcomes in a five-year pre-period ($t-5$ to $t-1$) to a five-year "late post" window two decades after adoption ($t+20$ to $t+25$). Persistent effects without mirror-image gains are consistent with Prediction 3 manifesting via SEPAs leading to canceled projects rather than pure relocation.

Border amplification versus statewide permitting frictions (Prediction 4). Because SEPAs apply statewide, interior counties may also respond through increased permitting frictions. We therefore test whether the border exhibits *additional* effects beyond any statewide component by (i) comparing SEPA border counties to SEPA interior counties within the adopting state, and (ii) examining attenuation with distance-to-border. Border amplification—stronger effects near the state line that fade into the interior—is consistent with an added border margin (cross-border substitution and/or disruption of cross-border networks).

Dirty industries (Prediction 5). We implement Prediction 5 by estimating border-pair event-time effects separately for dirty versus non-dirty industries and/or by interacting the post-adoption indicator with an industry pollution-intensity measure. We focus on whether SEPA-side declines are disproportionately concentrated in pollution-intensive activity and whether composition shifts away from dirty industries over longer horizons.

Small establishments (Prediction 6). We implement Prediction 6 by estimating effects by establishment size bins (and, where feasible, interacting size with industry pollution intensity to isolate "small-dirty" exposure). This tests whether any overall decline in entry and establishment counts is concentrated among small establishments, consistent with a fixed-cost burden channel.

3.3 Sample and Evidence on Identifying Assumptions

Our primary outcome data come from the *County Business Patterns* (CBP), assembled into a consistent county-level panel spanning 1965–2020 to cover early SEPA adoption and multi-decade post-adoption horizons. SEPA adoption dates are taken from statutory enactment years. Border adjacency is defined using county boundary contiguity; for each adopting state and adoption event, we generate all cross-state adjacent county pairs where the neighbor lies in a non-adopting state. Full details on pairing, weights, variable construction, and consistency checks are provided in Appendix E.

Figure 2 highlights the resulting sample after restricting to border-pair counties. As of one year before SEPA adoption, the final 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.

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. Table A.I indicates no economically or statistically significant pre-existing differences in measures of overall economic scale between SEPA treated counties and those across the border, including in terms of the natural logarithm of establishments and employees and the firm size and industry distributions.

More importantly, Figure 3 indicates little evidence of pre-existing differential trends in establishment growth rates. Figure A.V further 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.

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 4 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 4, 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 Banking as a Transmission Channel of SEPA-Induced Agglomeration Weakening

Panel A of Table 5 examines how SEPA adoption reshapes local banking structure and whether pre-existing banking depth moderates the decline in establishments. Columns (1)–(2) show that the share of local (state-chartered) banks in total banks increases significantly on the SEPA side following adoption. In the short run, the local-to-total bank ratio rises by roughly 2 percentage points, and the effect grows to over 20 percentage points in the long-run comparison window. These results indicate a relative retrenchment of nationally chartered banks and a compositional shift toward locally embedded financial institutions in SEPA-affected counties.

Columns (3)–(4) further show that the number of national banks does not decline immediately but falls significantly in the long run. The absence of a short-run response, coupled with a sizable long-run contraction, is consistent with gradual adjustment on the extensive margin: national banks do not immediately exit SEPA counties but reduce their presence over time as regulatory frictions persist and project pipelines weaken. Because national banks are more integrated into interregional capital markets and often serve larger, tradable-sector borrowers, their long-run retrenchment suggests that SEPA weakens cross-regional financial integration alongside industrial activity.

Column (5) links pre-existing banking depth directly to establishment outcomes. The positive and significant coefficient on *Post* $\times$ *Treated* $\times$ *LnNatlBankPre* indicates that counties with a larger pre-period national-bank presence experience smaller post-SEPA declines in establishments. In other words, deeper pre-existing financial intermediation mitigates the economic contraction associated with SEPA adoption. This finding aligns closely with the agglomeration framework: where financial networks are thicker and more developed, firms are better able to absorb procedural delays and fixed compliance costs. Conversely, in financially thinner counties, the same regulatory shock produces larger contractions.

Taken together, Panel A provides evidence that SEPA's economic effects operate not only through direct project deterrence but also through the erosion and restructuring of local financial networks. The relative rise of local banks and the gradual retreat of national banks indicate that financial intermediation becomes more localized following SEPA adoption. At the same time, the moderating role of pre-existing national-bank depth underscores that agglomeration economies include financial integration: stronger financial networks dampen the adverse real effects of regulatory frictions. These results reinforce the interpretation that SEPA weakens agglomeration forces in border regions, with finance acting as a key transmission channel.

Panel B of Table 5 examines how SEPA adoption affects consumer credit markets and whether local banking composition mediates these effects. Columns (1)–(2) report estimates for the logarithm of credit card lending, while Columns (3)–(6) focus on consumer loans. Across specifications, the coefficient on *Post* $\times$ *Treated* is negative and statistically significant, indicating that credit card and consumer lending decline on the SEPA side relative to matched non-SEPA counties. These reductions mirror the contraction in establishments and employment documented earlier and suggest that SEPA-induced frictions are accompanied by a tightening of local credit conditions.

The interaction with the local-bank ratio further illuminates the financial channel. In the credit card regressions, the positive and statistically significant coefficient on *Post* $\times$ *Treated* $\times$ *LoclBankRatio* implies that counties with a larger local-to-national bank ratio experience a smaller post-SEPA contraction in credit card lending. In other words, greater reliance on locally embedded financial institutions mitigates the decline in consumer credit. This buffering effect is consistent with relationship-based lending models, in which local banks—owing to superior soft information and closer borrower ties—are better positioned to continue lending during periods of regulatory disruption. By contrast, nationally integrated banks may retrench more quickly in response to increased uncertainty or reduced project flow.

The consumer-loan results exhibit a similar directional pattern, although the magnitude and statistical strength are somewhat weaker in the long-run window. Taken together, the evidence suggests that SEPA adoption not only suppresses industrial activity but also reshapes local financial intermediation. Where national banks dominate, credit contractions are larger, reinforcing the real-side downturn. Where local banks are more prevalent, financial relationships cushion the shock and slow the erosion of economic activity.

These findings strengthen the agglomeration interpretation advanced throughout the paper. Agglomeration economies encompass not only supplier networks and labor-market pooling but also local financial linkages. By weakening project pipelines and increasing regulatory uncertainty, SEPA disrupts both industrial and financial integration. The extent of local credit-market resilience therefore determines how strongly the regulatory shock translates into sustained economic contraction, underscoring finance as a key transmission channel of agglomeration weakening.

4.3 Heterogeneity Across States

The patterns in Table 2 mask substantial variation across SEPA-adopting states. Figure 5 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 6 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 5 and Table 6 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 6 and Tables 7 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 8 and 9, 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 6 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 7 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 6 and Tables 7–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.

5.1.1 Financial Structure and Retail Reallocation

Table A.IV examines whether pre-existing financial structure shapes the retail-sector reallocation documented above. Specifically, we interact SEPA exposure with *LnNatlBankPre*, defined as the natural logarithm of the average number of national banks in a county during the five-year pre-adoption window (-5 to -1 years relative to SEPA enactment). This measure captures the extent to which local credit markets were historically integrated with nationally chartered banking institutions prior to the regulatory shock.

The estimates reveal a negative and statistically significant coefficient on the triple interaction term *Post* $\times$ *Treated* $\times$ *LnNatlBankPre* in both short- and long-run specifications. While SEPA adoption is associated with an overall increase in the retail share of establishments (positive *Post* $\times$ *Treated*), this increase is substantially attenuated in counties with greater pre-existing national-bank presence. In other words, the post-SEPA rise in retail and restaurant activity is strongest in counties that were less financially integrated prior to adoption and weaker in counties with deeper national banking networks.

This pattern is consistent with a financial-intermediation channel operating alongside the amenity mechanism. Retail expansion following SEPA appears to occur largely through new entry into local, non-tradable sectors. Such entrants may lack established credit relationships and rely on locally embedded financial institutions rather than nationally integrated banks that primarily serve incumbent or capital-intensive borrowers. In counties where national banks dominated prior to SEPA, the retail reallocation is muted, suggesting that existing financial structures can slow compositional adjustment even when environmental regulation shifts local demand toward cleaner, consumer-facing sectors. These results reinforce the broader agglomeration framework: SEPA not only reshapes industrial composition, but does so in ways mediated by the depth and structure of local financial networks.

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 8 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 9 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 8 and 9 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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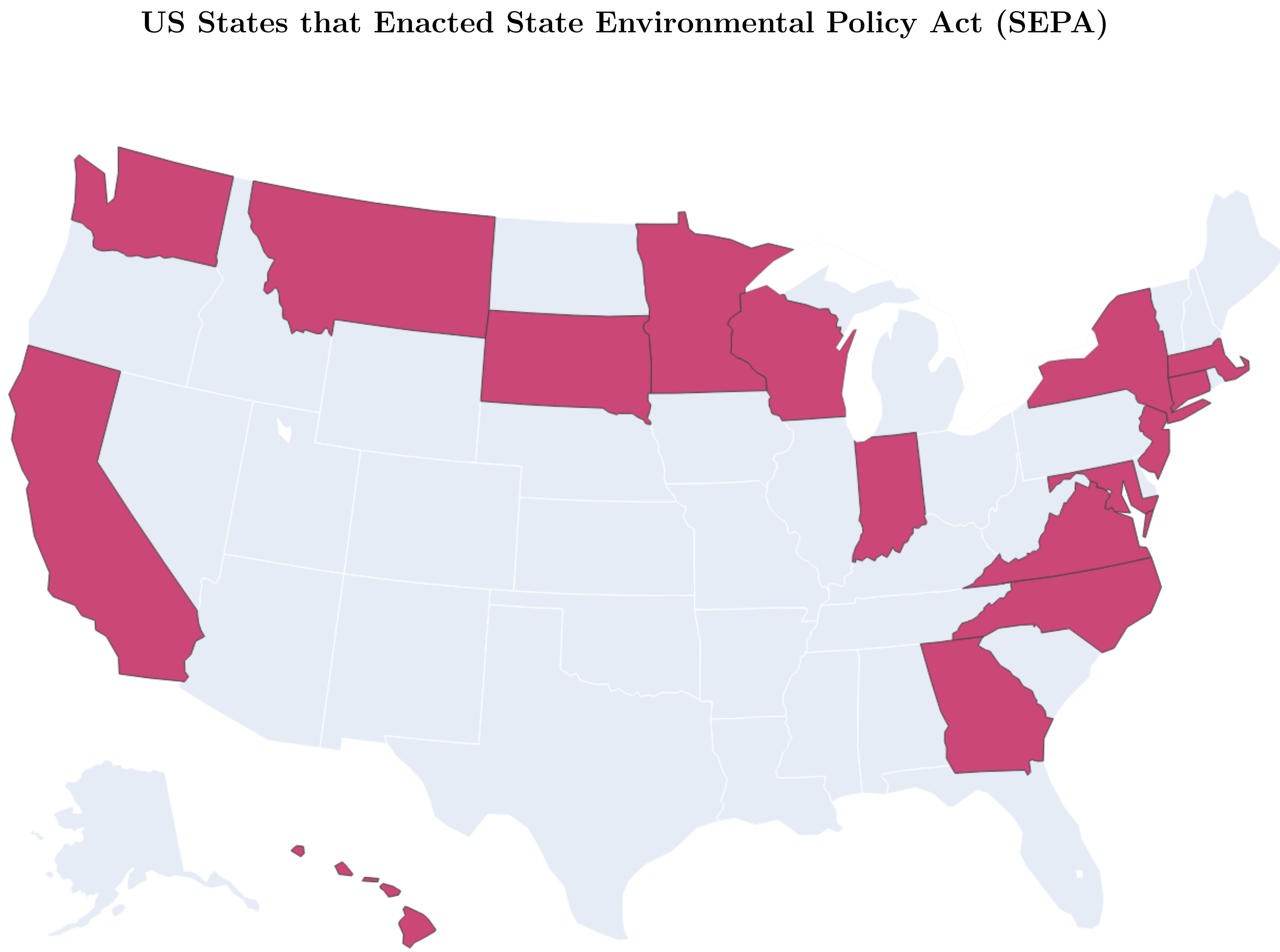


Figure 1: US States that Enacted State Environmental Policy Acts (SEPAs). This figure shows the sixteen U.S. states that have passed a SEPA. These states, with their year of enactment in parentheses, are: California (1970), Connecticut (1973), Georgia (1991), Hawaii (1974), Indiana (1972), Maryland (1973), 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.

SEPA and Non-SEPA Border-Pair Counties.

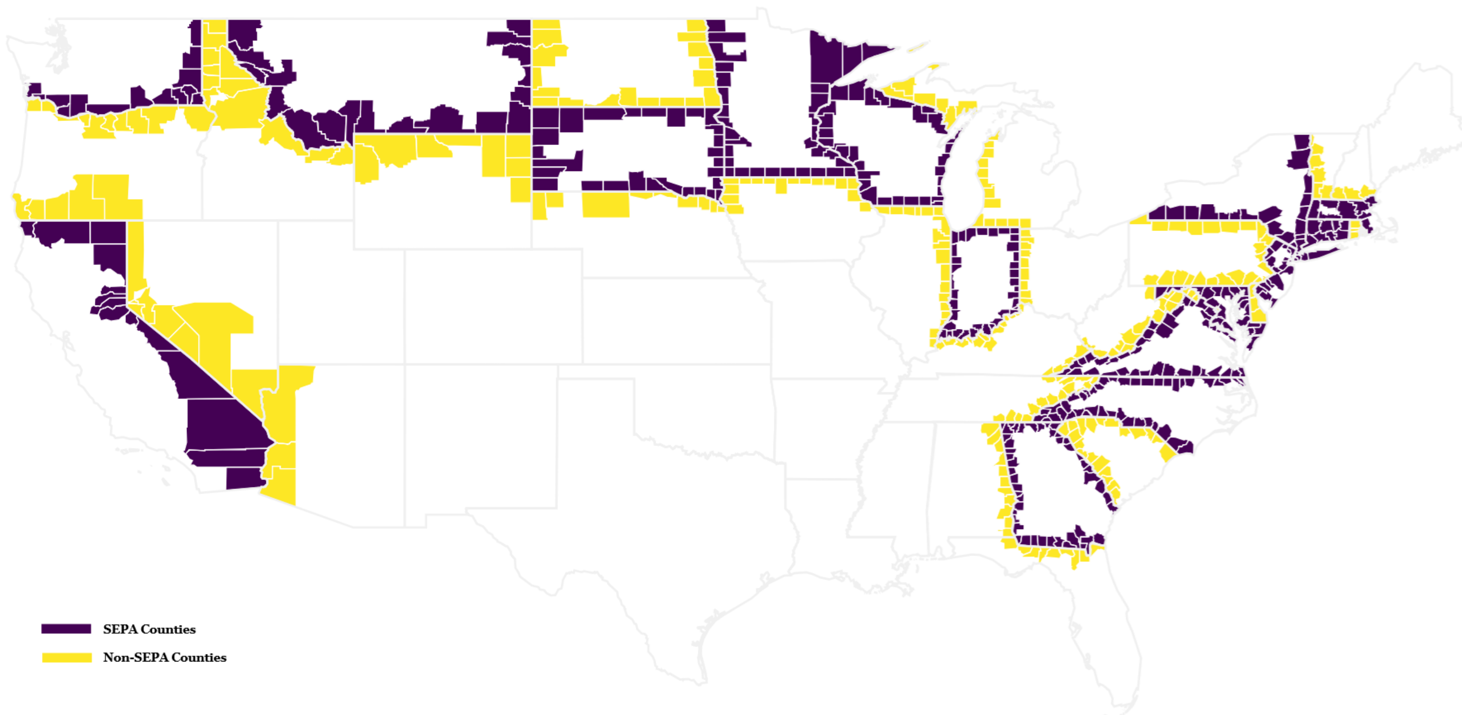
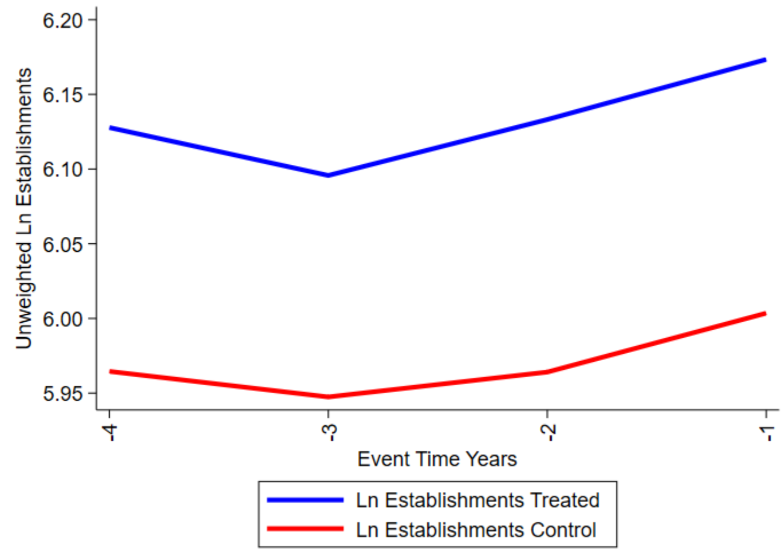
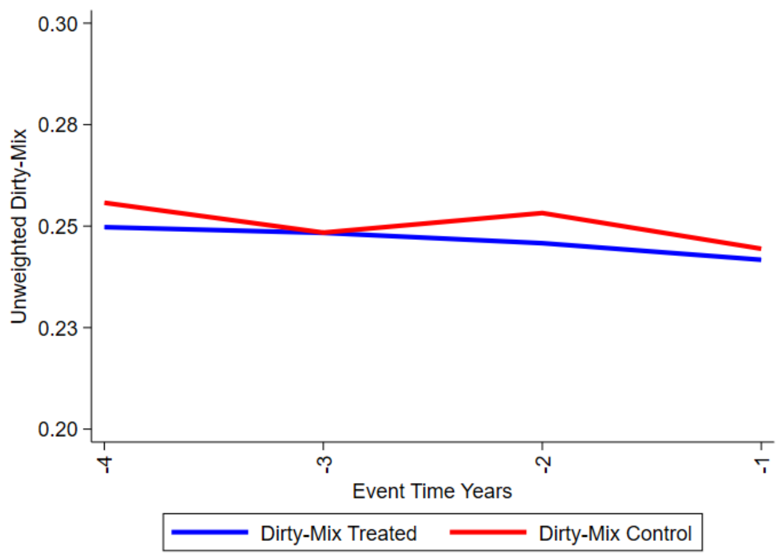


Figure 2: SEPA and Non-SEPA Border-Pair Counties. This figure illustrates our border pair matching strategy. The sample consists of all counties in a SEPA-adopting state that share a border with a county in a non-SEPA state (shown in purple), and those corresponding non-SEPA border counties (shown in 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 3: **Pre-Trends in Establishment Counts and their 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

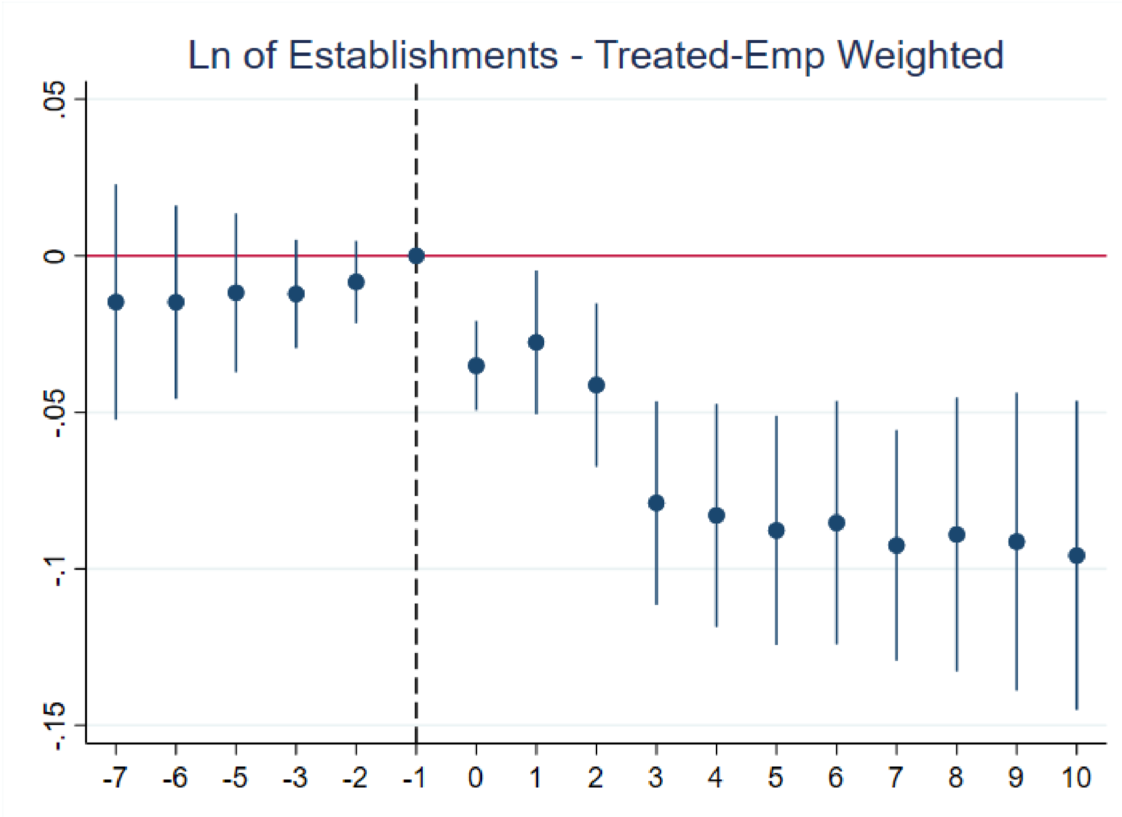


Figure 4: Event-Study Analysis of SEPA Enactment on the Number of Establishments. The figure shows the estimated year-by-year effect of a SEPA enactment on the log number of business establishments in treated border counties relative to their control-county neighbors. The model includes county-pair-by-year 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

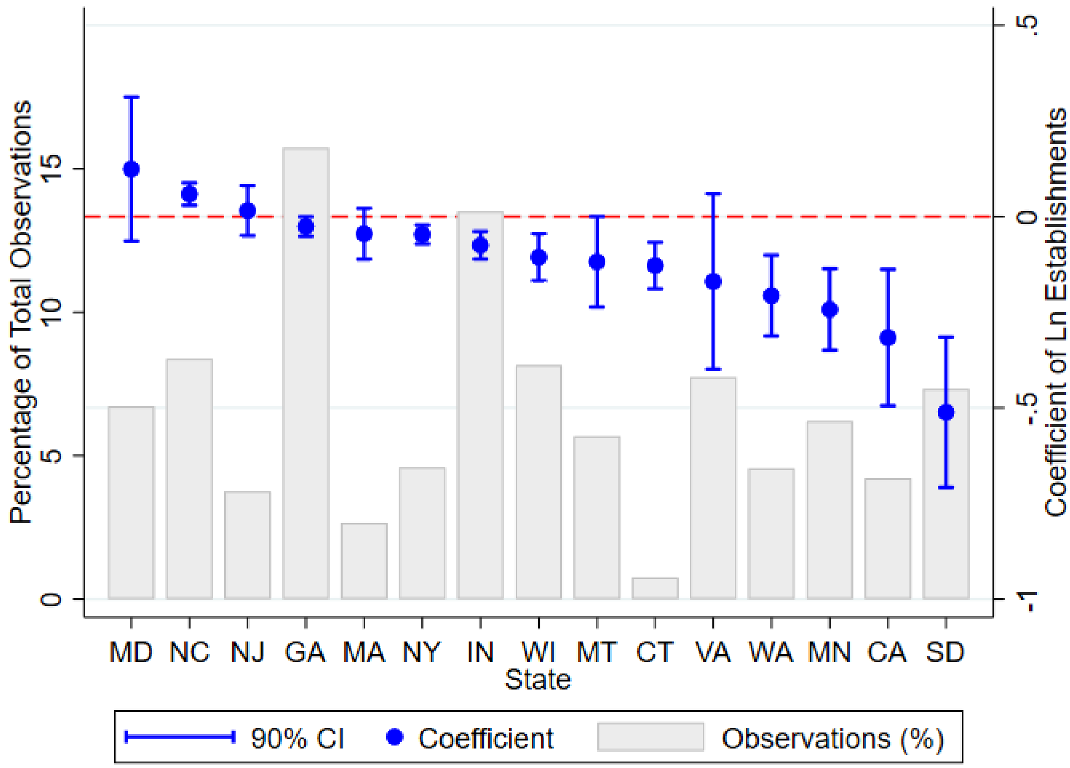


Figure 5: Heterogeneity in the Short-Run Impact of SEPA Adoption by State. This figure disaggregates the average treatment effect by plotting the state-specific DiD coefficient for the log number of establishments from our short-run (-5, +5 year) specification. The blue dots show the point estimate for 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

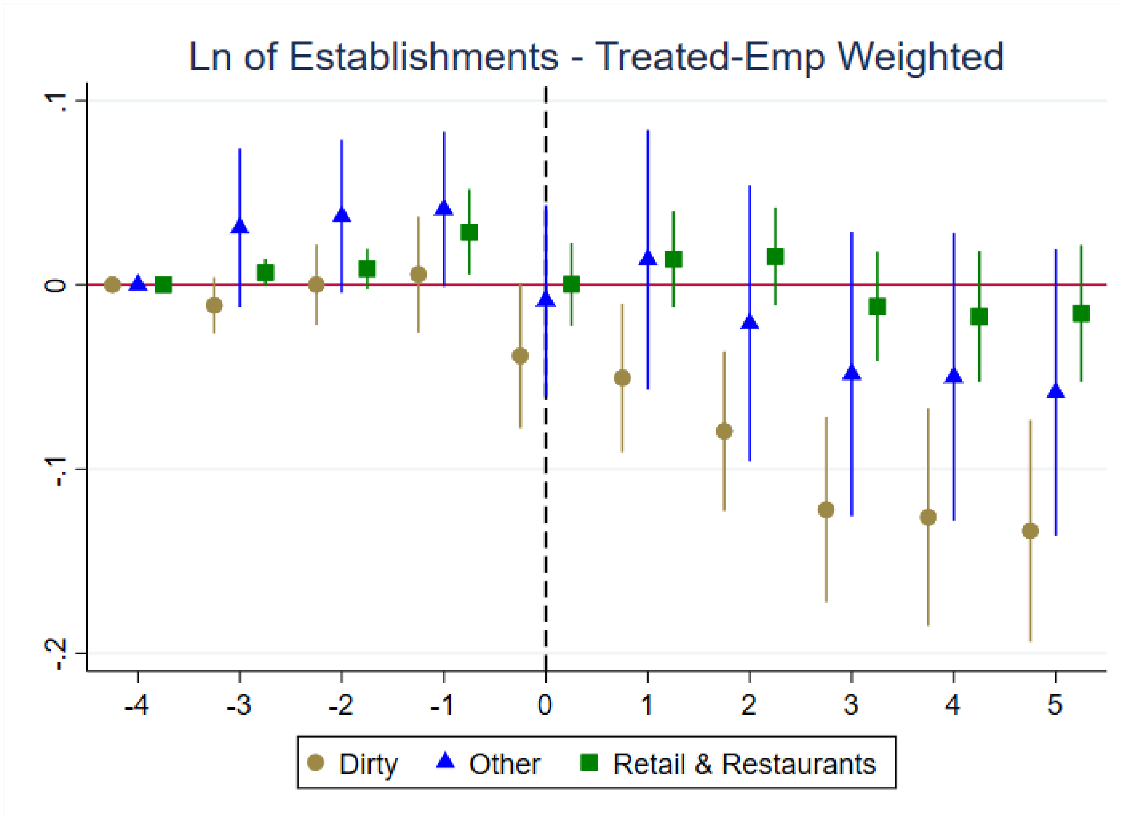


Figure 6: Heterogeneous SEPA Effects: Event Study by Industry Cohort. This figure disaggregates the main treatment effect by plotting the dynamic DiD coefficients for three industry cohorts: ‘Dirty,’ ‘Retail & Restaurants,’ and ‘Other’ (all non-dirty, non-retail firms). The results show a clear ordering 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: **Banking as a Channel of Agglomeration Effects.**
This table examines whether local banking structure mediates the economic effects of SEPA adoption. Panel A reports the impact of SEPA on banking composition and national-bank presence. Columns (1)–(2) use the ratio of local (state-chartered) banks to total banks as the dependent variable, while Columns (3)–(4) use the number of national banks. Column (5) relates pre-existing national-bank presence to establishment dynamics. *LnNatlBankPre* is the natural logarithm of the average number of national banks in a county during the five-year pre-adoption period (years -5 to -1 relative to SEPA enactment). Panel B examines credit-market responses using the logarithm of credit card loans and consumer loans as dependent variables. *LoclBankRatio* is the ratio of local to national banks in a county-year. The interaction terms test whether pre-existing banking depth and contemporaneous banking composition shape the post-SEPA response of local business activity and credit provision. Columns labeled $(-5, +5)$ estimate short-run effects, while columns labeled $(-5, -1)$ & $(+20, +25)$ compare pre-adoption averages to long-run post-adoption averages. All regressions include county fixed effects and county-pair-by-year fixed effects. 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. Standard errors are clustered at the county-pair level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Local and National Banks Dynamics

	(1)	(2)	(3)	(4)	(5)
Dep Var:	Local to Total Bank Ratio		Num of National Banks		Ln Estabs
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)
Post × Treated	0.020**	0.215***	1.963	-17.569***	-0.194***
	(2.159)	(3.271)	(0.234)	(-3.558)	(-2.798)
Post × LnNatlBankPre					-0.094***
					(-3.188)
Post × Treated × LnNatlBankPre					0.065***
					(2.613)
CntyPair#Year FE	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES
Observations	8,742	10,072	8,742	10,072	8,570

Panel B: Credit Card and Consumer Loan Dynamics

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var:	Log of Credit Card Loans			Log of Consumer Loans		
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)	(-5,+5)	(-5,-1) & (+20,+25)	(-5,+5)
Post × Treated	-1.286*	-10.410**	-1.883*	-1.628**	-11.741**	-1.805
	(-1.691)	(-2.219)	(-1.685)	(-2.081)	(-2.018)	(-1.616)
Post × Treated × LoclBankRatio			11.583*			8.251
			(1.898)			(1.292)
LoclBankRatio			-2.440			-6.133*
			(-1.163)			(-1.748)
Post × LoclBankRatio			-7.656**			-2.107
			(-1.986)			(-0.482)
Treated × LoclBankRatio			-10.373			-10.042
			(-1.292)			(-1.193)
CntyPair#Year FE	YES	YES	YES	YES	YES	YES
Cnty FE	YES	YES	YES	YES	YES	YES
Observations	8,742	10,072	8,742	8,742	10,072	8,742

Table 6: **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 7: **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 8: **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 9: **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

A Background on State Environmental Review (SEPAs)

This appendix provides institutional detail on environmental review frameworks to complement the brief discussion in Section 2. The goal is descriptive: to clarify why SEPAs can plausibly operate as routine local permitting frictions for private businesses, and to motivate the mapping from institutions to the reduced-form wedge w_{iks} in the conceptual framework.

A.1 NEPA: A procedural, federally triggered, review framework

NEPA is primarily procedural rather than substantive: it requires federal agencies to evaluate and disclose the environmental impacts of major federal actions (Mandelker et al., 2024). In practice, NEPA review is typically triggered by an interaction with the federal government such as with federal land, permits, or funding. Thus, many purely local private projects do not trigger NEPA directly.

If NEPA engagement occurs, it is typically tiered. Some actions are categorically excluded; other actions undergo an Environmental Assessment (EA) to determine whether impacts are significant. If impacts are not significant, the agency issues a Finding of No Significant Impact (FONSI). Otherwise, the agency prepares a more extensive Environmental Impact Statement (EIS), including analysis of alternatives, mitigation, and public comment periods. These steps can impose both direct compliance costs (technical studies, consultants, legal documentation) and indirect costs through timing and uncertainty, including the possibility of delay, redesign, or litigation (Ryan, 2012; Pindyck, 1991).

A.2 SEPA: Mini-NEPAs integrated into state and local permitting

Following NEPA, many states adopted State Environmental Policy Acts (SEPAs), which extend NEPA-style procedural review to state and local government actions and, in many states, to private projects that require state or municipal approvals, zoning actions, or public support. The defining feature for our setting is scope: whereas NEPA depends on federal involvement, SEPAs can attach procedural review to routine state and local permitting environments, where everyday business entry and expansion decisions occur.

From the perspective of private businesses, SEPAs can generate two broad classes of frictions. First, they can raise fixed compliance costs through documentation and review requirements (consultants, legal, technical studies). Second, they can raise timing and uncertainty costs through longer review horizons, opportunities for public participation, mitigation negotiation, and the possibility of administrative appeal or suit (Ryan, 2012; Pindyck, 1991). These frictions are naturally summarized in the conceptual framework by the wedge w_{iks}, which we allow to vary by project type and size (e.g., larger for permitting-intensive activities and smaller firms).

A.3 Illustrative mechanics: CEQA-style workflow and examples

To make these mechanisms concrete, we provide several representative illustrations from CEQA (California's SEPA-style framework). Figures A.I and A.II summarize the basic workflow: an initial screening phase (project definition, exemptions, and initial study) followed, when required, by a more extensive review pathway that includes draft document preparation, circulation, public comment, responses, and final certification/approval. Each stage can require administrative effort and introduce delay, and the public-facing nature of the process can create additional uncertainty for time-sensitive investments.

Figure A.III provides an example page listing potential impacts and associated mitigation measures, illustrating how procedural review can translate into concrete conditions that must be satisfied before a project proceeds. Figure A.IV illustrates the participatory nature of review via a public-hearing notice. Together, these examples show how review frameworks can introduce fixed costs, extend timelines, and elevate uncertainty for private projects whose permitting would otherwise be routine.

A.4 Implications for empirical interpretation

The institutional scope of SEPAs motivates our empirical design in two ways. First, because SEPAs can attach to routine local approvals, they may affect entry and expansion decisions across a broad set of establishments, not only large projects. Second, because relocation costs can be low near state borders, border-county comparisons provide a tight laboratory for identifying border-margin responses (deterrence/delay versus relocation) while also allowing within-state comparisons to assess whether border counties exhibit additional effects beyond any statewide component.

B Formal statements for Predictions 1–2

This appendix provides simple formal statements that correspond to the two short-run, border-margin predictions in the main text. For notational simplicity, we fix (M_S, M_N) and suppress type indices (k, s) where they are not needed.

B.1 Setup and choice rule

Each project i compares payoffs from locating in the SEPA county S, the adjacent non-SEPA county N, and not entering (outside option normalized to 0):

$$U_{iS}(w) = \pi_S(M_S, M_N) + \varepsilon_{iS} - c_i - w, \tag{5}$$

$$U_{iN} = \pi_N(M_S, M_N) + \varepsilon_{iN} - c_i - \kappa_i. \tag{6}$$

The project chooses S iff $U_{iS}(w) \geq \max\{U_{iN}, 0\}$, chooses N iff $U_{iN} \geq \max\{U_{iS}(w), 0\}$, and otherwise does not enter.

Let $\mathbf{1}_{iS}(w) = \mathbf{1}\{U_{iS}(w) \geq \max\{U_{iN}, 0\}\}$ and $\mathbf{1}_{iN}(w) = \mathbf{1}\{U_{iN} \geq \max\{U_{iS}(w), 0\}\}$. Expected entry is

$$E_S(w) = \Lambda\, \mathbb{E}[\mathbf{1}_{iS}(w)], \qquad E_N(w) = \Lambda\, \mathbb{E}[\mathbf{1}_{iN}(w)],$$

where the expectation is taken over the project population.

B.2 Prediction 1: the SEPA-side response is monotone

PROPOSITION 1 (Monotone decline in SEPA-side entry (Prediction 1))**.** *For any distribution of project characteristics $(\varepsilon_{iS}, \varepsilon_{iN}, c_i, \kappa_i)$, expected entry into the SEPA county, $E_S(w)$, is weakly decreasing in the SEPA wedge w. In particular, if $w' > w$, then $E_S(w') \leq E_S(w)$.*

Proof. Fix a project i and consider two wedges $w' > w$. From (5), $U_{iS}(w') = U_{iS}(w) - (w' - w) \leq U_{iS}(w)$, while U_{iN} and the outside option 0 do not depend on w. If i chooses S under w', then $U_{iS}(w') \geq \max\{U_{iN}, 0\}$. Since $U_{iS}(w) \geq U_{iS}(w')$, it follows that $U_{iS}(w) \geq \max\{U_{iN}, 0\}$, so i also chooses S under w. Hence $\mathbf{1}_{iS}(w') \leq \mathbf{1}_{iS}(w)$ for every i. Taking expectations and multiplying by Λ yields $E_S(w') \leq E_S(w)$. □

B.3 Prediction 2: mirror-image gains require mobile, viable projects

PROPOSITION 2 (Mirror-image gains are possible but not guaranteed (Prediction 2))**.** *An increase in the SEPA wedge w need not increase expected entry into the adjacent non-SEPA county, $E_N(w)$.*

Proof. We provide a simple environment in which $E_S(w)$ falls (by Proposition 1) but $E_N(w)$ does not rise. Suppose $U_{iN} < 0$ for all projects (e.g., κ_i is sufficiently large or π_N is sufficiently low). Then U_{iN} is never the maximizer against $\{U_{iS}(w), 0\}$, so $\mathbf{1}_{iN}(w) = 0$ for all i and all w, implying $E_N(w) = 0$ for all w. Yet $E_S(w)$ is weakly decreasing in w and will fall strictly if a positive mass of projects is marginal on the SEPA side. Thus $E_N(w)$ need not increase when w rises. $\square$

PROPOSITION 3 (Sufficient condition for mirror-image gains)**.** *Fix $w' > w$. Suppose there exists a subset of projects $\mathcal{R}$ with positive measure such that for each $i \in \mathcal{R}$: (i) $U_{iS}(w) \geq \max\{U_{iN}, 0\}$ (the project chooses S under w); (ii) $U_{iN} \geq 0$ (the project is viable in N); (iii) $U_{iN} > U_{iS}(w')$ (after the wedge increases, N strictly dominates S). Then $E_N(w') > E_N(w)$.*

Proof. Take any $i \in \mathcal{R}$. Conditions (ii)–(iii) imply $U_{iN} \geq \max\{U_{iS}(w'), 0\}$, so i chooses N under w' (up to ties). Condition (i) implies i chooses S under w and therefore does not choose N under w. Hence $\mathbf{1}_{iN}(w') - \mathbf{1}_{iN}(w) = 1$ for each $i \in \mathcal{R}$. Because $\mathcal{R}$ has positive measure, $\mathbb{E}[\mathbf{1}_{iN}(w')] > \mathbb{E}[\mathbf{1}_{iN}(w)]$, and multiplying by Λ yields $E_N(w') > E_N(w)$. $\square$

Interpretation. Proposition 1 formalizes Prediction 1: a higher SEPA wedge reduces entry on the treated side. Propositions 2–3 formalize Prediction 2: mirror-image gains in adjacent non-SEPA counties arise only when a nontrivial mass of projects is both (a) displaced from S by the wedge and (b) sufficiently mobile and viable in N.

Appendix C 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 manufacturing (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 D Additional Plots & Tables

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

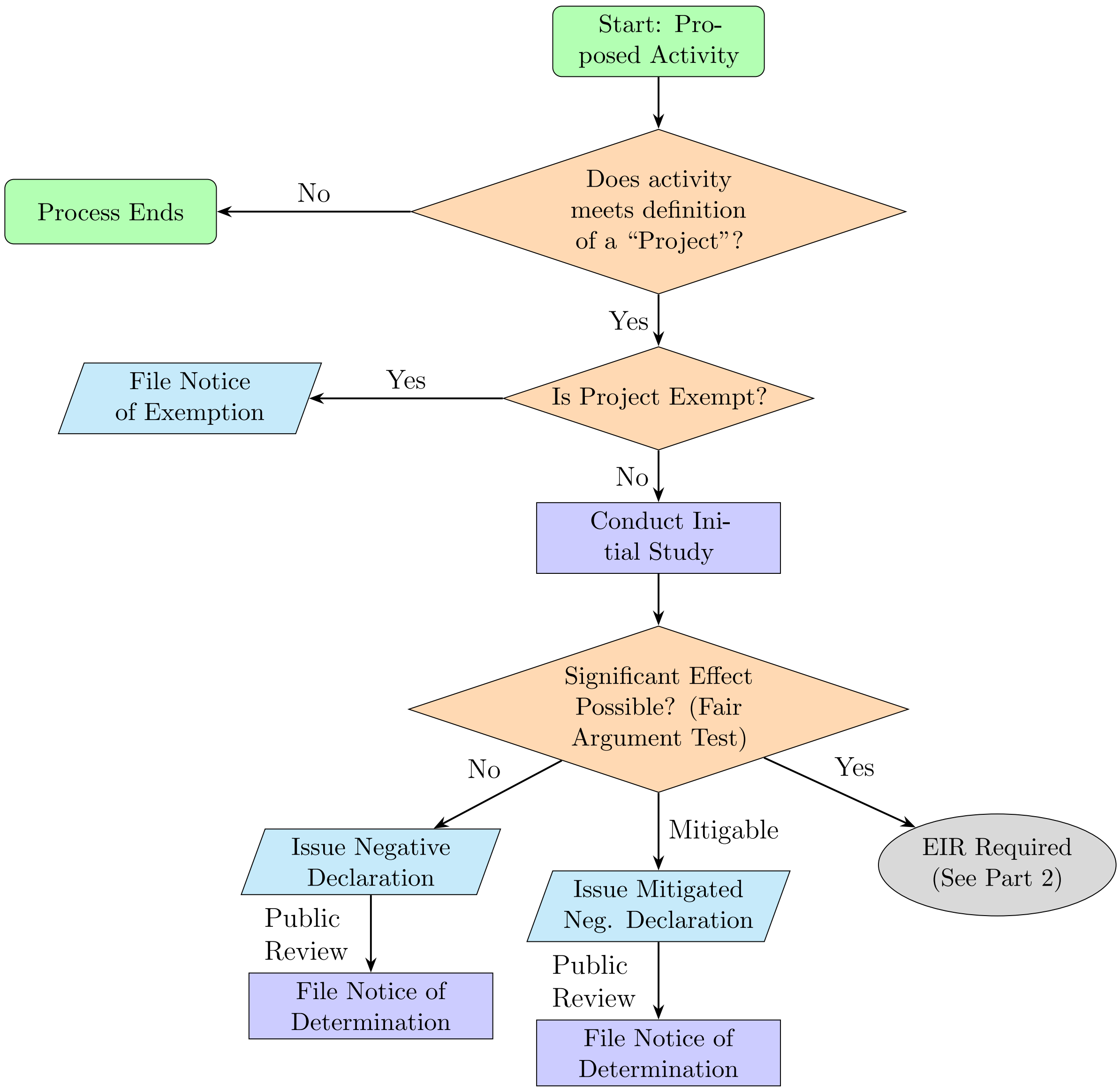


Figure A.I: California SEPA (CEQA) Process Flowchart (Part 1): Initial Screening and Streamlined Paths. Taking California's SEPA as an example, this flowchart details the initial screening process for a proposed project. An activity is first evaluated to determine if it meets the legal definition of 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

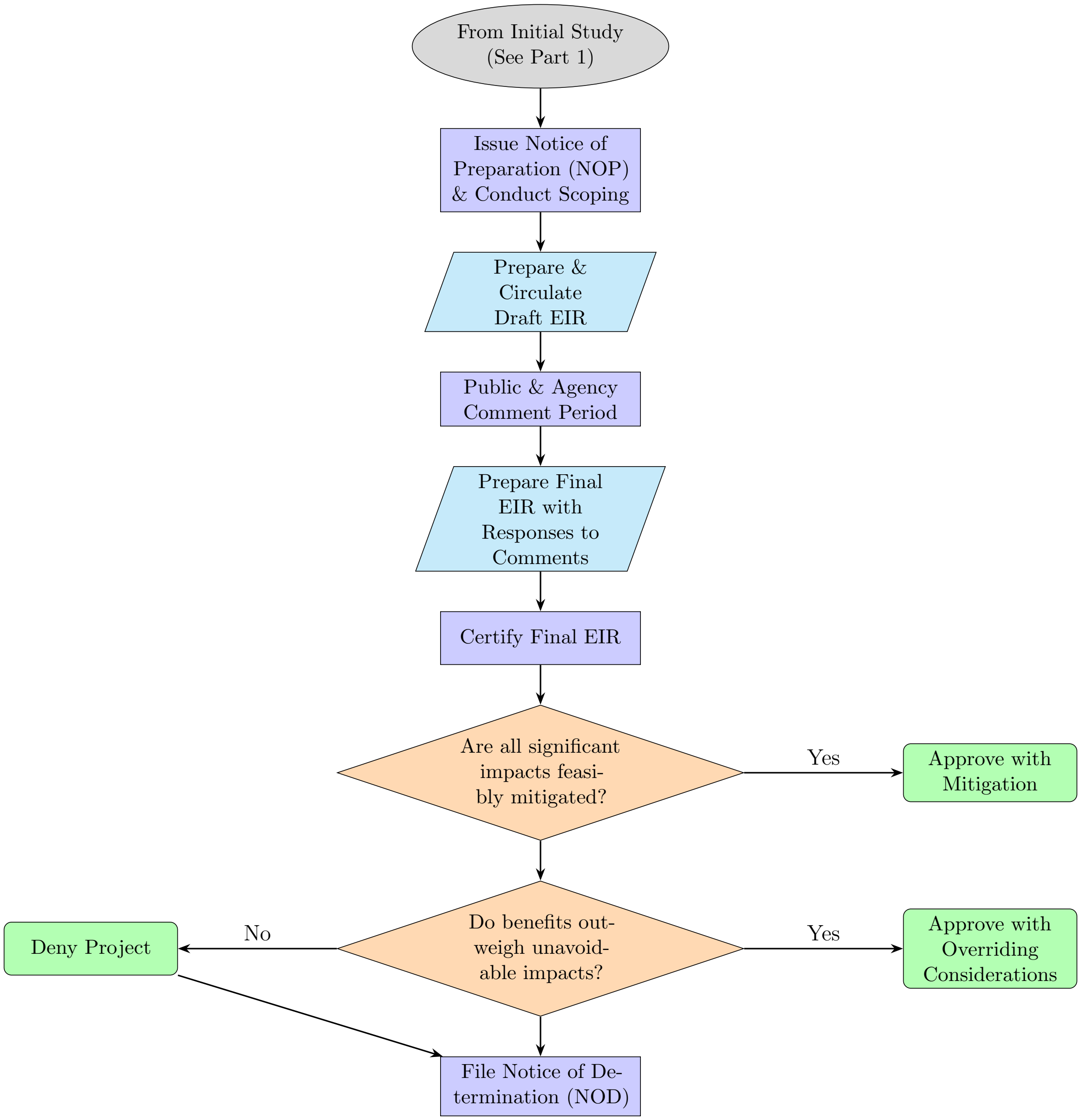


Figure A.II: California SEPA (CEQA) Process Flowchart (Part 2): The Environmental Impact Report (EIR) Track. Taking California's SEPA as an example, this flowchart details the comprehensive EIR process that is required once a project is determined to have a potentially significant environmental effect. 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.

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.III: **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
- ☒ Within a 500-Foot Radius
- ☐ Abutting a Proposed Development Site

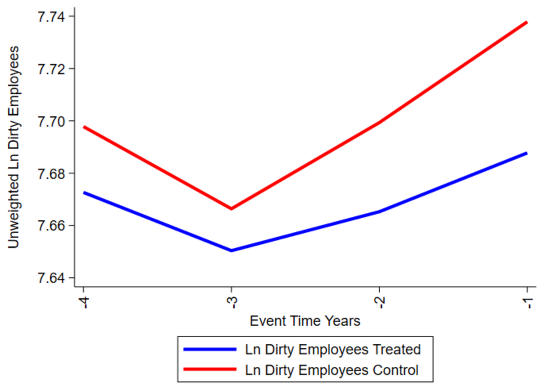
And Occupants:
- ☐ Within a 100-Foot Radius
- ☒ Within a 500-Foot Radius

And:
- ☒ Interested Parties/Others

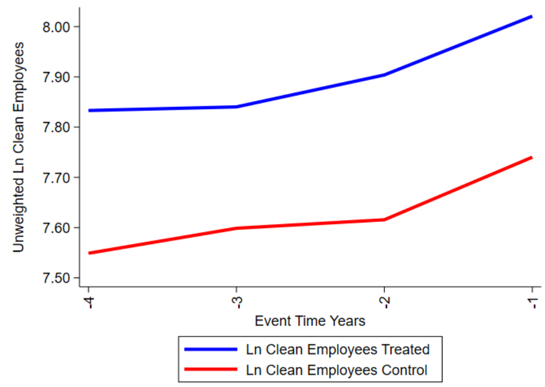
This notice is sent to you because you own property or are an occupant residing near a site for which an application was filed with the Department of City Planning. All interested persons are invited to attend the public hearing where you may listen, ask questions, and/or present testimony regarding 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

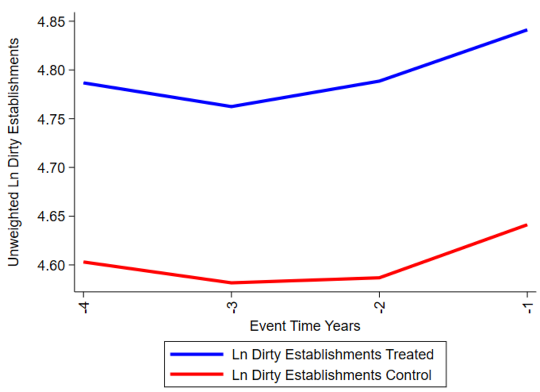
Figure A.IV: **Example Notification for a Public Hearing Under CEQA.** This figure shows a standard "Notice of Public Hearing" for a proposed hotel project in Los Angeles, California. The notice illustrates the broad public engagement mandated by the SEPA process. Critically, notification is not limited 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.



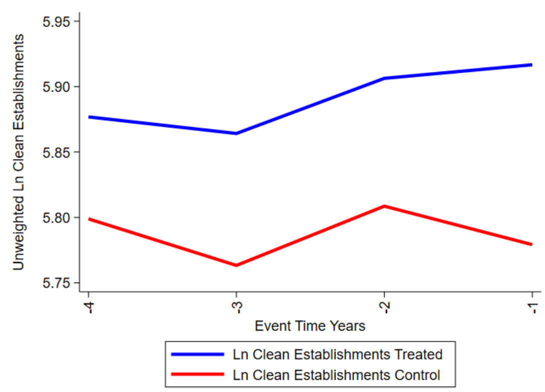
(a) Dirty Employees



(b) Clean Employees



(c) Dirty Establishments



(d) Clean Establishments

Figure A.V: **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.

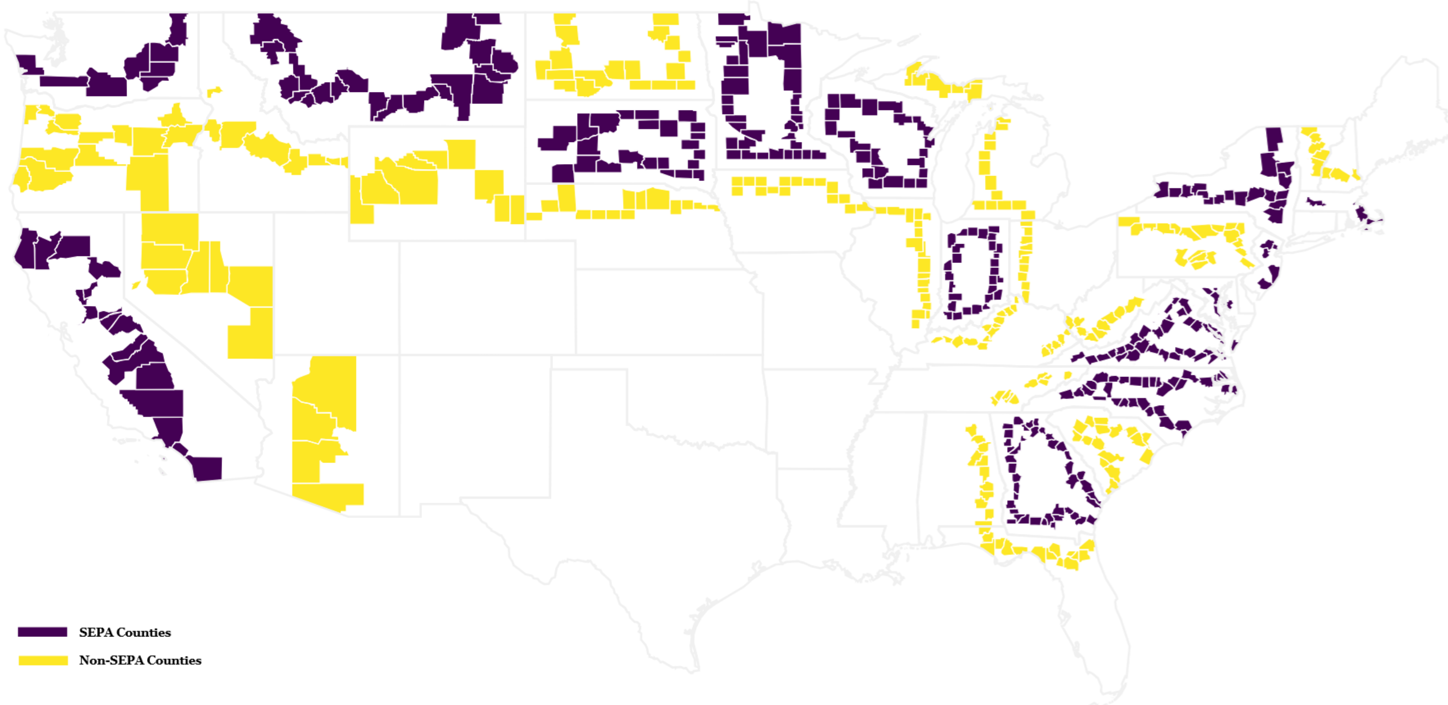


Figure A.VI: **SEPA vs. Non-SEPA Inner-State County Groups.** This figure illustrates the construction of the "inner-state" county groups used for the placebo-style robustness tests designed to assess the spatial reach of SEPA effects. The example uses counties along the Indiana–Illinois border. Clay 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.

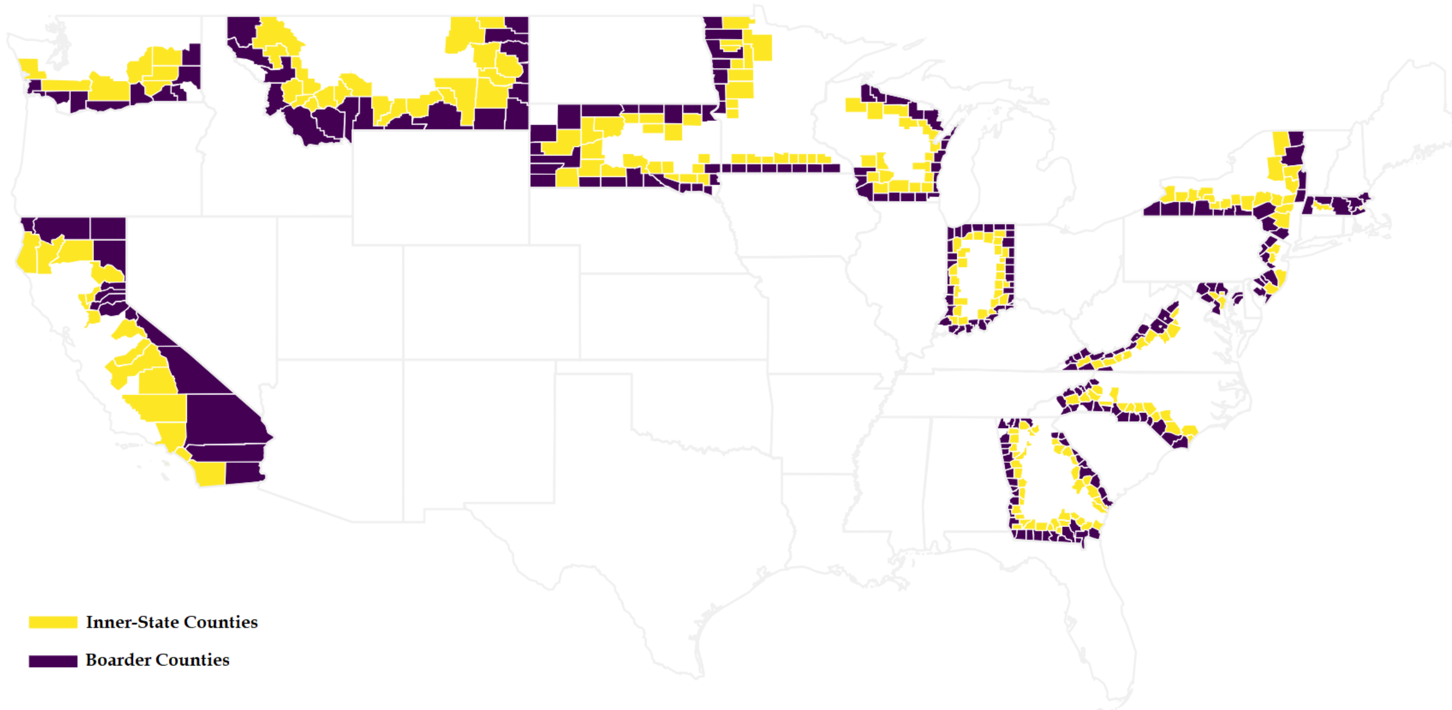


Figure A.VII: **Within-SEPA State County Pairs.** This figure illustrates the construction of the within-state county pairs used to examine whether SEPA impacts are concentrated near the state border or diffuse throughout SEPA-adopting states. The example uses Indiana, a SEPA state. The yellow-shaded 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.

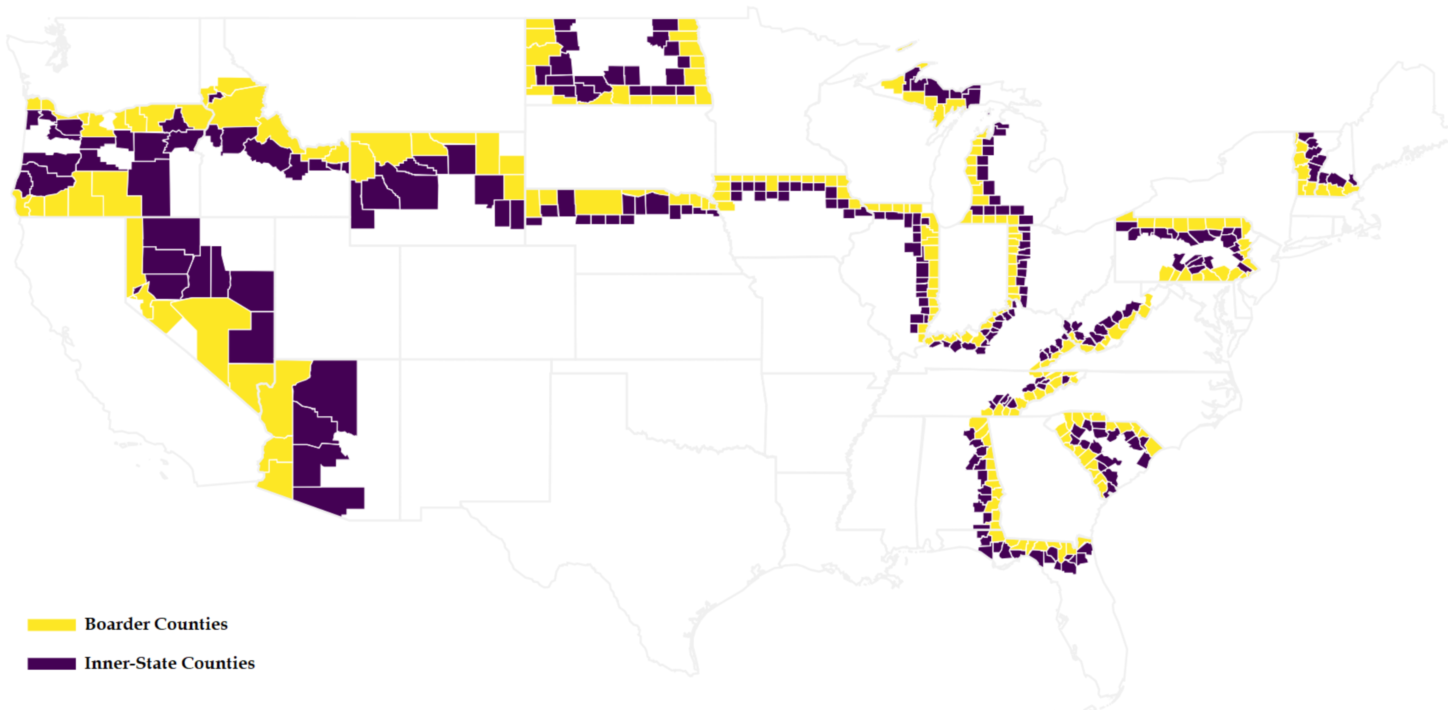


Figure A.VIII: **Within-Non-SEPA State County Pairs.** This figure illustrates the construction of the within-state county pairs used for the placebo analysis in non-SEPA states. The example uses Illinois, a non-SEPA state bordering SEPA-adopting Indiana. The yellow-shaded counties (Clark and Crawford) 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.VII, 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: **Pre-existing Level of National Banks and Retail Mix.** This table reports county-level regressions examining how pre-existing national-bank presence shapes the effect of SEPA adoption on the retail mix, defined as the share of retail and restaurant establishments in total establishments. The dependent variable is the retail share of establishments in a county-year. *LnNatlBankPre* is the natural logarithm of the average number of national banks in a county over the five-year pre-adoption period (years -5 to -1 relative to SEPA enactment). Columns (1) and (2) report short-run (-5 to $+5$ years) and long-run (comparing -5 to -1 with $+20$ to $+25$ years) specifications, respectively. The coefficient on *Post* $\times$ *Treated* $\times$ *LnNatlBankPre* measures how the post-SEPA change in retail share varies with pre-existing national-bank depth. All specifications include county fixed effects and county-pair-by-year fixed effects. 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. Standard errors are clustered at the county-pair level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Dependent Variable	(Retail/Total) Establishments	
Years Relative to Event	(-5,+5)	(-5,-1) & (+20,+25)
Post × Treated × LnNatlBankPre	-0.019***	-0.041***
	(-2.783)	(-4.173)
Post x Treated	0.062***	0.123***
	(3.192)	(4.180)
Post × LnNatlBankPre	0.022***	0.043***
	(4.094)	(5.665)
CntyPair#Year FE	YES	YES
Cnty FE	YES	YES
Observations	8,570	9,736

Table A.V: **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.

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Table A.V: 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.V: 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 E Data Description

As we discuss in Section 3.3, 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 E.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 E.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 E.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 E.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 E.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 E.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.IXb 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.IXb, 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 E.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.IXc 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 E.5.3 Non-SEPA-Only County Pairs

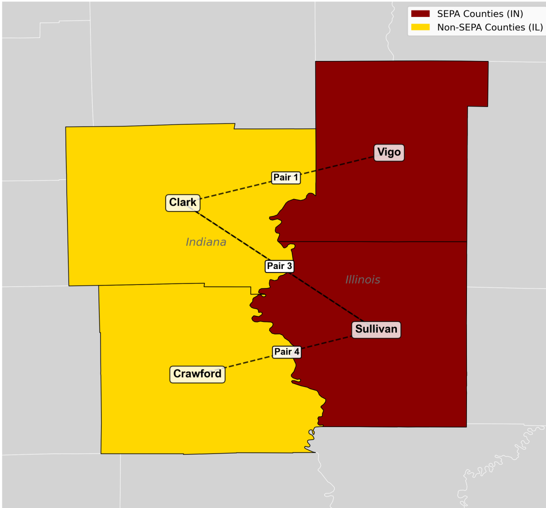
We also construct a complementary sample of *within-state county pairs* for states that did not adopt SEPAs. This "non-SEPA-only" analysis serves as a placebo test, helping us assess whether the patterns observed in SEPA-adopting states are unique to the presence of environmental regulation or simply 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.IXd 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.

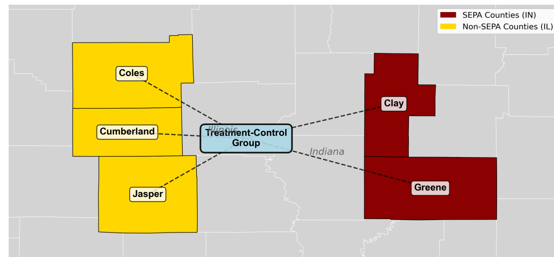
The purpose of these non-SEPA-only pairings is to provide a *placebo benchmark*. Because these counties share similar geographic and economic characteristics but are all located in non-regulated states, any systematic differences between border and interior counties here would reflect underlying regional 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 unregulated 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.

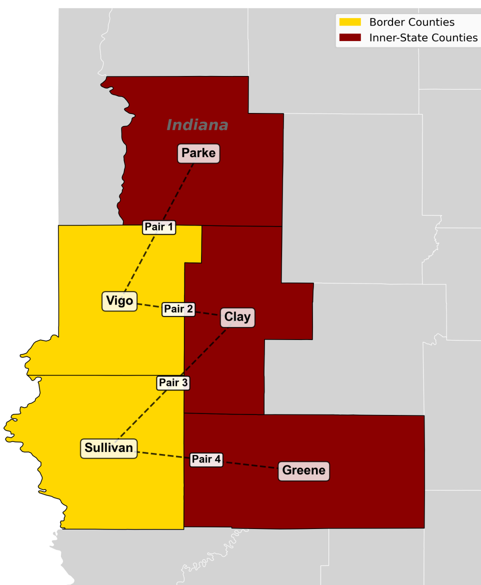
Overall, the non-SEPA-only county-pair analysis acts as a robustness and falsification exercise, reinforcing the causal interpretation of our border-pair findings. By demonstrating that comparable spatial patterns do not arise in states lacking SEPA regulation, we strengthen the argument that the documented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



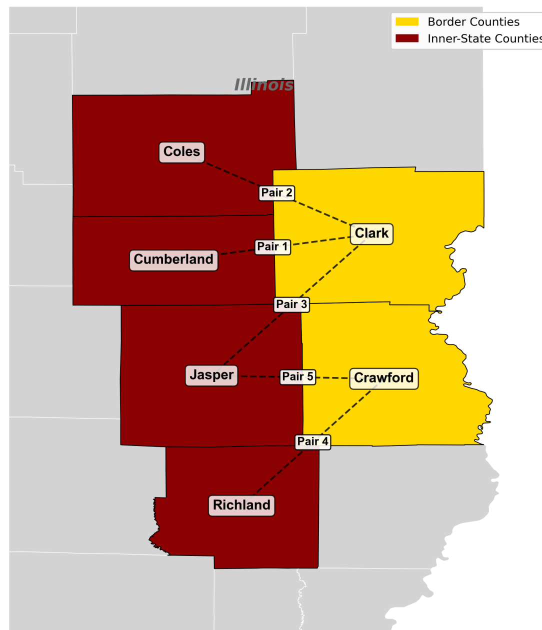
(a) SEPA vs non-SEPA Boarder County Pairs



(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.IX: **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.