The Costs of Environmental Land Use Regulation

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Abstract

This paper measures the effect of environmental land use regulation on housing supply using variation in the timing and location of species listings under the Endangered Species Act (ESA). In a newly constructed panel of municipal housing permits, I find that species listings reduce annual permit flows by 0.5 permits per thousand residents, about 10% of the average annual flow. The effect is robust across different species, geographies, and place characteristics. The effect is larger for critical habitat listings, zero for proposed but not-yet-enforced listings, and is reversed after de-listings. Satellite land use data show reductions in both greenfield and infill development, suggesting that the ESA constrains construction in already-developed areas where benefits to endangered habitats are limited.

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1 Introduction

Housing has become expensive because supply is constrained in cities where demand is high and growing [14]. An important driver of these supply constraints is environmental land use regulation. This paper studies "the strongest environmental law in the world" [28], the Endangered Species Act (ESA), and finds large negative effects on housing permit flows, including mistargeted effects on construction in already-developed areas where endangered habitat may not be at stake.

Section 9 of the Endangered Species Act prohibits "harming or harassing" any endangered animal species. Clearing land and building houses are obvious forms of "harming or harassing," but milder habitat disturbances such as noise, light pollution, and stormwater runoff can also constitute a violation of the ESA. Stronger regulations on federal actions in Section 7 of the ESA spread to private housing construction via federal permitting, land, and funding.

Crucially, these regulations only apply near the habitats of endangered animals. The list of endangered animals varies over time and their habitats vary across space, so the land use regulation attached to these species also varies. Each addition to the endangered species list adds a layer of regulation to areas which happen to host that species but not to others. In 1980, for example, 200 plant and animal species were listed as endangered or threatened and in 2025 there were 1,465 such listings. Each listed species has a distinct habitat. Thus, the growing list of protected habitats is a source of detailed variation in the intensity of land use regulation.

This paper studies the effect of these regulations on housing construction. In a staggered difference-in-difference framework, exploiting 1,200+ species listings across the U.S. from 1980-2024, I find that the average treated place reduces its annual permit flows by 0.52 permits per thousand 1980 residents after a species listing, about 10% of the average annual flow.

Aggregating this effect over all listings since 1980 implies that land use regulations under the ESA have displaced or deterred 9.1 million housing permits. The 95% confidence interval around the central treatment effect (-0.913, -0.128) implies a range of 2–16 million displaced or deterred permits. Displaced permits may lead to an inefficient spatial distribution of living space, but they do not decrease national housing stock. In Section 8, I estimate that 70% of the treatment effect comes from deterred permits that do not show up elsewhere. Thus, I estimate that ESA listings have reduced U.S. housing stock by between 1.6 and 11.1 million units, with a central estimate of 6.3 million units, about 4% of 2025 housing stock.

A concern with the staggered difference-in-difference framework is that ESA listings may be more common in places with characteristics that drive growth (or decline) in permit flows. For example, listings may be downstream of local environmental activism, growth pressure, or other unobserved forces that simultaneously influence construction activity and listing probability. I address this identification problem in four ways. First, in Section 2, I look at how the U.S. Fish and Wildlife Service (USFWS) makes listing decisions. Statutorily, the USFWS is directed to make listing decisions "without reference to possible economic or other impacts" [5], which suggests exogeneity with respect to some of the economic and political factors that may affect permit flow trends.

Second, in Sections 4 and 5, I use geographic fixed effects which control for some of these unobserved factors by identifying the treatment effect from comparisons of places within the same state or metro area. I also use two matching specifications, propensity score matching [29] and entropy balancing [17], that construct control groups which are similar to the treated places on a broad set of observable pre-treatment characteristics. The estimated treatment effect is stable in sign and largely in magnitude (43-135% of the central estimate) through all of these specifications.

Third, in Section 6, I test for the influence of unobserved factors by exploiting treatment reversals that follow species de-listings. De-listing decisions are made internally within the USFWS based on quantifiable status changes like recovery or extinction, without petitions from environmental groups. Thus, de-listings vary the level of land use regulation but not the unobserved factors potentially associated with species listings. I find that de-listings increase permit flows by roughly the same magnitude as listings decrease them, consistent with a causal interpretation of the treatment effect.

Finally, in Section 6.2, I test for a dose-response relationship between the level of regulatory force within ESA listing decisions and permit flows. Prohibitions under the ESA range from the most restrictive critical habitat listings, to standard endangered animal listings, less-protected plant listings, not-yet-enforced proposed listings, and finally de-listings. A dose-response relationship lends credibility to the baseline treatment effect because it suggests a causal relationship running through costs of regulation rather than a confounder that happens to associate species listings with lower permit flows. Indeed, I find effect sizes that fit the ordering of legal costs: largest in critical habitat listings, zero for not-yet-enforced listings, and a reversal of the effect size after de-listings. If listings merely correlated with other factors that reduce permitting, one would not expect a dose-response relationship because these factors would also be correlated with, say, endangered plant listings.

Extending to a measure of extensive and intensive-margin land use changes in the National Land Cover Database in Section 7, I find that endangered species listings slow down the rate of both greenfield and infill development by around 10%. Treatment probability and treatment effects are at least as large in highly developed places as they are in the full sample, and permit flows for dense, 5+ unit buildings fall more than permit flows for other building types after an ESA listing. Infill development and construction of larger buildings in already-developed areas is unlikely to have negative effects on endangered species habitat, and may even have positive effects as it substitutes for less dense housing elsewhere with greater energy and transportation use. This drag on infill development suggests substantial mistargeting of the effects of the ESA and room for a Pareto improvement in the tradeoff between housing production and species protection.

A large literature finds that land-use regulations and permitting delays can reduce housing construction, lower housing-supply elasticities, and raise prices [20, 13, 15, 27, 16]. Duranton and Puga show that these local constraints can aggregate into substantial reductions in national output [7]. The Endangered Species Act and its effect on land use has also been the subject of some research. Previous work has identified decreases in oil-lease values, agricultural profits, and housing construction after species listings [3, 21, 41]. This paper extends Zabel and Paterson's analysis [41] of the ESA's effect on housing construction to a national panel of permit-issuing places and to all species listings from 1980–2024, rather than only Californian critical-habitat designations. I also use satellite land-use data to distinguish effects on greenfield and infill development, which clarifies difficult tradeoffs between species protection and housing.

The closest recent work is Frank et al. [11], which uses the same endangered species list as treatment variation and a similar staggered difference-in-difference framework, but with main outcomes measured in land-market data rather than with building permits. They find at most modest effects on land market transaction volumes and prices. They also find no detectable effect of species listing on building permit flows in a set of descriptive correlations and first-differences at the county level. A null first-difference effect is theoretically consistent with a negative difference-in-difference effect if permit flows would have increased in treated places absent the listing and instead only stayed flat. Indeed, in Appendix Section 11.3 I find that this is the case in my data as I am able to replicate their descriptive results in the same sample that produces the negative difference-in-difference treatment effect.

The remainder of the paper proceeds as follows. Section 2 describes how the USFWS adds to the endangered species list and the legal mechanism that translates those listings into enforceable constraints on development. Section 3 describes the permitting panel and the construction of place-level listing exposure from USFWS habitat geometries. Section 4 illustrates the design using the Northern Long Eared Bat listing, the largest endangered listing in the 1980-2024 period. Section 5 presents the main stacked event-study results. Section 6 tests predictions of the legal mechanism using proposed listings, plant listings, critical habitat, de-listings, and heterogeneity analyses. Section 7 studies the possible mistargeting of land use regulations under the ESA in already-developed areas and infill construction. Section 8 aggregates the treatment effect and estimates the share of permits that are deterred rather than displaced by the ESA to calculate the implied loss to national housing stock. Section 9 discusses the implied tradeoffs and the scope for policy design that reduces housing costs without weakening species protection.

2 Legal Mechanism

2.1 Listing Decision

The central identification assumption in a difference-in-difference research design is parallel trends: absent treatment, treated and control units would have followed the same average trend in the outcome. Here, that means per capita permit flows in treated and control locations would have moved in parallel had the endangered species listing never occurred — i.e., the location of species habitat is independent of the underlying factors that drive permit trends.

This assumption of parallel trends could be violated if, for example, a turn in local political attitudes against development leads to species listings while also constraining development in other ways, like negative public comments at zoning hearings. The legal framework through which the USFWS makes listing decisions makes this scenario unlikely. The Endangered Species Act instructs the USFWS to make listing decisions "solely on the basis of the best scientific and commercial data" and the agency's implementing regulations explicitly require classification decisions to be made "without reference to possible economic or other impacts of such determinations" [32, 5]. Listing decisions are made with reference to biological factors like disease, predation, and habitat loss that are unrelated to changes in local political attitudes. The USFWS does accept petitions calling for the listing of certain species, and many of these petitions come from influential environmentalists or advocacy groups. However, only 20%

of these petitions are accepted by the USFWS and 50% of endangered species listings come solely from internal review and so are never petitioned for by an outside group.

The requirement to ignore economic or political factors in listing decisions helps allay concerns over local politics driving the reduction in permit flows and overstating the treatment effect. Other worries, like the fact that species may become endangered under the pressure of rapid development, may also cause bias by selecting treated places on permit flow trends, although this bias would reduce the size of the estimated effect. Both possible sources of bias are addressed further in later sections of the paper. Sections 4 and 5 use geographic fixed effects and matching to narrow the scope for confounding differential trends, and Section 6 uses variation in the legal bite of ESA listings to isolate the compliance requirements of species listings from the local politics that might otherwise confound the result.

2.2 Regulatory Constraints

This paper's identification strategy counts any post-treatment change in the difference between treated and control permit flows as the causal effect of exposure to the Endangered Species Act. For that to be credible, there must be a legal mechanism connecting species listings to additional costs faced by developers. The ESA has two such mechanisms.

The first works similarly to the National Environmental Policy Act (NEPA). Section 7 of the Endangered Species Act requires the federal government to ensure that its agencies are not "likely to jeopardize endangered or threatened species" or to "adversely modify" species habitat [35]. In practice, compliance takes the form of consultations with the USFWS. The first step of these consultations is determining which species may be present in the area around the action in question, and whether they are likely to be adversely affected. Tens of thousands of these "informal" consultations happen every year [19] and they take nearly three months to complete on average [24].

For at least 90% of informal consultations, the result is a written concurrence from the USFWS that no adverse effect on a listed species is likely. The rest graduate to the formal consultation process. After preparing all of the required information, applicants wait nine months for a determination, on average, and can frequently wait more than a year. The outcome of this process is an incidental take statement that describes "reasonable and prudent" mitigation measures that must be followed for the incidental take related to the federal action to be permissible.

Optionally, when the USFWS lists a species it can designate a certain area as "critical habitat" for that species. This raises the standards required of federal actions to protect the habitat, and can extend the protection to areas with no extant members of the species. Critical habitats do not add explicit standards on top of the Section 9 prohibition on "take" but they are often placed in areas where the species is particularly likely to live, and thus where take is particularly likely to occur.

The ESA's regulation of federal actions spreads to housing construction via federal permitting, land, and funding. Most housing projects do not have a federal nexus, but many do. For example, if a subdivision is built on wetlands and requires a discharge permit from the U.S. Army Corps of Engineers, or if the project gets funding from the Department of Housing and Urban Development, then it must comply with the rules in Section 7 of the ESA.

Unlike NEPA, the Endangered Species Act has direct regulations that apply to "all persons", not just the federal government. Section 9 of the ESA prohibits "take" from endangered and threatened animal species. This rule differs from the regulations on federal agencies in three ways.

First, the standards for an action to constitute "take" are higher than they are for "likely to jeopardize or adversely modify." The exact definition of the term is the subject of a substantial body of jurisprudence but essentially it encompasses any action which leads to the actual death or injury of members of an endangered species. The standing Supreme Court precedent in Babbitt v Sweet Home rules that habitat modification, even when it does not directly harm an animal, can satisfy this definition[1] [40].

Second, Section 9 only prohibits take from listed animals, not plants [39]. Take is still prohibited if the plants are on federal land, or if take of the plant is prohibited by state law, but in general the protections for listed plants are weaker under Section 9 than under Section 7.

Finally, most compliance with Section 9 happens through self-assessed avoidance and adaptation under threat of ex-post punishment or litigation rather than through ex-ante consultation with the USFWS. Consultation does happen when projects apply for an incidental take permit, but this is a small fraction of housing construction projects.

The best way to understand the concrete mechanics of how housing construction interacts with the

[1] On July 10, 2026, FWS and NMFS finalized a rule rescinding this definition of "harm." Relying on *Loper Bright* and Justice Scalia's *Sweet Home* dissent, it removes habitat modification from the Section 9 take prohibition. Section 7 and critical habitat prohibitions are unchanged. The rule is prospective and, as of this writing, is expected to face APA litigation and possible preliminary injunction. This rule change does not affect any of the listings in my sample nor the back-casted estimate of the ESA's aggregate effect on housing stock since 1980, but it may be important for future predictions of the impact of ESA listings.

Endangered Species Act is through examples. Below, I give three examples that encompass most of the ways that the ESA can impact housing. The examples are increasing in the costs of compliance but decreasing in frequency. Roughly, the first example reflects ≈90% of ESA compliance for housing construction; small costs from biological consulting and minor adaptation of the project. The second reflects around 10% of ESA compliance, concentrated in particular areas with Habitat Conservation Plans. The final example reflects the large costs facing the ≈1% of projects that have a federal nexus and require a formal consultation with the USFWS.

Figure 1. Samuel Madden Homes Redevelopment Site

Notes: In the center of the image are the old public housing buildings planned for demolition as well as the trees that make up the potential NLEB habitat.

For the first example, consider the Alexandria Redevelopment & Housing Authority's ongoing redevelopment of the Samuel Madden Homes in Alexandria, Virginia. The plan involves demolishing 13 aging public housing buildings on a 3.45-acre site to construct two new mixed-use towers containing 532 affordable and market-rate units. Despite the fact that this development is in an already highly-developed area (Figure 1), Alexandria is within the area listed as habitat for the endangered Northern Long Eared Bat (NLEB) and there are trees on site which may serve as habitat. Planning for this project is ongoing and construction has not yet begun, but the developers have already hired an environmental compliance consultant and run an IPaC report as part of the environmental assessment for this project [4]. IPaC is an online tool provided by the USFWS to streamline compliance with the ESA. In this case, IPaC confirmed the overlap of the project's impact area with NLEB habitat and, after a series of questions about the project site and the results of surveys of the potential habitat, output a determination of no effect, conditional on following certain conservation measures like only removing trees between October and March, avoiding canopy gaps of more than 1,000 feet, and preserving "primary roost trees" [25]. The developer must adapt to these conservation measures, perhaps via delaying construction to January and changing plans to maintain more tree cover, before starting construction.

For the second example, consider a hypothetical 50-unit, 10-acre subdivision in Thurston County, Washington, where there is a regional Habitat Conservation Plan (HCP) covering several local endangered species including the Olympia Pocket Gopher [31]. A regional HCP is a delegation of permitting authority from the USFWS to a state, county, or municipality on the condition of certain mitigation measures; most often land set-asides and buybacks funded by conservation fees as well as mandatory "best management practices." In the case of this 10-acre development in Thurston County, when the developer applies for a building permit, the county will check the soil map and conclude that the project area overlaps with gopher habitat. If the developer has not already conformed their plans to the best management practices, the county may reject the permit until it reflects them; requiring, for example, clustering of the buildings on a smaller area, shrinking the project to avoid high-value soils, and establishing "no-work zones" that are sheltered during construction [30]. Once the permit is approved, the next step is paying the gopher conservation fee. This fee is calculated as a dollar amount per acre of disturbed habitat using methodology that the USFWS agreed to in the County's HCP application. In this case, there are 10 acres of affected habitat at $51,000 per acre making a total cost of $510,000 in mitigation fees. The County uses that money to buy and conserve gopher habitat elsewhere and the developer gets a "Certificate of Inclusion" which gives them incidental take coverage to bulldoze the site, even if gophers are harmed or harassed as a result of the construction.

The final example is the Kingston Project in Lee County, Florida, which is currently under construction. Kingston is a 7,000 acre master-planned community expected to build on the order of 10,000 homes on an "inactive and distressed citrus grove" that overlaps with the habitat of the Audubon's crested caracara, the eastern indigo snake, and the Florida panther [18, 36]. The Kingston Project will fill 13 acres of wetlands and 128 acres of other "waters of the U.S.," requiring a Clean Water Act Section 404 permit from the U.S. Army Corps of Engineers and activating Section 7 of the ESA [33]. Formal consultation with the USFWS took place before construction began and lasted 8 months. The service's biological opinion was issued in January of 2025 and it requires permanent conservation and restoration of 3,293 acres secured by conservation easements with third-party enforcement rights for the USFWS. For panther mitigation, the opinion requires the developer to purchase 3,771 “panther habitat units” and 4.57 “wetland mitigation credits” from local conservation banks, in addition to making cash contributions of $350 per development acre (about $1.19 million), $200 per building permit (about $2 million), and $200 per home resale. The opinion further requires approximately $20 million in contributions to a fund for Lee County’s Corkscrew Road and obligates the developer to construct, at its own expense, a large wildlife crossing on the internal four-lane spine road plus 12 smaller crossings. Using recent southwest-Florida mitigation bank price quotes of $850 per panther habitat unit and $190,000 per wetland mitigation credit, the required bank-credit purchases total roughly $4.1 million [22, 6]. Combined with the other cash contributions, the direct mitigation bill is about $27 million, excluding the cost of setting aside half of the plot for conservation, wildlife crossing construction, species surveys, and the multi-decade reporting required by the incidental take statement.

The economic cost of endangered species listings is corroborated by public and industry responses to new listings. The listing of the Northern Long Eared Bat, for example, prompted Representative Pete Stauber of Minnesota and Senator Markwayne Mullin of Oklahoma to introduce legislation attempting to overturn the listing in 2023 due to its economic impacts [26]. The American Road and Transportation Builders Association worried that the listing could “impose significant new regulatory requirements and development restrictions in up to 37 states,” and the National Association of Home Builders claimed that “this seemingly innocuous bat could have an outsized impact on the U.S. residential construction sector’s ability to provide desperately needed building lots” [1].

The legal mechanism of the Endangered Species Act supports the research design of this paper in two ways. First, it supports the central identification assumption that species listing exposure is independent of trends in per capita permit flows. The USFWS is explicitly prohibited from considering economic or political factors in listing that might otherwise violate this assumption. Second, it adds credibility to the claim that post-treatment changes in the difference between treated and control permit flows are the causal effect of exposure to the Endangered Species Act. The regulations on federal and private behavior under the ESA have clear connections to housing construction, with costs ranging from minor seasonal restrictions and informal consultations affecting tens of millions of projects to ten-million-dollar mitigation packages for a small number of projects with a large impact. These regulatory mechanisms create barriers to construction that vary systematically with species listings, giving the variation needed to identify the effect of land use regulation on permit flows.

3 Data

This paper uses two primary datasets: (i) annual housing permit flows at the place level from the Census Building Permits Survey (BPS), and (ii) the timing, location, and characteristics of endangered species listings from the U.S. Fish and Wildlife Service ECOS Data Explorer.

3.1 Building permits and place geography

The BPS reports annual counts of permitted housing units by permit-issuing place. The permit-issuing place is a geographic unit unique to the BPS which is based on the legal jurisdiction of the individual permit offices that the survey is sent to each year. This means permit-issuing places are a combination of incorporated cities, villages, and towns as well as county subdivisions and occasionally counties in areas where there is no other local government with permitting authority [34]. There were 23,000 unique places in the most recent year of the survey.

To crosswalk the building permit information with the habitat maps from the USFWS that define treatment, each of these places needs an associated geography. The Census does not have shapefiles for any of the permit-issuing places and only has FIPS place codes for a subset of the more recent observations. After extensive data cleaning to propagate the available geographic IDs to earlier observations of the same place as well as fuzzy matching on place names with manual corrections, I am able to associate 99.4% of all unique places in the BPS data with an NHGIS code from IPUMS. For clarity, I use the centroid of each place in the maps in the main paper, but the actual boundaries are reproduced in Appendix Figure 10a. I also assign a fixed, 2010 CBSA code to every observation by connecting each place to the nearest CBSA. This means that all places are assigned to a CBSA, even if they do not intersect any metropolitan area borders. This supports the CBSA$\times$year fixed effects used in the main specifications.

Table 1 summarizes permitting and demographic characteristics in the balanced panel of 12,664 places observed from 1980 to 2024. The sample covers about two-thirds of national permit flows in benchmark Census years, and all statistics are weighted by 1980 population so that the table describes the distribution faced by the average baseline resident rather than the average permit-issuing jurisdiction. The standard-deviation columns decompose variation into persistent differences across places and within-place changes over time. Panel B reports the average decadal growth for the variables where multiple years of data are available.

Table 1. Building Permits Survey Summary Statistics

Variable	Mean	*Standard deviations* Overall	Between	Within	*Percentiles* P10	P50	P90
Panel A. Levels							
Permits	1,239.2	2,761.7	2,241.2	1,613.7	4.0	156.0	3,650.0
Permit rate	5.7	14.9	10.7	10.3	0.2	2.5	13.0
Housing stock per-capita	0.41	0.08	0.07	0.03	0.34	0.41	0.48
Density	1,876	3,270	3,167	816	160	992	4,137
Population (1000s)	385.3	741.7	737.5	78.5	6.1	62.6	1,327.4
Urban share	90.1	25.8	24.5	8.1	62.5	100.0	100.0
Vacancy share	7.9	5.6	5.2	2.1	3.2	6.7	13.0
Owner share	59.9	16.4	16.0	3.4	39.5	60.0	81.3
Black share	14.6	17.7	17.3	3.8	0.4	7.0	39.8
Under 18 share	24.6	4.5	3.8	2.4	19.2	24.7	30.0
Over 65 share	13.2	4.7	4.1	2.5	8.1	12.6	18.8
BSH elasticity	0.32	0.19	0.19	0.00	0.15	0.25	0.61
Saiz elasticity	1.78	1.13	1.13	0.00	0.75	1.45	3.23
WRLURI	0.18	0.71	0.71	0.00	-0.78	0.21	1.01
Log median income	10.45	0.55	0.33	0.45	9.66	10.49	11.15
Log per-capita income	9.74	0.59	0.30	0.52	8.86	9.80	10.47
Panel B. Decennial changes							
Permit rate	0.1	9.6	4.5	8.4	-3.9	-0.1	3.4
Housing stock	2.7	6.5	3.3	5.6	-4.1	2.6	9.9
Density	3.3	34.9	18.8	29.4	-10.7	2.5	16.5
Population	7.3	17.7	12.9	12.2	-6.6	4.2	22.3
Urban share	0.3	10.0	4.4	9.0	-1.0	0.0	1.2
Vacancy share	0.4	3.0	0.9	2.8	-2.9	0.4	3.8
Owner share	-1.0	4.3	1.8	4.0	-4.6	-0.9	2.5
Black share	0.7	3.3	2.3	2.3	-2.2	0.3	3.9
Under 18 share	-1.5	2.1	1.0	1.9	-3.9	-1.6	1.2
Over 65 share	1.2	2.4	1.3	2.0	-1.5	1.1	4.1
Median income	41.0	27.7	9.5	26.0	14.2	33.3	83.7
Per-capita income	48.8	32.1	9.8	30.6	18.4	38.9	97.2

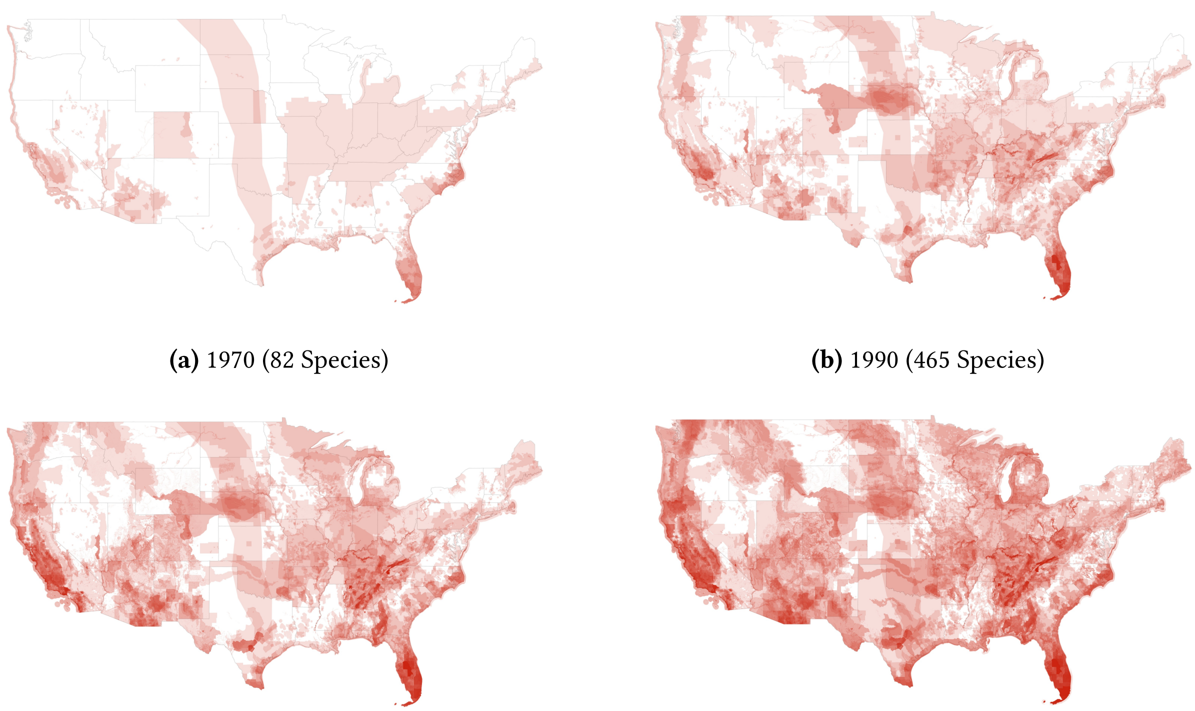
Notes: This table summarizes the balanced BPS panel used in the permit regressions.
- *Sample:* Balanced panel of 12,664 permit-issuing places, 1980–2024. Statistics are weighted by 1980 population. The balanced panel covers 64.4%, 67.1%, and 66.2% of national permits in 2000, 2010, and 2020.
- *Rows:* Panel A reports place-year levels. Panel B reports changes between decennial Census observations in percent or percentage points. Permit rate reports changes in decade-average permit flow in permits per 1,000 1980 residents.
- *Columns:* Overall is the pooled standard deviation. Between is the standard deviation across place means. Within is the standard deviation within places over time. Percentile columns report the weighted distribution of observations.
- *Units:* Permit rate is measured in permits per 1,000 1980 residents. Housing stock is measured in units per resident. Population is measured in thousands. Shares are percentage points; share changes are percentage-point changes. BSH is the Baum-Snow-Han housing-supply elasticity. WRLURI is the Wharton Residential Land Use Regulatory Index.

3.2 ESA listings and habitat geometries

The second dataset tracks the geographic location, characteristics, and timing of species listings from the USFWS's ECOS Data Explorer [38]. Figure 2 shows how the map of endangered habitats has changed over time. I assign each species listing to an "event id", which is a unique combination of Species ID $\times$

Listing Status × Listing Date, and I union habitat boundaries together within this event when multiple files exist. For example, the event id for the listing of the Northern Long Eared Bat is *Myotis septentrionalis* × Endangered × 2015. To align the habitat data with the jurisdictional scope of the ESA, I clip habitats to the 50 U.S. states plus a 12-nautical-mile buffer (territorial sea). The resulting habitat panel contains a cleaned habitat map and a list of associated attributes for each species listing event such as taxonomy and a critical habitat indicator. I join the species listing shocks into the annual place-level panel by intersecting each habitat map with the map of permit-issuing place jurisdictions in the listing year, creating a binary treatment indicator for every place-year in the panel. De-listings are handled symmetrically. For species that are listed and later de-listed, I create a de-listing "event" with the same habitat map and species attributes at the de-listing date.

Figure 2. Habitats of Listed Species from 1970 to 2025



(a) 1970 (82 Species)

(b) 1990 (465 Species)

(c) 2010 (1,164 Species)

(d) 2025 (1,465 Species)

Notes: This figure maps the cumulative geographic reach of listed-species habitat.
- *Panels:* Each panel overlays range-map geometries for species listed as endangered or threatened by that year. Darker areas have more overlapping ranges. Panel titles report distinct listed species.

In Table 2, I define treatment using the levels of legal restrictions for listings under the ESA: animal endangered/threatened listings are classified as *Full* treatment; plant endangered/threatened listings are *Partial*; listings that have been proposed but not yet added are *Proposed*. Unless otherwise specified, I use "listings" to refer to full listings as defined above. Panel A shows the number of listing events in the average place-year as well as the cumulative "stock" of endangered habitats in the average place-year. Panel B summarizes the lifetime treatment characteristics of all places in the balanced sample. Nearly 90% of places are treated with a *Full* listing at least once, and the average place has just over 2 listings over the 45-year period in the panel.

Table 2. Summary of ESA Treatment Shocks

Variable	Mean	Pr(>0)	Mean (>0)	P95 (>0)	Max
Panel A. Place-year level					
New full listings (annual)	0.053	0.045	1.159	2	10
New partial listings (annual)	0.027	0.023	1.154	2	45
New proposed listings (annual)	0.063	0.053	1.198	2	6
New de-listings (annual)	0.032	0.026	1.253	2	3
Full listings stock	3.335	0.888	3.756	9	38
Partial listings stock	0.904	0.455	1.987	5	173
Panel B. Place level					
Ever receives full listing shock	0.870	0.870	1.000	1	1
Ever receives partial listing shock	0.566	0.566	1.000	1	1
Ever receives proposed listing shock	0.999	0.999	1.000	1	1
Ever receives de-listing shock	0.679	0.679	1.000	1	1
Years with $\geq$1 full listing	2.042	0.870	2.348	5	18
Years with $\geq$1 partial listing	1.034	0.566	1.826	4	12
Total full listings (1980–2024)	2.366	0.870	2.721	7	24
Total partial listings (1980–2024)	1.193	0.566	2.107	5	170

Notes: This table summarizes the distribution of ESA listing exposures across permit-issuing places.
- *Sample:* All 12,664 places in the balanced BPS panel. Statistics are equal-weighted.
- *Rows:* Panel A reports annual listing shocks and listing stocks at the place-year level. Panel B reports place-level exposure over 1980–2024. Full listings are endangered or threatened animal listings. Partial listings are endangered or threatened plant listings. Proposed listings are not final. De-listings remove listing status.
- *Columns:* Pr(>0) is the share of place-years or places with positive exposure. Mean (>0), P95 (>0), and Max are conditional on positive exposure.

4 Case Study: Northern Long Eared Bat

I begin by illustrating the identification strategy with the 2015 listing of the Northern Long Eared Bat. This methodology is extended to all species listings in Section 5.

The Northern Long Eared Bat (NLEB) is by far the largest single endangered species listing in the 1980-2024 period with over 1.5 million square kilometers of habitat, beaten only by the listing of the Aleutian Canada Goose in 1967. Figure 3a maps the centroids of the treated and control places that are included in the case study sample.

Treated places are those which intersect the NLEB habitat in 2015 and do not have any other listings in the 8-year radius around 2015. Control places are those which have no listings in the treatment year or the 16-year pre/post period. I also restrict both groups to states containing at least one treated place and one control place. There are 2,552 control places and 3,650 treated places in the sample. Figure 3b plots the average flow of per capita building permits within the treated and control groups, weighted by 1980 population, with the start of treatment after 2014 marked by the vertical dotted line.

Figure 3c plots the coefficients from the population-weighted event-study regression in equation 1.

$$y_{it} = \sum_{k=-8}^{8} \beta_k \left(T_i \times R_t^k\right) + \alpha_i + \lambda_t + \varepsilon_{it}, \qquad k \neq -1 \tag{1}$$

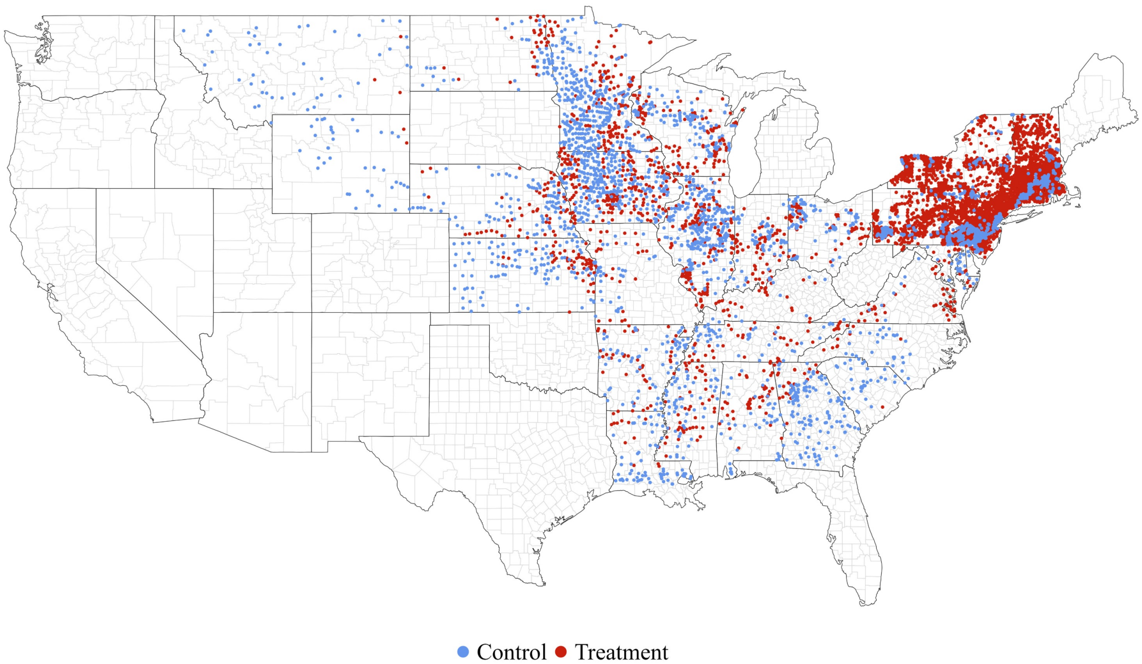
$$y_{it} = \beta \left(T_i \times Post_t\right) + \alpha_i + \lambda_t + \varepsilon_{it}. \tag{2}$$

Here, y_{it} is annual permits per 1,000 1980 residents, T_i is an indicator for treated places, R_t^k is an indicator for the calendar year k-years before the treatment in 2015, α_i is a place fixed effect, and λ_t is a year fixed effect.

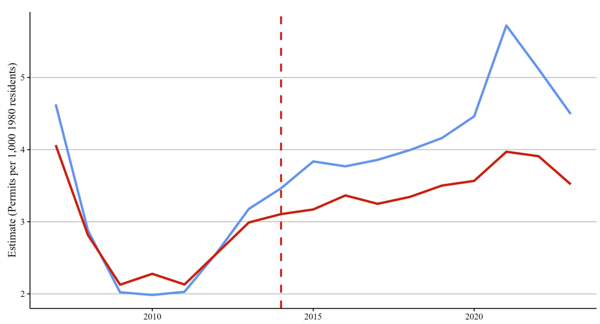
The event-time coefficients β_k measure differential changes in permitting among treated places relative to control places, with 2014 as the reference year. To see this, let bars denote population-weighted averages. In year $t = 2015 + k$, the model implies that the average permit flow in control places is $\bar{y}_{Ct} = \bar{\alpha}_C + \lambda_t$ while the average permit flow in treated places is $\bar{y}_{Tt} = \bar{\alpha}_T + \lambda_t + \beta_k$. The treated-control difference in year $t = 2015 + k$ is therefore $\bar{y}_{Tt} - \bar{y}_{Ct} = (\bar{\alpha}_T - \bar{\alpha}_C) + \beta_k$. Because the event-time indicator for 2014, $k = -1$, is omitted, the treated-control difference in 2014 must be $\bar{y}_{T,2014} - \bar{y}_{C,2014} = \bar{\alpha}_T - \bar{\alpha}_C$. It follows that $\beta_k = (\bar{y}_{T,2015+k} - \bar{y}_{C,2015+k}) - (\bar{y}_{T,2014} - \bar{y}_{C,2014})$. Thus, each β_k measures how much the treated–control permitting gap in year $2015 + k$ differs from the gap in 2014. Under the assumption of parallel trends, this difference in the treated-control gap measured by β_k is the causal effect of treatment. Table 4 reports estimates from equation 2, which replaces the event-time indicators with a single post-treatment indicator.

The average treatment effect estimated in the baseline event study regression is -0.78 permits per thousand 1980 residents per year. The national average per capita permitting rate since 1980 is about 5 permits per thousand, so -0.78 is a reduction of 15%. The event study is population weighted, so this result reflects the impact of the NLEB listing on the average person in the sample, rather than the impact on the average place.

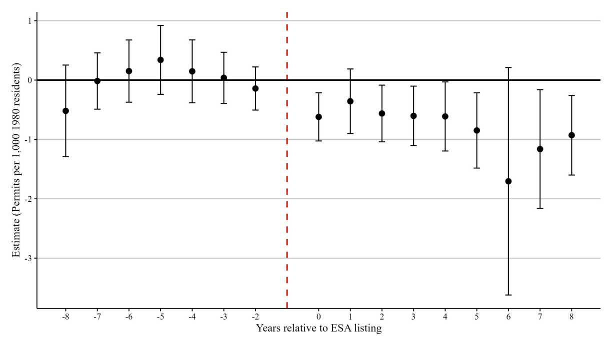
Figure 3. Case Study: Northern Long Eared Bat (NLEB)



(a) Treated vs Control Place Centroids (2015)



(b) Population-weighted levels



(c) Event-Study Coefficients

Notes: This figure maps places in the NLEB sample, plots average permit rates over time, and reports event-study coefficients.

- *Sample:* Treated places intersect NLEB habitat in 2015 and have no other full listing shock in the ± 8-year window. Controls have no full shock in the same window and are restricted to states with both groups.
- *Panels:* Panel A maps place centroids. Panel B plots 1980-population-weighted permit rates. Panel C plots event-study coefficients from equation 1.
- *Units:* Permit rates and coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Panel C omits the year before treatment. Averages and regressions are population-weighted.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

This estimate reflects the causal effect of the NLEB listing only if the gap in average permit flows between treated and control places would have remained stable were it not for the listing. This parallel trends condition is ultimately untestable, but it is more believable if treated and control places look similar, and look like they were on similar trajectories, in the pre-treatment period.

To this end, Table 3 reports the pre-treatment averages for a set of balance variables within the treatment and control groups and tests for statistically significant differences. The SD column compares these differences to the average standard deviation of each covariate, i.e. $\sqrt{\frac{(\sigma_T^2+\sigma_C^2)}{2}}$, where σ_T^2 and σ_C^2 are the standard deviations of each covariate within the treatment and control groups. The permit association column shows the typical relationship between an extra standard deviation of the given covariate and per capita permit flows in the pooled sample. The matched and entropy columns report the mean differences in matched samples aimed at improving pre-treatment balance, described in more detail below. In addition to standard demographic variables from decadal place censuses, I also include three important measures of housing supply elasticity or housing supply regulation from the urban economics literature: Nathaniel Baum-Snow and Lu Han's micro-geography elasticities [2], aggregated to the metro level, Albert Saiz's metro level elasticities [27], and the Wharton Land Use Regulatory Index [16].

Overall, the NLEB treated and control groups exhibit substantial imbalance, with just over half of the covariates having significant mean differences. There is close balance on pre-treatment changes in permit flows, housing stock, and population, which can be seen in the close parallel trends in Figures 3c and 3b. Parallel trends in permit flows is sufficient to identify the causal effect of treatment, even when other variables are imbalanced. Still, this imbalance in baseline characteristics and trends is a potential threat to the causal interpretation of the result.

I turn to matching methods to address these balance concerns. First, I estimate a propensity score for treatment using the pre-treatment balance covariates in Table 3 and match each treated place to the five nearest control places within a 0.2-standard-deviation caliper of the propensity score logit. This constructs a control group of untreated places that, based on pre-treatment characteristics, had a similar probability of being listed as NLEB habitat [29]. Matching improves the pre-treatment balance on these observed characteristics and narrows the scope for confounding. To the extent that confounding forces are reflected in pre-treatment demographics, income, housing-market conditions, regulation, or permit-flow trends, the propensity matched specification will control for their influence on treatment probability. Second, I use entropy balancing, which keeps the treated observations fixed and reweights the control observations so that their weighted covariate means exactly match the treated covariate means [17]. This achieves a much closer balance and a smoother distribution of weight across the control group than propensity score matching.

Table 3. Pre-treatment Balance in the NLEB Sample

Covariate	*Scale* SD	*Scale* Permit assoc.	*Means* Treated	*Means* Control	*Treated − control difference* Original	*Treated − control difference* Matched	*Treated − control difference* Entropy
Panel A. Levels							
Permit rate	9.42	9.36	4.60	4.55	0.06	0.12	0
Housing stock	0.07	0.02	0.43	0.44	-0.01***	0	0
Density	1696	-1.25	837	1769	-932***	-849	0
Population	323.2	-0.62	76.3	214.9	-138.6	-344.6	0
Urban share	27.5	-0.36	84.1	90.6	-6.5***	-6.5***	0
Vacancy share	5.0	0.22	8.4	9.6	-1.2***	-0.6	0
Owner share	15.3	1.16	66.6	58.2	8.4***	3.5	0
Black share	20.4	-0.43	11.9	24.0	-12.2***	-9.5*	0
Under 18 share	3.6	1.19	22.8	23.1	-0.3	0.3	0
Over 65 share	3.9	-1.30	14.5	13.9	0.6**	0.7	0
BSH elasticity	0.19	1.76	0.42	0.31	0.11***	0.09**	0
Saiz elasticity	1.23	0.74	1.90	2.19	-0.28***	0.05	0
WRLURI	0.85	-0.79	0.28	0.25	0.03	-0.24*	0
Log median income	0.38	1.23	10.91	10.71	0.19***	0.09	0
Log per-capita income	0.33	1.02	10.22	10.09	0.13***	0.04	0
Panel B. Pre-treatment changes							
Permit rate	0.99	1.25	-0.06	-0.06	0.00	-0.03	0
Housing stock	5.4	0.48	3.3	3.7	-0.5	0.3	0
Population	15.4	4.74	5.9	3.5	2.4	1.5	0
Density	15.2	2.49	3.6	-2.0	5.5***	2.8	0
Urban share	7.4	1.23	0.8	0.4	0.4***	0.2	0
Vacancy share	2.2	0.41	1.7	2.0	-0.3	0.4	0
Owner share	3.2	0.63	-1.2	-2.0	0.8**	0.9	0
Black share	3.3	0.15	1.3	1.2	0.1	0.6**	0
Under 18 share	1.8	0.69	-1.6	-1.6	0	0.3	0
Over 65 share	2.0	1.49	0.3	-0.7	1.0***	0.6	0
Median income	12.6	0.83	23.7	22.8	1.0	0.6	0
Per-capita income	12.4	0.64	26.0	26.7	-0.7	-1.8**	0

Notes: This table reports population-weighted pre-treatment covariate balance.

- *Sample:* In the NLEB sample, treated places intersect NLEB habitat in 2015 and have no other Full listing shock in the ±8-year window. Controls have no Full listing shock in the same window. The sample is restricted to states with both treated and control places.
- *Rows:* Panel A reports pre-treatment levels. Panel B reports changes between two pre-treatment Census-decades in percent or percentage points, except permit rate, which reports the average annual change in permit flow over the pre-treatment period.
- *Columns:* SD is $\sqrt{(\sigma_T^2+\sigma_C^2)/2}$, the average within-group standard deviation of the covariate. Permit assoc. is the pooled pre-treatment association between permit rates and a one-standard-deviation increase in the covariate. Treated and Control report weighted means. Original, Matched, and Entropy report treated minus control differences. Propensity matching uses up to five controls within a 0.2-standard-deviation logit caliper; entropy balancing reweights controls to match treated means.
- *Units:* Permit rate is measured in permits per 1,000 1980 residents. Housing stock is measured in units per resident. Population is measured in thousands. Shares are percentage points; share changes are percentage-point changes. Other changes are percent changes. BSH is the Baum-Snow-Han elasticity; WRLURI is the Wharton index.
- *Inference:* Standard errors are clustered by place. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The columns of Table 4 test the robustness of the permit flow decline result to the matching specifications described above. The rows of the table introduce increasingly restrictive geography × year fixed effects $\delta_{g(i)t}$ to equation 2, where $g(i)$ is either state or CBSA. These geography-by-year fixed effects absorb contemporaneous shocks shared by places in the same geography, such as local housing-demand or construction-cycle shocks, and effectively restrict the regression to comparisons within states or metro areas.

The effect is moderately robust, staying negative across all specifications and ranging from -1.1 to -0.28 permits per 1,000 1980 residents, though many of the estimates are not statistically significant. The largest effects, but also the largest standard errors, are in the entropy balanced specification. Entropy balancing doesn't drop any control observations, but forcing exact balance on 27 covariates that differ substantially between the NLEB treated and control groups concentrates weight on a minority of control places that resemble the treated group, lowering the effective sample size and widening the intervals. The event study graphs corresponding to each of the nine estimates in Table 4 are available in Appendix Figure 11.

Table 4. Treatment Effect Estimates in the NLEB Sample

	Sample construction		
Geography × year FE	**Original**	**Propensity matched**	**Entropy balanced**
National	-0.783***	-0.776**	-1.123
	(0.230)	(0.293)	(0.927)
State	-0.387	-0.565*	-0.653
	(0.250)	(0.269)	(0.701)
CBSA	-0.297	-0.280	-0.846
	(0.232)	(0.218)	(1.098)
Treated places	3,650	3,621	3,650
Control places	2,552	2,399	2,552

Notes: This table reports treatment effects for the 2015 NLEB listing.
- *Sample:* The NLEB sample as defined in Figure 3 and summarized in Table 3.
- *Columns:* Column labels indicate sample construction. Original keeps all treated and control places in the NLEB sample; propensity matched uses the matched sample from Table 3; entropy balanced reweights controls as in Table 3.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each cell is the coefficient on $T_i \times Post_t$ from equation 2. Regressions include place fixed effects and the geography-by-year fixed effect listed in the row.
- *Inference:* Standard errors are clustered by place. Place counts are unweighted counts. Event-study plots are in Appendix Figure 11. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The NLEB case study identifies a treatment effect that is not explained by observable imbalance or contemporaneous metro-level shocks. The matched specifications test whether imbalanced covariates, rather than the listing itself, drive the treatment effect. If the permit-flow reduction were caused by some underlying feature correlated with imbalanced demographics, then reweighting the sample to remove those differences would attenuate the treatment effect. In fact, the matched estimates are as large or larger in absolute value than the baseline estimate. Entropy balancing, which forces equality of the treated and control covariate means in Table 3, produces the largest effects in the table. The geography$\times$year fixed effects test a separate concern that the treatment effect reflects contemporaneous shocks hitting metro areas where treated places happen to concentrate, rather than the effect of the species listing itself. I find that the estimate stays negative even when comparisons are restricted to places within the same metro area, though it does attenuate somewhat. These robustness checks support the straightforward reading of Figure 3 under the assumption of parallel trends. More important than robustness within the NLEB listing, however, is robustness across all the other listings under the Endangered Species Act. The next section tests for exactly this.

5 Stacked DiD

Since 1980, the Endangered Species Act has generated 661 endangered or threatened animal listing events with heterogeneous timing, geography, and repeated exposure as places accumulate multiple species. A single two-way fixed effects regression on the full panel is therefore hard to interpret. I instead use a stacked event-study design that isolates clean, one-time listing shocks and estimates the average change in permitting around these shocks.

An *event* is a unique Species ID s and listing year t for a *Full* animal endangered/threatened listing. For each event $e = (s,t)$, I build a balanced $\pm$8-year panel around the listing year t. Treated places are those that intersect species s habitat and receive exactly one full listing shock in the entire $[t-8, t+8]$ window and controls are places with zero full listing shocks in the same window. This window restriction removes overlapping treatments that would otherwise confound dynamics and mix different regulatory exposures. Because the outcome is annual, treatment is assigned by calendar listing year. Any within-year timing, like a species listing that begins six months into the treatment year instead of in January, attenuates the first post-period and biases estimates towards zero. A place can appear multiple times across different events, so standard errors are clustered by place. I weight by 1980 population so the estimand is the effect on the average resident rather than the average permitting office. I keep only events with meaningful treated support ($n_{\text{treated}} > 20$) to avoid unstable event-specific comparisons. The stacked sample contains 4.7 million place–event–year observations.

Table 5. Pre-treatment Balance in the Stacked Sample

Covariate	Scale: SD	Scale: Permit assoc.	Means: Treated	Means: Control	Treated − control difference: Original	Treated − control difference: Matched	Treated − control difference: Entropy
Panel A. Levels							
Permit rate	14.09	10.48	6.13	5.04	1.09^{***}	-0.31	0
Housing stock	0.08	0.57	0.42	0.41	0.01^{**}	0.01^{**}	0
Density	3790	-1.09	2240	1737	503	663	0
Population	640.4	-0.66	407.3	229.5	177.8	53.8	0
Urban share	27.7	-0.03	90.2	87.3	2.9^{***}	-1.6^{**}	0
Vacancy share	5.3	0.81	8.1	7.5	0.6^{***}	0.3	0
Owner share	16.8	1.01	59.6	61.5	-1.9	-0.4	0
Black share	17.9	-0.99	15.9	13.1	2.8^{*}	0.4	0
Under 18 share	4.5	1.41	24.5	24.5	0.0	0	0
Over 65 share	4.6	-2.05	13.2	13.8	-0.6^{***}	0.6^{**}	0
BSH elasticity	0.20	1.25	0.33	0.34	-0.02	0.03^{**}	0
Saiz elasticity	1.17	0.33	1.86	1.95	-0.10^{*}	0.12^{*}	0
WRLURI	0.73	-0.36	0.16	0.18	-0.02	-0.18^{***}	0
Log median income	0.56	2.23	10.51	10.44	0.07^{*}	-0.05	0
Log per-capita income	0.60	1.89	9.80	9.70	0.10^{**}	-0.02	0
Panel B. Pre-treatment changes							
Permit rate	1.96	0.44	0.09	0.14	-0.05	0.04	0
Housing stock	6.1	0.14	3.9	3.9	0.0	-0.4	0
Population	17.1	6.58	6.3	5.4	0.8	-0.7	0
Density	49.7	1.14	2.5	2.3	0.2	-0.4	0
Urban share	9.6	0.66	0.3	0	0.3^{***}	0	0
Vacancy share	2.6	-0.42	1.4	0.4	1.0^{***}	0.1	0
Owner share	3.3	-0.27	-0.9	-1.3	0.4^{**}	0.4	0
Black share	3.2	-0.26	1.1	0.9	0.2	0.2	0
Under 18 share	2.1	-0.12	-2.0	-2.0	-0.1	0.4^{***}	0
Over 65 share	2.3	-0.04	1.0	1.7	-0.7^{***}	-0.1	0
Median income	31.1	1.58	42.4	47.7	-5.3^{**}	-1.9	0
Per-capita income	37.5	1.71	49.6	58.6	-8.9^{***}	-3.3	0

Notes: This table reports population-weighted pre-treatment covariate balance in the stacked sample.

- *Sample:* Unit is a place-event in the year before treatment in the stacked sample. A place can appear in more than one listing event.
- *Rows:* Panel A reports pre-treatment levels. Panel B reports changes between two pre-treatment Census-decades in percent or percentage points, except permit rate, which reports the average annual change in permit flow over the pre-treatment period.
- *Columns:* SD is $\sqrt{(\sigma_T^2+\sigma_C^2)/2}$, the average within-group standard deviation of the covariate. Permit assoc. is the pooled pre-treatment association between permit rates and a one-standard-deviation increase in the covariate. Treated and Control report weighted means. Original, Matched, and Entropy report treated minus control differences. Propensity matching is within each event's treated-control risk set; entropy balancing matches treated covariate means and event composition.
- *Units:* Permit rate is measured in permits per 1,000 1980 residents. Housing stock is measured in units per resident. Population is measured in thousands. Shares are percentage points; share changes are percentage-point changes. Other changes are percent changes. BSH is the Baum-Snow-Han elasticity; WRLURI is the Wharton index.
- *Inference:* Standard errors are clustered by place. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Using this sample, I estimate the following population-weighted stacked event-study regressions:

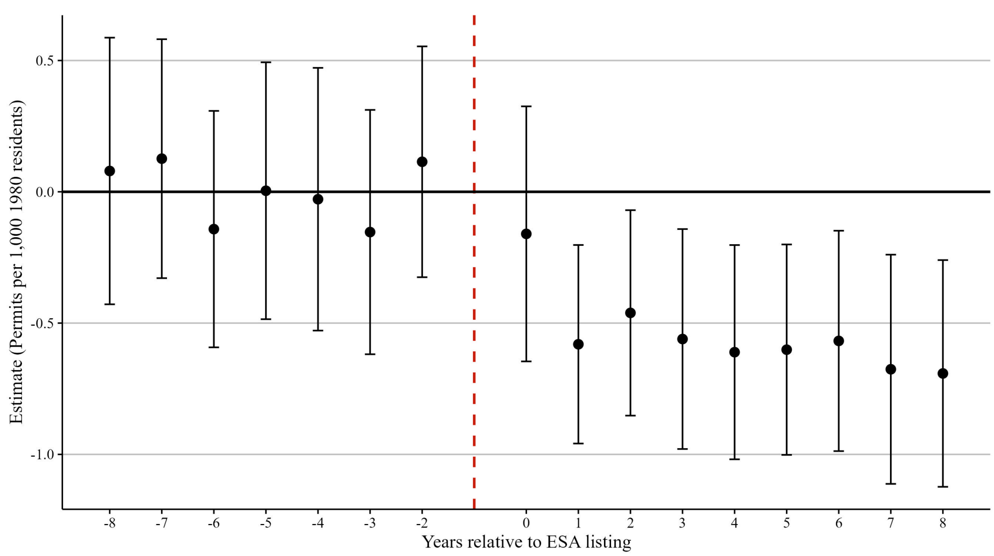
$$y_{iet} = \sum_{k=-8}^{8} \beta_k \left(T_{ie} \times R_{et}^{k}\right) + \alpha_{ie} + \lambda_{te} + \delta_{g(i)t} + \varepsilon_{iet}, \qquad k \neq -1 \tag{3}$$

$$y_{iet} = \beta \left(T_{ie} \times Post_{et}\right) + \alpha_{ie} + \lambda_{te} + \delta_{g(i)t} + \varepsilon_{iet}. \tag{4}$$

Here, y_{iet} is annual permits per 1,000 1980 residents, T_{ie} is an indicator for places treated in event e, R^k_{et} is an indicator for the calendar year k-years before the listing year of event e, α_{ie} is a place fixed effect, and λ_{te} is a year fixed effect, both of which are allowed to vary across events. I also add geography-by-year fixed effects, $\delta_{g(i)t}$, where $g(i)$ is either state or CBSA. These restrict the regression to comparisons between treated and control places within the same state or metro area, reducing the influence of sub-national shocks on the result. The preferred specification sets $g(i)$ equal to the 2010 CBSA.

The key identification assumption supporting this regression is that, absent a listing, treated and control places would have followed parallel trends in permitting, conditional on the fixed effects. Table 5 assesses this assumption by summarizing treated–control balance in the years before treatment. The pooled treated and control samples are more similar on observables than in the Northern Long-Eared Bat case study, and the sample is balanced on pre-treatment changes in permit flows, housing stock, and population. This supports the parallel-trends assumption, although significant differences in many other balance variables still remain. To assess whether this imbalance, rather than the causal effect of the ESA, explains the post-treatment divergence in treated-control permit flows, I run two matched specifications that enforce closer balance and check whether the main effects are robust in these specifications.

Figure 4. All Listings Stacked Event Study



Notes: This figure plots event-study coefficients for the stacked sample.

- *Sample:* In the stacked sample, treated place-events intersect the listed habitat and receive exactly one full listing shock in the event year, with no other full shock in the ±8-year window. Controls have no full shock in the window. Events are filtered for meaningful treated support ($n_{\text{treated}} > 20$).
- *Units:* Coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Coefficients are from equation 3 with CBSA-by-year fixed effects. The omitted year is the year before treatment. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

Figure 4 plots coefficients from the population-weighted stacked event-study regression in equation 3. The pre-treatment coefficients are all statistically indistinguishable from zero, indicating that treated and control places had similar permitting trajectories before listing. After listing, the coefficients turn uniformly negative and zero can be rejected in all but the first post-treatment year.[2] The negative values on this graph reflect that permit flows in treated places after a species listing are further below their pre-listing permitting rate than contemporaneous untreated places in the same metro area.

Table 6 reports the coefficients from nine different specifications of equation 4 and gives the headline magnitude. The baseline, propensity score matched, and entropy balanced specifications vary along the columns and the geographic level of the geography×year fixed effects varies along the rows. In the preferred CBSA×year specification, the average full listing exposure reduces permitting by 0.52 units per 1,000 1980 residents per year, about 9% of the mean flow in Table 1 and 12% of the 2025 average per capita permit flow [8].

Table 6. Treatment Effect Estimates in the Stacked Sample

	Sample construction		
Geography × year FE	**Original**	**Propensity matched**	**Entropy balanced**
National	-0.224	-0.378	-0.658**
	(0.151)	(0.227)	(0.254)
State	-0.393**	-0.392*	-0.705***
	(0.137)	(0.158)	(0.168)
CBSA	-0.521**	-0.253	-0.650***
	(0.200)	(0.184)	(0.192)
Treated place-events	10,222	10,125	10,175
Control place-events	305,234	21,039	295,445

Notes: This table reports treatment effects in the stacked sample.
- *Sample:* The stacked sample as defined in Figure 4 and summarized in Table 5.
- *Columns:* Column labels indicate sample construction. Original keeps all treated and control place-events in the stacked sample; propensity matched uses the matched sample from Table 5; entropy balanced reweights controls as in Table 5.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each cell is the coefficient on $T_{ie} \times Post_{et}$ from equation 4. Regressions include place-by-event, event-by-year, and the geography-by-year fixed effect in the row.
- *Inference:* Standard errors are clustered by place. Counts are place-events, not unique places. Event-study plots are in Appendix Figure 12. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The estimate becomes more negative, and roughly doubles in absolute value, as the geography-year fixed effects become more local (national → state → CBSA). If listing timing mainly reflected omitted local demand shocks, absorbing more of that demand variation would shrink the estimate towards zero. Instead, the effect strengthens, consistent with a supply shift that is masked when treated places are

[2] Many listings happen part-way through the first treatment year, so some attenuation in the first year of treatment is expected.

compared to controls in different markets with different cyclical paths.

All nine estimates are negative and range from 43% of the preferred estimate (-0.224, national×year, unmatched) to 135% of it (-0.705, state×year, entropy balanced). Three cannot be distinguished from zero at the 5% level and a fourth clears only that level; the other five reject zero at the 1% level or better. Two of the three insignificant cells do not use geography×year fixed effects, and so compare treated places to controls in unrelated housing markets. The third is the propensity matched CBSA estimate of -0.253, which is about half the absolute value of the preferred CBSA estimate. Propensity score matching drops over 90% of the control group to compare treated places with controls that seem to have a similar probability of treatment based on the pre-treatment balance variables.

Entropy balancing, which forces exact equality of the treated and control covariate means in Table 5, gives estimates 25–35% larger in absolute value than the preferred estimate. All three entropy balanced estimates are significant at the 1% level and nearly identical across the three fixed effect rows. Once the control group is balanced on observables, restricting comparisons to the same state or metro area does not move the estimate. Entropy balancing in the stacked sample doesn't raise the standard errors as much as it did in the NLEB sample because the treated and control groups are more similar to begin with and most listings have many more available controls per treated place than the NLEB. The event study figures corresponding to each of the nine estimates in Table 6 are available in Appendix Figure 12.

The stacked difference-in-difference design pools 661 listings over 45 years and finds an effect that is supported by close parallel pre-trends, is robust across matched samples and fixed-effect specifications, and is similar to the Northern Long Eared Bat case study from Section 4. Taken together, these results make a non-causal interpretation difficult. In the entropy-balanced specification with CBSA×year fixed effects, for example, the treatment effect is identified off of comparisons between treated and control places in the same metro area with the same average baseline characteristics. For the estimate not to reflect the causal effect of listing, unobserved shocks would have to arrive at the listing date, differentially reduce permitting in habitat-intersecting places relative to nearby non-intersecting places in the same housing market, and do so in a way that is not correlated with observed pre-treatment levels or trends.

This pattern of estimates is also consistent with the legal mechanism. A final ESA listing raises the expected cost, delay, and uncertainty of converting land into housing. In a simple model, developers file for permits when the expected value of a project exceeds the fixed and variable costs of approval and construction. Listings push marginal projects below that cutoff by increasing compliance costs, mitigation requirements, liability risk, and expected delay.

The next section further tests the predictions of the legal mechanism. In particular, I use variation in the legal bite of ESA listings to see whether effects are larger where the ESA's bite is strongest (critical habitat), smaller when protections are weaker (plant listings), zero where the statute is not yet enforceable (proposed/petitioned listings), and reversed when the legal constraint is removed (de-listings).

6 Empirical Tests of the Legal Mechanism

The stacked estimates provide evidence of a negative permitting response after full ESA listings: pre-trends are flat, the estimates survive increasingly local geography-by-year fixed effects, and the result is robust to balancing on pre-treatment covariate levels and trends. These features significantly narrow the scope for confounding, but they do not rule out all forms of endogenous selection into listing that might bias the results. Species listings may be correlated with unobserved forces that also affect housing production, including local environmental politics, development pressure on habitat, or other place-specific shocks. If these forces both predict ESA activity and reduce permitting through channels other than ESA enforcement, the stacked estimate could partly reflect selection rather than the causal effect of the listing itself.

This section tests for these remaining risks by measuring a dose-response relationship between the legal bite behind ESA listing decisions and the treatment effect on permit flows. The legal mechanism of the Endangered Species Act provides a rich set of doses at varying levels of regulatory force. Listings with critical habitat, listings of less restrictive plant species (*Partial listings*), and listings that are proposed but not yet enforced (*Proposed listings*) all have different legal effects, but many possible selection forces are correlated with all three types of listing. In particular, local environmental and development pressure that endangers species will lead to petitions, proposed listings, plant listings, animal listings, and critical habitat designations. Therefore, if these underlying forces are the source of the permitting decline, then permit flows should fall around ESA events regardless of whether the event substantially changes the legal constraint on development. Otherwise, if the legal mechanism is the source of the effect, the response should instead be ordered by legal bite.

Even more directly, species are sometimes removed from the endangered species list in a de-listing.

These de-listings are triggered by regular, internal, scientific reviews at the USFWS rather than by petitions from local environmental groups [37]. This removal of the regulatory force of the ESA provides another test of the headline result. If the main estimates are caused by ESA compliance costs, permitting should rise when the costs are removed. If instead they are caused by persistent local activism, growth pressure, or other selection forces, there is no reason for the effect to reverse at de-listing.

In the rest of this section, I re-run the same stacked construction under alternative "events" that differ in regulatory force but are built from the same habitat maps, the same permitting panel, and the same event-time window. If the main results are measuring the causal effect of stricter environmental land use regulation rather than the effect of underlying political activism, effects should be larger where the ESA's constraints are sharper, near zero where the statute is not (yet) enforceable on private development, and reversed when the constraint is removed.

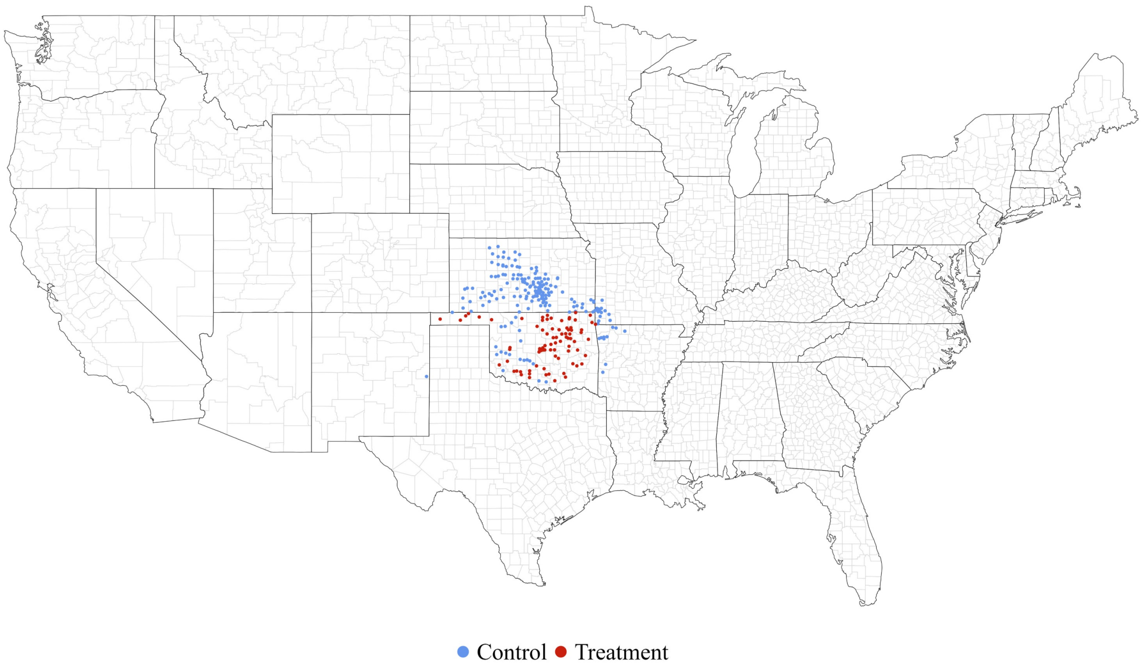
6.1 De-listings

I begin by looking at de-listings in detail. When a species is de-listed, Section 9 take liability and Section 7 consultation duties tied to that species end. Additionally, de-listings are triggered by regular, five-year internal reviews of the endangered species list that find either extinction or recovery for a listed species [37]. De-listings are rare; there have been 52 de-listings since 1980.

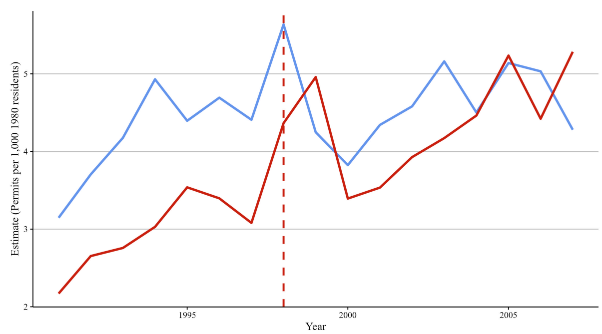
Figure 5 shows the results of a case study similar to the NLEB case in Figure 3 but for the 1999 de-listing of the Peregrine Falcon. The falcon was listed in the first tranche of listings under the ESA in 1967, with habitat for the endangered bird concentrated in the plains of Oklahoma. In the 8 years leading up to its de-listing the permit-issuing places inside this habitat followed the same regional trends as other places in Oklahoma, Kansas, and Missouri, but at a lower level. After the de-listing in 1999, the places relieved of their legal obligations to the Peregrine Falcon caught back up to their neighbors' permitting rates, adding about 1.1 permits per thousand 1980 residents to their annual permit flows.

Next, I extend to the full sample of de-listings and build a stacked difference-in-difference specification with de-listing events. For each event I form a balanced $\pm$8-year panel. Treated places are those that receive exactly one de-listing shock in the window and no full listing shock in the window. The estimating equation mirrors the stacked listing specification, with place$\times$event and event$\times$year fixed effects and geography$\times$year fixed effects, and standard errors clustered by place.

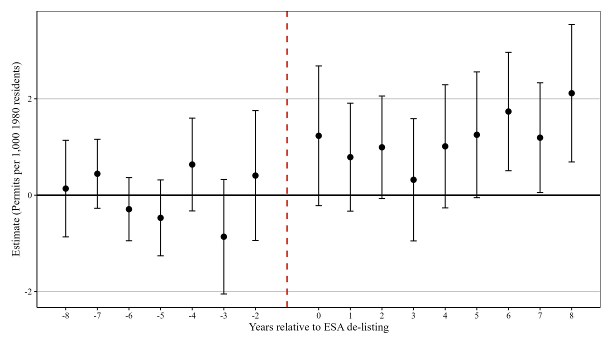
Figure 5. Case Study: Peregrine Falcon De-listing



(a) Treated vs Control Place Centroids



(b) Population-weighted levels



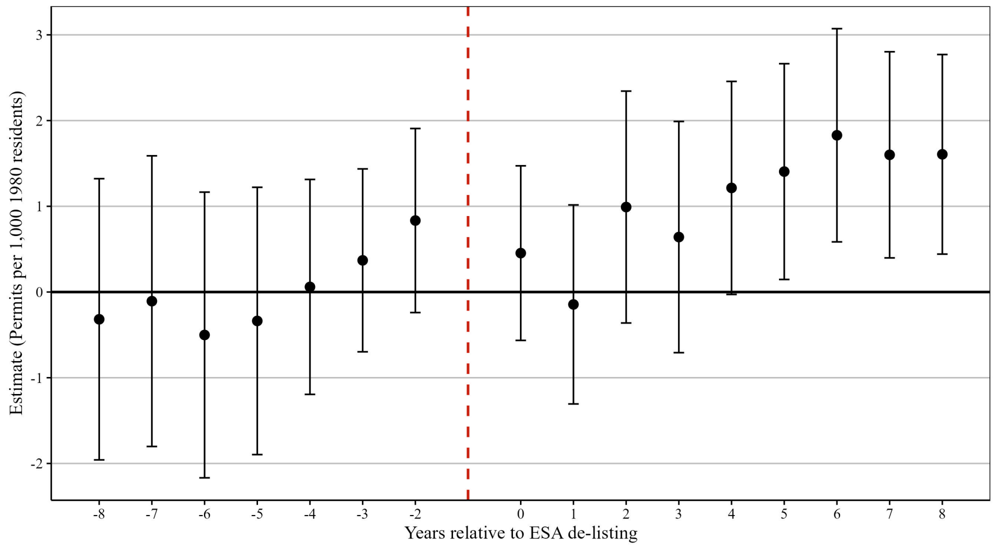
(c) Event-Study Coefficients

Notes: This figure maps places in the Peregrine Falcon sample, plots average permit rates over time, and reports event-study coefficients.

- *Sample:* Treated places intersect the Peregrine Falcon habitat at the 1999 de-listing, receive no full listing shock in the ±8-year window, and have fewer than five pre-existing full listings in the event year. Controls receive no de-listing shock in the event window, satisfy the same full listing restriction, and are restricted to shared CBSAs.
- *Panels:* Panel A maps place centroids. Panel B plots 1980-population-weighted permit rates. Panel C plots de-listing event-study coefficients.
- *Units:* Permit rates and coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Panel C omits the year before treatment. Averages and regressions are population-weighted.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

Figure 6 pools all de-listing events. Pre-trends are flat and the post-treatment coefficients shift upwards by about 0.8 permits per 1,000 1980 residents, similar to the magnitude of the main treatment effect but reversed in sign. Table 7 summarizes the average post effect across the same nine combinations of geographic fixed effects and matched samples as in Tables 4 and 6. All nine event study figures are available in Appendix Figure 13.

Figure 6. All De-listings Stacked Event Study



Notes: This figure plots event-study coefficients for the stacked de-listing sample.

- *Sample:* In the stacked de-listing sample, treated place-events intersect de-listed species habitat and receive a de-listing shock. Controls have no de-listing shock in the event window. Windows with full listing shocks are excluded.
- *Units:* Coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Coefficients are from the de-listing analog of equation 3 with CBSA-by-year fixed effects. The omitted year is the year before treatment. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

The de-listing treatment effect is always estimated to be positive, and is roughly centered around the same absolute value as the main treatment effect, but the estimates are very noisy and more sensitive to specification than the main effect. The estimates range from an increase of 0.161 annual permits per 1,000 1980 residents but indistinguishable from zero to an increase of 2.1 permits per thousand, significant at the 1% level. Less precise estimates are not surprising given how much lower the sample size of de-listing events is. There are 661 unique full listings since 1980 but only 52 de-listings, and many of these cannot be included in the sample because of confounding from overlapping listings in the pre/post period window.

This positive de-listing estimate is consistent with the legal mechanism and with a causal interpretation of the main treatment effect. This does not eliminate every possible confounder, especially given the significant noise and specification sensitivity, but it does substantially narrow the set of non-causal explanations because those explanations would have to generate effects of opposite sign at the moment the legal status reverses.

Table 7. Treatment Effect Estimates in the Stacked De-listing Sample

	Sample construction		
Geography × year FE	**Original**	**Propensity matched**	**Entropy balanced**
National	1.336^{***}	0.161	2.107^{**}
	(0.346)	(0.377)	(0.784)
State	0.606	1.446^{*}	1.461^{*}
	(0.761)	(0.637)	(0.713)
CBSA	0.845	1.741	0.699
	(0.512)	(1.317)	(0.719)
Treated place-events	1,566	1,544	1,566
Control place-events	101,467	5,177	101,467

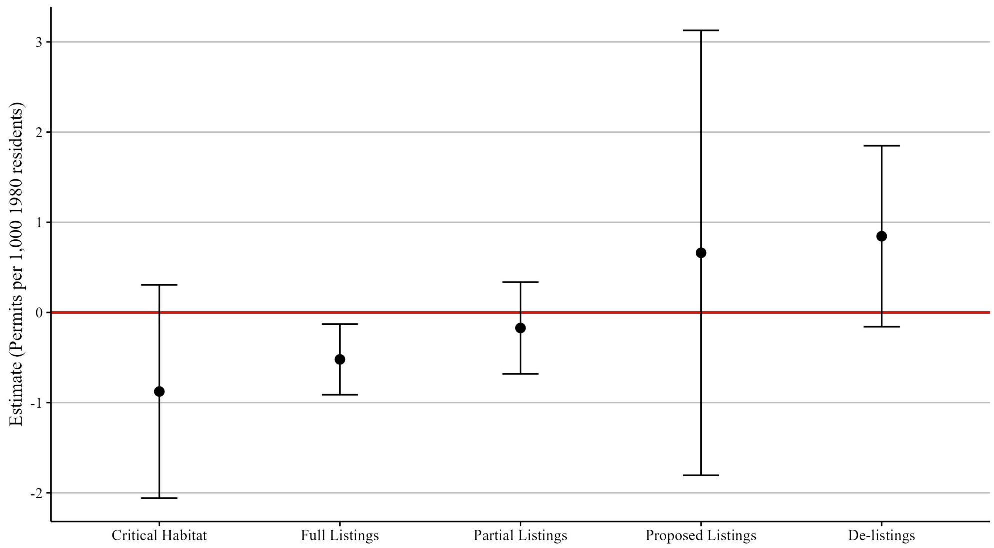
Notes: This table reports treatment effects in the stacked de-listing sample.
- *Sample:* The stacked de-listing sample as defined in Figure 6.
- *Columns:* Column labels indicate sample construction. Original keeps all treated and control place-events in the stacked de-listing sample; propensity matched uses the matched version of that sample; entropy balanced reweights controls.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each cell is the coefficient on $T_{ie} \times Post_{et}$ from the de-listing analog of equation 4. Regressions include place-by-event, event-by-year, and the geography-by-year fixed effect in the row.
- *Inference:* Standard errors are clustered by place. Counts are place-events, not unique places. Event-study plots are in Appendix Figure 13. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6.2 Critical, Partial, and Proposed Listings

In addition to the outright removal of endangered species protections in de-listings, there are other, finer gradations of compliance costs imposed by the ESA. Critical habitat listings impose additional restrictions over and above those associated with full listings, endangered and threatened plant species have fewer legal protections under Partial listings, and Proposed listings are not-yet-enforced. However, all three of these listing types can result from the local environmental politics or development pressure that might confound the main treatment effect. Therefore, the estimated treatment effect of these three listing types compared to the effect of standard listings and de-listings is informative about the extent of the possible confounding from local shocks that are correlated with listing. If the treatment effects are similar to the standard listing, then variation in the regulatory force of the ESA is not driving variation in permit flows and the causal story is less credible. On the other hand, if the treatment effects vary according to the predictions of the legal mechanism, the scope for non-causal interpretations of the result is narrowed.

Figure 7 compares five stacked ATTs, three new estimates for Critical Habitat listings, Partial (plant) listings, and Proposed listings, as well as the main estimate for full listings and the estimate for de-listings from Figure 6. Each of the three new estimates are derived from the same event-study machinery and the same outcome scale.

Figure 7. Treatment Effects by Legal Mechanism



Notes: This figure compares treatment effects across ESA events with different legal force.

- *Sample:* "Critical Habitat" restricts treated places to those near or inside critical-habitat geometry. "Full listings" reports the effect from Table 6; "Partial listings" are plant listings; "Proposed listings" are not-yet-enforced; "De-listings" reports the effect from Table 7.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each point is from a separate version of equation 4 with CBSA-by-year fixed effects.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

The largest negative effect appears when I restrict treated places to those near mapped critical habitat for the listed species. This is where the ESA is demanding for projects with a federal nexus, and where "take" under Section 9 is most likely. Critical habitat shifts expected costs up and increases the likelihood of long, information-intensive review. A more negative effect in this subset is the pattern the legal mechanism predicts.

Partial (plant) listings have much weaker bite for private housing because Section 9 take does not generally apply to plants on private land. If the main Full-listing estimate were driven by unobserved local shocks correlated with environmental activism, for example, plant listings should also depress permitting. In the data, the effect estimate for Partial listings is near-zero.

Proposed listings communicate information and are often the result of political petitioning, but do not carry enforceability. The near-zero Proposed estimate rejects large systematic anticipation and provides evidence against endogeneity. That is consistent with the legal mechanism and with the event-study timing in Section 5. The change in permitting flows occurs when the listing becomes enforceable rather than when the proposal is published.

The five ATT estimates in Figure 7 match closely in both order and magnitude to what is predicted by a causal connection between ESA listings and increased costs of housing construction, rather than what would be predicted by endogenous factors driving the result. Only the full listings result is statistically distinguishable from zero, though a near-zero effect is expected for partial and proposed listings. This increased noise is expected given the far smaller sample sizes available for the other listing types. Despite the wide confidence intervals, the ordering of the point estimates is consistent with a causal connection between the legal prohibitions of the ESA and reduced permitting flows.

6.3 Heterogeneity

Table 8 reports how the treatment effect differs across four cross-sectional slices of the main full listing regression sample. These heterogeneity tests are useful both as further tests of the legal mechanism and as an accounting of what kind of species listings and places are driving the overall average effect. Each panel reports the coefficient on $T_{ie} \times Post_{et}$ from the stacked event study specification in equation 4 with CBSA$\times$year fixed effects, but where the sample is restricted to match the characteristic listed in the row. Group assignments use pre-treatment information (or attributes of the treatment event itself) so they do not condition on post-treatment outcomes.

The heterogeneity across species groups is large. Bird listings reduce permitting by 1.38 units per 1,000 residents, larger than the mammal baseline and much larger than the null effect from Clam listings. It is harder to rule out the possibility of harm or harassment to highly mobile species than it is for species that live in stationary river beds. Bird species also tend to have large habitats, so they make avoiding habitat by moving the location of projects more difficult. Aquatic taxa show unstable and sometimes positive point estimates, consistent with more predictable and less costly prohibitions when habitat polygons intersect places primarily through water or narrow riparian corridors that are not the relevant margin for most housing production.

Heterogeneity by pre-existing listing stock does not show any evidence for decreasing costs as species stocks increase, though this result is statistically weak. On the margins most common in my sample, this is consistent with the legal mechanism where costs to housing come from the liability risk of discovering endangered species near your plot or from the direct costs of permitting and mitigation measures under habitat conservation plans. Adding overlapping species habitats makes discovery more likely, and diminishing marginal risk is not guaranteed if the baseline probability is low. Other compliance costs like mitigation fees also scale with species counts without clear forces pushing towards diminishing marginal costs. Eventually, finding endangered species becomes a certainty. In Maui County, for example, there are over 180 endangered plants and animals. There, the costs of an additional listed species are likely small, but almost all places have a stock between 1 and 7 listed species. On this margin, additional species listings add substantial additional costs to housing construction, regardless of pre-existing stock.

Table 8. Heterogeneity Analysis

Group	**Treated** (Count)	**Population** (Weight)	**Estimate** (SE)	**Difference vs. base** (SE)
Panel A. Species group				
Mammals	4,259	303,349	-0.256 (0.187)	—
Birds	2,516	369,403	-1.382 (0.405)	-1.126 (0.415)
Reptiles	1,735	174,380	-0.280 (0.177)	-0.024 (0.239)
Insects	771	101,771	-0.648 (0.615)	-0.392 (0.641)
Clams	520	38,650	0.135 (0.448)	0.391 (0.482)
Fishes	294	19,945	1.182 (0.490)	1.438 (0.524)
Panel B. Existing listings stock				
0	1,235	80,120	-0.274 (0.273)	—
1–2	3,693	348,792	-0.425 (0.308)	-0.151 (0.352)
3–5	4,692	491,187	-0.613 (0.225)	-0.339 (0.305)
6+	602	103,855	-0.733 (0.463)	-0.460 (0.529)
Panel C. Habitat area quintile				
Q1	419	30,455	-0.030 (0.281)	—
Q2	634	57,938	1.085 (0.505)	1.115 (0.573)
Q3	1,954	242,036	-0.400 (0.307)	-0.369 (0.415)
Q4	838	75,674	0.657 (0.406)	0.688 (0.487)
Q5	6,377	617,850	-0.982 (0.293)	-0.952 (0.406)
Panel D. Vacancy tercile				
LowVac	2,946	317,393	-0.225 (0.189)	—
MidVac	3,322	451,045	-0.585 (0.270)	-0.360 (0.263)
HighVac	3,907	247,342	-0.917 (0.319)	-0.692 (0.322)

Notes: This table reports heterogeneity in treatment effects across species and place characteristics in the stacked sample.
- *Sample:* The stacked sample as defined in Table 5. Groups are defined at the treated place-event level using listing attributes or pre-treatment place characteristics.
- *Columns:* Treated counts treated place-events. Population sums 1980 population across treated place-events. Difference vs. base subtracts the first row in the panel.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each panel estimates equation 4 with $T_{ie} \times Post_{et}$ interacted with the listed group. Regressions include place-by-event, event-by-year, and CBSA-by-year fixed effects and are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place.

Habitat-area heterogeneity is stronger and statistically significant. The largest habitat quintile produces a large negative effect (-0.98 permits per capita) while the smallest habitat species have a precise zero effect. Broad-range listings are more likely to cover the developable margin and to be difficult to avoid.

Vacancy patterns are also consistent with a causal interpretation of the effect of species listings. Effects are larger where vacancy is high: -0.92 permits per capita in the highest vacancy tercile vs -0.23 in the lowest. When expected rents are low and demand is slack, small increases in expected cost and delay push more projects below the profitability cutoff.

Taken together, the legal-mechanism tests are consistent with the assumption that the treatment effect is the causal result of the land use regulations brought by endangered species listings. The effect is larger when ESA constraints are sharper, near zero when legal bite is weak or not yet enforceable, reversed when protections are removed, and strongest in the places and listings where permitting costs should be most likely to bind. These patterns do not mechanically rule out every selection concern, but they make the remaining non-causal story much more restrictive: unobserved conservation politics, growth pressure, or agency attention would have to generate the same timing and the same ordering by legal enforceability, habitat incidence, species type, and local permitting margins.

7 Mistargeting

At the extensive margin of housing production, where low-density developments fill in greenfield sites, the tradeoff between natural habitats and housing production is binding; each new house occupies a lot that might have otherwise hosted an endangered species. This is a difficult tradeoff to assess and this paper only covers one half of it; the cost of species listings to housing production, not the benefit of listings.

However, at the intensive margin of housing production, new developments are often replacing existing buildings or are filling in space in a highly developed area that could not host endangered species even if no new construction took place. On the intensive margin, the tradeoff with species protection does not bind, and may even be positive sum as it substitutes for less dense greenfield development. Therefore, whether and how much the ESA constrains development on the extensive vs intensive margin is relevant to the tradeoffs we face between housing production and species protection, and thus is relevant to the aggregate welfare effects of the law.

In this section I extend the main empirical specification of the paper to satellite data on land use from the National Land Cover Database (NLCD) [23] and to heterogeneity within the Building Permits Survey to assess where the effects of the Endangered Species Act are accruing.

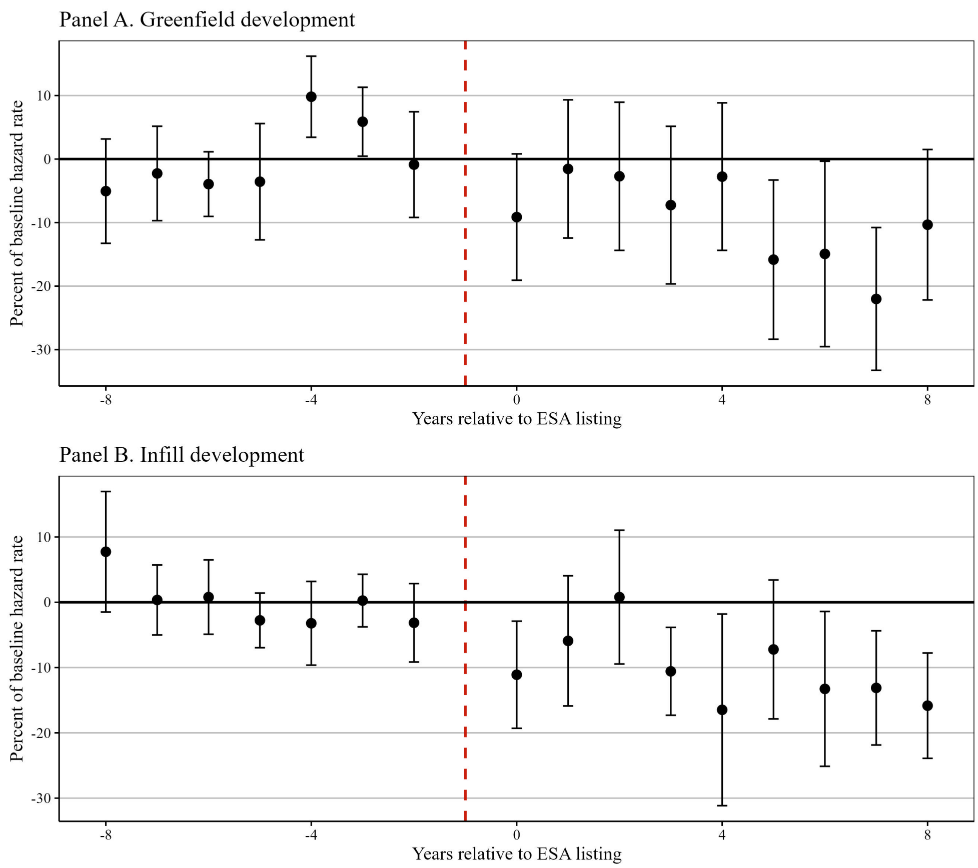
The NLCD is a set of satellite images of the United States compiled and pre-classified by the U.S. Geological Survey. They classify 30-square-meter pixels into one of fifteen land use groups, including four levels of development, three types of forest, and two types of wetland. The NLCD has annual files going back to 1985. I overlap these pixels with the map of permit-issuing places in the BPS using constant 2024 borders, and track the changes to pixels within each place over time. The hazard rate of extensive margin or greenfield development is measured by the flow of non-developed pixels (e.g. forests or wetlands) into any of the four levels of developed land use, divided by the total area of greenfield land use. This measures the chance that any given greenfield pixel will transition into a developed pixel in a given year. Note that my definition of greenfield land does not include agricultural land since, for the purpose of endangered species habitat, agricultural land is highly controlled and developed despite not having many physical structures on it. Similarly, infill development is measured by the flow of developed open space into higher intensities of developed land use, also normalized by the total area of developed open space.

I run the same regression specification as in equations 4 and 3 with the dependent variable of permit flows replaced with the measures of greenfield and infill hazard rates defined above. Figure 8 and Table 9 report the results. After an endangered species listing, greenfield sites are around 10% less likely to transition into developed land use in any given year compared to untreated controls in the same metro area. This is expected given the previous results on permit flows and the intention of the ESA to prevent harm, harassment, or habitat destruction for endangered species. However, infill sites are also about 10% less likely to transition into more intensely developed land use types. The bottom section of Table 9 reports the denominator of these percentage effects. Before treatment, the average place converts about 0.34% of its non-agricultural greenfield land into developed land use each year and about 1.2% of its developed open space into a higher intensity of developed land use. After treatment these hazard rates decrease by about 0.034 and 0.12 percentage points, respectively.

The sample size of land use classification changes is small, so these estimates are imprecise. The population-weighted average treated place in the sample sees land use changes in only about 1% of its area each year, or 2.16 km^2 of land use changes out of 227.5 km^2 total area. Greenfield and infill developments are a subset of this two square-kilometer flow. The average treated place in the year before treatment develops about 0.174 km^2 of greenfield land and 0.276 km^2 of infill land. The CBSA treatment effect estimates therefore correspond to reductions of roughly 0.017 fewer square kilometers of greenfield transition and 0.025 fewer square kilometers of infill transition, about 8 football fields of combined area per year.

Still, the fact that the effect on infill development may be as large as the effect on the greenfield development rate is not consistent with the interpretation that ESA listings simply redirect construction from greenfield into infill. Listings reduce extensive land conversion and also slow redevelopment and upzoning inside already-developed land. Since infill development is unlikely to have negative effects on endangered species habitat, and may even have positive effects, the impact of species listings on infill development rates suggests substantial mistargeting of the effects of the ESA and room for a Pareto improvement in a tradeoff between housing production and species protection.

Figure 8. Greenfield and Infill Event-Study Coefficients



Notes: This figure plots event-study coefficients for greenfield and infill land-cover transitions.

- *Sample:* The stacked sample as defined in Table 5.
- *Panels:* Greenfield is forest, shrub/scrub, or wetland pixels becoming developed. Infill is developed open-space pixels becoming higher-intensity developed.
- *Units:* Coefficients are reported as percent changes relative to the pre-listing mean hazard rate.
- *Specification:* Regressions follow equation 3 with land-cover transition outcomes and CBSA-by-year fixed effects. The omitted year is the year before treatment. Weights are 1980 population times the source-pixel share.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

Table 9. Treatment Effect Estimates on Greenfield and Infill Development

	Greenfield Development			*Infill Development*		
	(1) Nat'l	**(2) State**	**(3) CBSA**	**(4) Nat'l**	**(5) State**	**(6) CBSA**
$T_{ie} \times Post_{et}$	-3.0	-5.6	-10.0*	-9.1**	-8.6**	-9.2**
	(3.5)	(3.1)	(4.3)	(3.2)	(2.9)	(3.0)
Fixed effects						
Place × event	Yes	Yes	Yes	Yes	Yes	Yes
Event × year	Yes	Yes	Yes	Yes	Yes	Yes
Geography × year	National	State	CBSA	National	State	CBSA
Pre-listing mean hazard rate	0.340	0.340	0.340	1.184	1.184	1.184
Treated place-events	9,560	9,560	9,560	9,819	9,819	9,819
Control place-events	274,188	274,188	274,188	286,857	286,857	286,857

Notes: This table reports treatment effects on NLCD land-cover transition rates.
- *Sample:* The stacked sample as defined in Table 5.
- *Columns:* Greenfield is forest, shrub/scrub, or wetland pixels becoming developed. Infill is developed open-space pixels becoming low-, medium-, or high-intensity developed.
- *Units:* Treatment effects are reported as percent changes relative to the pre-listing mean hazard rate. The hazard rate is the annual share of source pixels that transition.
- *Specification:* Each cell is the coefficient on $T_{ie} \times Post_{et}$ from the land-cover analog of equation 4. Regressions include place-by-event, event-by-year, and column-specific geography-by-year fixed effects. Weights are 1980 population times the source-pixel share.
- *Inference:* Standard errors are clustered by place. Event-study plots are in Appendix Figures 14 and 15. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I also look to two other sources of evidence on the incidence of ESA land use regulations on infill vs greenfield development. First, in Table 10 I split the main regression sample by NLCD developed share, which is the proportion of each place's area that is classed as developed at low, medium, or high intensity, and re-run the specification in equation 4 with CBSA×year fixed effects. The top panel summarizes the sample size, population weight, treatment probability, and developed share within each slice of the regression sample. In the bottom panel, the "All" column reproduces the results from Table 6 for comparison and the other columns report the results within each sub-sample.

It is notable that the probability of treatment is slightly higher in the most developed places than it is in places with below median development intensity. Moreover, the effects of ESA listings on permit flows are at least as large in places with a higher pre-treatment development share than they are in the full sample. Places with a below median developed share have smaller effects, and even a positive point estimate in the baseline specification, though this is not robust to matching.

These results are consistent with the claim that endangered species listings constrain infill development, though they may also be picking up a mechanical effect where the space available for development in already-developed places has fewer substitutes than in more sparsely developed places. When a species listing raises development costs in some areas, it may be easier to find substitutes in sparsely developed places than in those with little open space to begin with.

Table 10. Treatment Probability and Effects by Baseline Developed Intensity

	Baseline developed-intensity sample			
	All	**Below median**	**Above median**	**Above 75th pct.**
Median developed share	53.0%	30.0%	75.3%	87.0%
Treatment probability	3.2%	3.1%	3.7%	3.8%
Treated place-events	10,222	7,670	2,552	811
Treated population, 1980 (mil.)	122.5	54.0	68.5	36.7
Control place-events	305,234	238,760	66,474	20,277
Effect on permits per 1,000 1980 residents, CBSA × year FE				
Original	-0.521**	0.489*	-0.533*	-0.749*
	(0.200)	(0.202)	(0.230)	(0.312)
Propensity matched	-0.253	-0.017	-0.377	-0.708
	(0.184)	(0.210)	(0.361)	(0.373)
Entropy balanced	-0.650***	-0.259	-0.559*	-0.954*
	(0.192)	(0.264)	(0.261)	(0.383)

Notes: This table reports treatment probability and treatment effects by baseline developed intensity.
- *Sample:* The All column uses the stacked sample from Table 5. Split columns restrict both treated and control place-events to the displayed baseline developed-intensity sample.
- *Columns:* Baseline developed intensity is the share of place area in low-, medium-, or high-intensity developed NLCD classes in the year before treatment; developed open space is excluded. Missing baseline years are filled from the same place when possible, otherwise from the nearest-year weighted median. The top rows report sample composition; the bottom rows report treatment effects.
- *Units:* Treatment effects are measured in annual permits per 1,000 1980 residents. Treated population is summed 1980 population across treated place-events.
- *Specification:* Treatment effects use equation 4 with CBSA-by-year fixed effects and 1980 population weights.
- *Inference:* Standard errors are clustered by place. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The final specification I use to investigate the incidence of ESA land use regulation on the extensive vs intensive margin of housing construction uses heterogeneity in the outcome variable of building permits rather than heterogeneity in the treated and control population. The Building Permits Survey reports the number of housing units permitted in each place, split by the size of the building those units are in: 1-unit single family homes, 2-unit duplexes, 3-4 unit three-deckers, and 5+ unit apartment buildings. Table 11 decomposes the total-units effect by structure type. The decline in permitting activity after a species listing is not confined to small single-family projects. The 5+ unit category falls by 0.26 units per 1,000 residents, which is a 16% decrease in the average permit flows for that structure type, larger than the overall 10% effect. This is consistent with the claim that species listings are slowing infill developments because dense 5+ unit constructions are more common in infill construction, though it is not conclusive because large buildings can be built on greenfield sites as well.

These results are consistent with substantial mistargeting of land use regulations under the ESA to dense, infill construction, though none of the results are individually decisive. In particular, the NLCD estimates are imprecise, the developed-share split may partly reflect a mechanical scarcity of substitute land in built-up places, and the structure-type decomposition cannot separate infill from dense greenfield projects. Still, the NLCD transition-rate regression shows no evidence that effects on intensive margin development are any smaller than effects on the extensive margin, and the two heterogeneity analyses show larger effects in the subgroups associated with more intensive margin development.

Table 11. Heterogeneity by Residential Structure Type

	Residential structure type			
	(1)	**(2)**	**(3)**	**(4)**
	1-unit	**2-unit**	**3–4 units**	**5+ units**
$T_{ie} \times Post_{et}$	-0.203	-0.036*	-0.021	-0.261*
	(0.143)	(0.014)	(0.016)	(0.117)
Fixed effects				
Place × event	Yes	Yes	Yes	Yes
Event × year	Yes	Yes	Yes	Yes
Geography × year	CBSA	CBSA	CBSA	CBSA
Pre-listing mean permit rate	3.344	0.161	0.145	1.632
Average share of stock	0.691	0.050	0.032	0.227
Treated place-events	10,222	10,222	10,222	10,222
Control place-events	305,234	305,234	305,234	305,234

Notes: This table reports treatment effects by residential structure type.
- *Sample:* The stacked sample as defined in Table 5.
- *Units:* Treatment effects and pre-listing means are measured in permits per 1,000 1980 residents. Average share of stock is the unit share in each structure type in the population-weighted average place.
- *Specification:* Each column is the coefficient on $T_{ie} \times Post_{et}$ from equation 4 with CBSA-by-year fixed effects. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The implications of these results on the welfare effects of the Endangered Species Act and the possibility for Pareto-improving reforms are discussed further in Section 9.

8 Aggregation

The stacked difference-in-difference design identifies a local, partial-equilibrium treatment effect. Permitting flows fall in treated places relative to controls in the same event window and metro-year. However, if the development prevented by ESA listings is simply displaced to somewhere else in the country, then the place-level ATT may be overstated, since treatment may simultaneously decrease permitting in treated places and increase it in controls if permitting relocates from one to the other. Even if the control group is not contaminated by extra permits fleeing treatment, permits may still be displaced to somewhere else in the country outside of the regression sample. Then the ATT would be correct for the treated places but simply adding up that treatment effect over all species listings would still overstate the number of permits that are deterred from entering the national housing stock.

To address these concerns, I estimate the extent of possible displacement of permits from treated places to other parts of the country. First, in Panel A of Table 12 I aggregate permits to broader geographies g (CBSA, State, and EPA region) and construct an index of species listing exposure:

$$Y_{gt} = \beta_{\text{agg}} X_{gt} + \alpha_g + \lambda_t + \delta_g t + \varepsilon_{gt}, \tag{5}$$

$$X_{gt} = \sum_{i \in g} \text{Population Share}_i \times \text{Full}_{it}. \tag{6}$$

Here, X_{gt} is the "mass" of species listing treatment within each geography g and year t. This is equal to the population-weighted sum, across all permit-issuing places i within the geography, of the number of full species listings in each place. If the geography g is a metro area, then $X_{gt} = 1$ when an endangered species habitat listed in year t covers the entire metro area and all the places inside it are treated. $X_{gt} = 1/2$ when only half of the population in a metro area is within listed habitat. I then regress aggregated permitting Y_{gt} on X_{gt} with geography fixed effects α_g, year fixed effects λ_t, and geography-specific linear trends δ_g.

The coefficient $\hat{\beta}_{\text{agg}}$ measures the same relationship between species listings and permit flows as the place-level ATT, just at a broader geographic level. However, in this case treatment X_{gt} is usually between zero and one rather than binary. When treatment covers only part of a geography, any displacement of permits from treated places to untreated places within a city, state, or region will attenuate $\hat{\beta}_{\text{agg}}$ toward zero. If, on the other hand, there is no displacement and the aggregate effect on, say, state-level permitting is equal to the sum of all of the place level effects, then the coefficient on the sum term X_{gt} in equation 5 should be equal to the coefficient in equation 4. Therefore, the ratio $\hat{\beta}_{\text{agg}}/\hat{\beta}_{\text{place}}$ can be read as the share of the local treatment effect that comes from deterred permits which do not show up elsewhere.

Panel A of Table 12 reports these deterrence estimates. At the CBSA level, the point estimate of -0.252 implies a deterrence share of 0.48, though it is imprecise. This implies that if, for example, a single town of 1,000 people within a metro area is treated with endangered species habitat and loses 0.52 permits per year (the full treatment effect from Table 6) then the metro area itself will also lose permits, but only around half as much: around -0.25 per year. The other half of the local treatment effect is offset by permits that are displaced elsewhere within the metro area. At the state level, the deterrence share is more precisely estimated and is larger at about 70% and statistically indistinguishable from 1. The EPA region estimate is almost exactly the same as the state level. This pattern is consistent with some within-market displacement, but it is also consistent with full deterrence observed through some statistical noise that attenuates the result.

Panel B investigates the low deterrence share at the CBSA level further by decomposing the total change in permit flows at the metro area into permitting from treated and control places. Almost all of the change in CBSA-level permitting comes from listed jurisdictions. Control places show zero offsetting increase. This rejects the extreme displacement story in which ESA listings merely move construction across municipal boundaries within the same metro and points more towards attenuation as the cause of the low deterrence share. When permitting falls in listed places it is not replaced by a surge in unlisted places within the same market.

Table 12. Aggregation and Spillover Checks

	Estimate	**SE**	**Deterrence share**	***p*-value**	***N* geos**	***N* obs**
Panel A. Aggregate deterrence share						
CBSA	−0.252	0.209	0.483	0.230	364	16,380
State	−0.368	0.094	0.707	0.000	51	2295
EPA region	−0.387	0.211	0.744	0.067	10	450
Panel B. CBSA decomposition						
Total effect (all places)	−0.252	0.209	–	0.230	12,664	569,880
Effect: listed places only	−0.234	0.216	–	0.278	12,094	506,007
Effect: unlisted places only	−0.018	0.013	–	0.165	2194	63,873

Notes: This table reports aggregation and spillover checks for the place-level treatment effect.
- *Rows:* Panel A aggregates permits and exposure to the CBSA, state, or EPA-region level. Panel B decomposes CBSA totals into listed and unlisted places.
- *Columns:* Exposure mass is the 1980-population-weighted sum of Full listing stock across places in the geography. Deterrence share is $\hat{\beta}_{agg}/\hat{\beta}_{place}$, using the preferred place-level treatment effect from Table 6.
- *Specification:* Panel A regressions include geography fixed effects, year fixed effects, and geography-specific linear trends. Panel B uses the same CBSA-level exposure and separates permitting by listed status.
- *Inference:* Standard errors are clustered at the regression geography. Panel B clusters by CBSA.

In addition to the empirical tests of displacement and deterrence, there are other reasons to doubt that cross-place spillovers are explaining most of the results. The largest habitat listings have the largest effects and the most weight in the averge treatment effect, but these are also the least subject to spillovers. Developers may easily find alternative sites that avoid a small endangered clam habitat along a riverbed, but a habitat that spans half of the country like the NLEB is much harder to avoid. Additionally, a large fraction of all populated areas in the country are covered by at least one species habitat, so it is often not possible to avoid compliance costs from the ESA by moving projects to a different area.

To translate the treatment effect on the annual flow of housing permits within a place into an estimate of national permit losses, I use the following accounting:

$$\Delta\text{Permits}_i = \overbrace{\hat{\beta} \times \text{Deterrence} \times \text{Population (1980)}_i}^{\text{per-listing treatment effect}} \times \overbrace{\sum_t f(\text{Full Stock}_{it})}^{\text{effective number of listings}} \tag{7}$$

The total loss in permits from place i is the product of two terms. First is the per-listing treatment effect on total permits. This is the place-level per capita effect on permit flows, $\hat{\beta}$, multiplied by the share of that effect coming from actual permit loss rather than relocation, Deterrence, and by the 1980 population of each treated place. The second term tracks the number of times that the per-listing treatment effect is applied to each place. $f(\text{Full Stock}_{it})$ is the "effective" stock of listings in each place-year, where $f(\cdot)$ can account for diminishing marginal costs and the sum cumulates the annual flow effect over all the years post-treatment.

I use the 1980 population scale here to match with the analysis in the rest of the paper. However, if the effect of a species listing scales with a place's current population and permit flow, rather than with its 1980 population, then multiplying the treatment effect by 1980 population will understate the total permit effect in fast-growing places. If Phoenix Arizona gets a species listing in 2010, for example, and it reduces permit flows by -0.5 units per thousand 2010 residents, that will cause a larger reduction in total permits than the accounting above, which assigns an effect -0.5 units per thousand 1980 residents to that 2010 listing. Conversely, $\hat{\beta}$ itself may be overstated in Phoenix because a listing that reduces permit flows by -0.5 units per thousand 2010 residents will look like a larger decline in permit flows per 1980 residents.

In Appendix Section 11.1, I repeat the main stacked event study specification but replace the fixed 1980 normalization and weights with population numbers from the nearest pre-treatment decade for every listing. The result is very similar: -0.508 permits per capita vs -0.52 permits per 1980 population. It turns out that the modest upward bias in the 1980-normalized specification due to population growth is concentrated in high-growth places which also tend to have smaller treatment effects, so the overall bias is small. When I apply the alternative treatment effect to the aggregation accounting above, the slightly smaller coefficient gets multiplied by slightly higher contemporaneous populations, and the estimated number of lost permits is also similar.

Table 13 reports the estimate of cumulative permit loss under a range of assumptions on the treatment effect, the deterrence rate, and three models of how the marginal effects of species listings accumulate. The columns vary the size of the treatment effect according to the 95% confidence interval around the headline estimate in Table 6. The panels aggregate the given treatment effects according to different models of the diminishing costs of species listings. The constant rule assumes each additional listing adds the same marginal burden, consistent with the results in Table 8. The piecewise rule sets marginal effects to zero after four listings. The exponential rule allows marginal effects to decay smoothly at rate $\lambda = -0.10$ as places accumulate listings. Under this rule, the first listing is about twice as costly as the sixth listing, consistent with some fixed costs, learning, and bundling of compliance. Within each panel, I first report the total number of displaced or deterred permits from multiplying the treatment effect over all treated person-years since 1980. This is the total number of permits that were lost in treated places, but these local losses may or may not have shown up as gains in other places. I then report the implied loss to national housing stock under a range of assumptions on the deterrence rate informed by the excercise in Table 12.

There is substantial uncertainty and modeling sensitivity on several of the terms in equation 7, and these errors compound when multiplied together, so these estimates should be interpreted with caution as order-of-magnitude approximations. Under a 70% deterrence share and an exponential stock rule, the bounds of the 95% confidence interval for the preferred treatment effect imply a loss of 1.6 to 11.1 million units. Across all treatment effects, deterrence shares, and listing-stock rules reported in Table 13, the implied loss to the national housing stock ranges from 0.6 to 13.8 million units. The preferred calibration is highlighted in Panel B and produces a central estimate of roughly 6.3 million missing units over 1980–2024, about 4% of the 2025 housing stock.

This effect is large but not unprecedented in the literature. Duranton and Puga show that relaxing New York's planning regulations to the 75th percentile level among American cities would increase its population by 7.5 million [7]. Their model predicts that New York City alone would increase its permitting by more than my predicted national effects from the Endangered Species Act over a 45-year period. Doing the same to San Francisco would increase its population by 2.5 million and its permitting flows by a similar amount. Legal context and correspondence with developers makes it clear that the Endangered Species Act is only one part of a larger set of land use regulations that constrain housing construction. Reassuringly, the predicted aggregate effects of rolling back ESA protections to 1980 fit with this sense when compared to estimates in the literature of the effect of rolling back the entire set of planning regulations.

Table 13. National Permit Effects

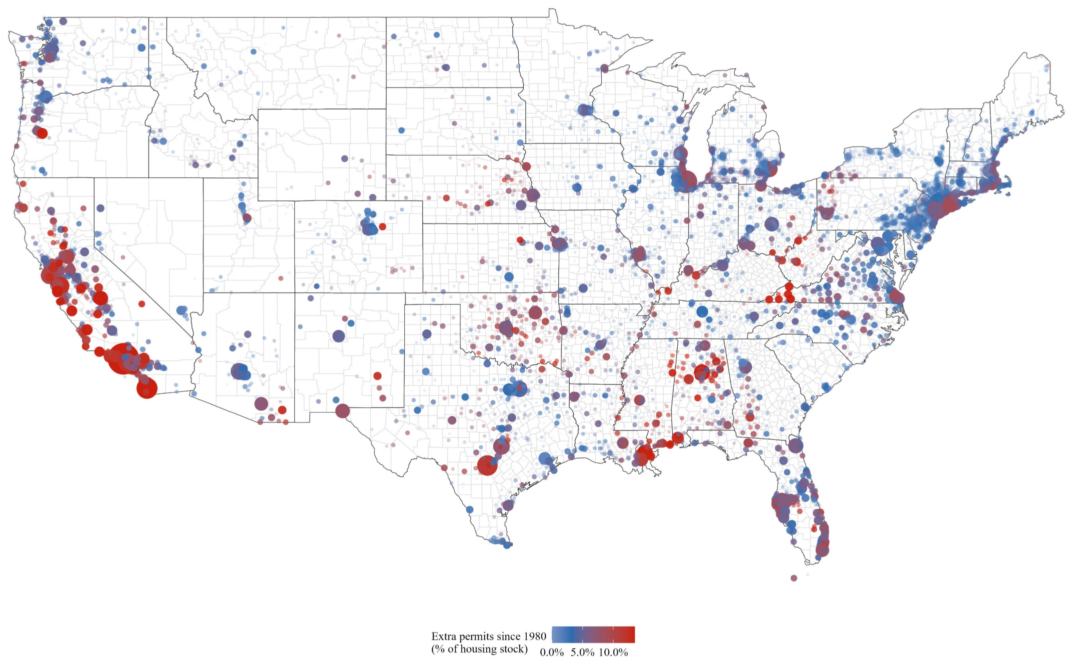
National effect	High $\hat{\beta} = -0.913$	Central $\hat{\beta} = -0.521$	Low $\hat{\beta} = -0.128$
	Treatment effect		
Panel A. Constant listing-stock rule			
Total displaced or deterred	19.7	11.2	2.8
Implied reduction in housing stock			
30% deterred	5.9	3.4	0.8
50% deterred	9.8	5.6	1.4
70% deterred	13.8	7.9	1.9
Panel B. Exponential listing-stock rule			
Total displaced or deterred	15.9	9.1	2.2
Implied reduction in housing stock			
30% deterred	4.8	2.7	0.7
50% deterred	8.0	4.5	1.1
70% deterred	11.1	6.3	1.6
Panel C. Piecewise listing-stock rule			
Total displaced or deterred	14.8	8.4	2.1
Implied reduction in housing stock			
30% deterred	4.4	2.5	0.6
50% deterred	7.4	4.2	1.0
70% deterred	10.3	5.9	1.5

Notes: This table reports national permit effects in millions of permits under alternative treatment-effect, deterrence, and listing-stock assumptions.

- *Panels:* Panels vary the mapping from the active Full listing stock to effective listings. Constant assigns equal weight to every active listing; piecewise assigns positive weight to the first four active listings and zero to later listings; exponential assigns weight $\exp[-0.10(j-1)]$ to the jth active listing.
- *Rows:* Total displaced or deterred aggregates the estimated local permit reductions without a displacement adjustment. The remaining rows report the implied reduction in housing stock after scaling this total by the share of local permit losses that do not show up elsewhere.
- *Columns:* High, central, and low use the lower confidence bound, point estimate, and upper confidence bound from the preferred estimate in Table 6.
- *Specification:* Each cell applies equation 7 using 1980 population, the treatment effect in the column, the deterred share in the row, and the listing-stock rule in the panel. The resulting permit totals in the balanced panel are scaled up to correct for 2/3rds coverage of the United States.

These counterfactual permit gains are concentrated in areas with lots of species listings since 1980 and high housing demand. Figure 9 shows that the largest gains accrue to coastal Californian cities. A counterfactual Los Angeles with no endangered species listings since 1980 has 300,000 additional units permitted over the 45-year period, a 20% increase in the 2024 housing stock of the incorporated place. The cities of San Francisco and San Jose gain 40,000 and 42,000 permits respectively, a 10% and 12% increase in housing stock. Other notable patterns include large percentage increases in housing stock in Appalachia and the Mississippi basin due mostly to the large number of listings in these biologically diverse areas over the period. The Chicago, New York, and coastal Florida metro areas also see large absolute gains in permits.

Figure 9. Cumulative Counterfactual Permit Gains



Notes: This figure maps counterfactual permit gains from removing post-1980 full listings.

- *Units:* Point size reports cumulative additional permits. Color reports cumulative additional permits divided by the place's last observed housing stock.
- *Specification:* Counterfactual gains use the preferred place-level treatment effect from Table 6, 0.7 deterrence share, and the exponential stock rule with $\lambda = -0.10$.

9 Effects on Endangered Species

Protecting endangered species is important and valuable. When a vulnerable vulture species in Mumbai went extinct, for example, human mortality increased by 4% due to increased dwell time for corpses [12]. The collapse of the Northern Long Eared Bat population due to the White Nose Fungus may have increased insect populations and pesticide use, possibly leading to an increase in infant mortality [10].

The importance of protecting endangered species from extinction and the importance of the Endangered Species Act are related but distinct questions. The effect of the Endangered Species Act on the recovery of endangered species is itself a difficult causal inference question. Less than 5% of listed species have ever grown out of endangered status, but the listed species are obviously selected for low chances of recovery. Attempts at causal estimates in the literature have mixed results, with some studies finding null or even negative effects of listing on recovery [9]. Accepting that the ESA does aid in the recovery of endangered species does not resolve the difficult tradeoff between these benefits and the economic costs I have identified in this paper. Both sides of the tradeoff are difficult to quantify and involve significant externalities.

However, this tradeoff is binding only if current policy is on the Pareto frontier between endangered species protection and housing supply, but there are good reasons to believe that it is not. The most urbanized 15% of places are responsible for 90% of total permit flows, while the highest-value endangered species habitat is well outside of these developed areas. The Endangered Species Act seems to restrict infill development in these dense areas as much as it restricts greenfield development in exurban sprawl (Tables 9, 10, 11). Relaxing the legal mechanism of the Endangered Species Act in already developed areas may increase permit flows in dense, energy- and land-efficient cities in California and the East Coast at the expense of sprawling suburbs in the Sun Belt, increasing both housing supply and endangered species habitat.

10 Conclusion

Environmental regulations are difficult to study because exogenous variation in their enforcement is rare. The Endangered Species Act is a unique opportunity to study the causal effects of environmental land use regulations because the intensity of land use regulation under the ESA varies according to the habitats of endangered species and these habitats have changed hundreds of times over since 1980. Endangered species listings have affected large and small areas across the entire country, come into force throughout the 45-year period during a range of macroeconomic and housing market conditions, and have enforced different levels of legal obligation, including the removal of land use regulations in de-listings.

Using this variation in a difference-in-difference framework, this paper provides evidence that an additional endangered species listing reduces annual housing permit flows by 0.5 permits per thousand 1980 residents, about 10% of the average place's permit flow. Accounting for spillovers and diminishing costs, my estimates suggest the aggregate effect of the ESA has been to reduce the national housing stock by several million units since 1980.

Trading off these large economic costs with the value of protecting endangered species is difficult, but less so if current policy is not at a Pareto-optimal allocation between environmental protection and economic growth. Even if fully Pareto-improving moves are unavailable, the spatial concentration of economic activity in just a few small areas in the U.S. means that large economic gains can be made without sacrificing much endangered species habitat. By limiting density in the most prosperous cities, environmental land use regulation may simultaneously have economic and environmental costs.

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take from endangered plants. URL: https://www.fws.gov/sites/default/files/documents/habitat-conservation-planning-handbook-entire0.pdf.

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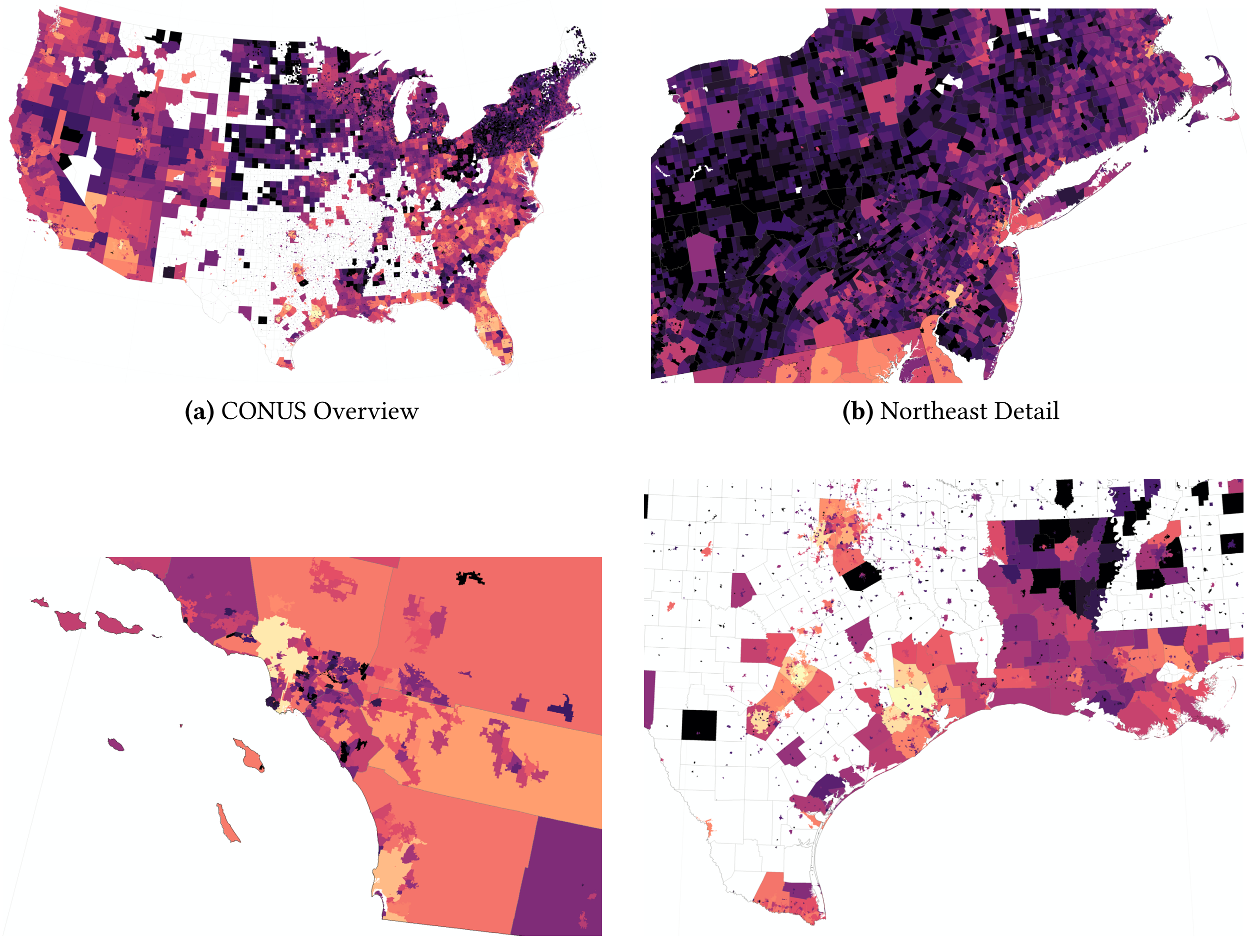
11 Appendix

Figure 10. Map of BPS Permit-issuing Places, 2020. Heatmap colors show log(permits + 1)



(a) CONUS Overview

(b) Northeast Detail

(c) Los Angeles Area Detail

(d) Gulf Coast Detail

Notes: This figure maps BPS permit-issuing jurisdictions linked to Census/NHGIS geography.

- *Panels:* Detail panels zoom to the Northeast, Los Angeles area, and Gulf Coast.
- *Units:* Color reports $\log(permits + 1)$ using 2020 permit counts.

11.1 Population Normalization and Aggregation

The main permit-flow regressions express annual permits per 1,000 1980 residents and weight observations by 1980 population. A natural concern is that this fixed denominator may overstate later permit effects in places that were small in 1980 but large by the time of listing. I therefore re-estimate the preferred specification using population measured immediately before treatment instead. The coefficient changes very little: the baseline CBSA estimate is -0.521 permits per 1,000 1980 residents, while the event-baseline population specification gives -0.508 permits per 1,000 event-baseline residents.

Let P_{iet} denote annual permits for place i in listing-event stack e and year t. Let N_{i0} be 1980 population and let N_{ie} be population from the Census decade immediately preceding listing event e, with population measured in thousands so that coefficients are permits per 1,000 residents. The baseline outcome is

$$y^0_{iet} = \frac{P_{iet}}{N_{i0}},$$

and the alternative event-baseline outcome is

$$y^e_{iet} = \frac{P_{iet}}{N_{ie}}.$$

When the 1980 population of a place is small, y^0_{iet} will be larger than y^e_{iet}. However, in both cases I weight these outcomes in the regression by the same population used in the denominator. This cancels out the difference in denominators and the weighted outcome equals the permit count:

$$N_{ib} y^b_{iet} = N_{ib} \frac{P_{iet}}{N_{ib}} = P_{iet}, \qquad b \in \{0, e\}.$$

For example, if a place had 5 residents in 1980, 10 residents at treatment, and a listing reduces permits by 1 per-current-resident, then the place loses 10 total permits per year after the listing. Measured as per-1980-resident, this looks like an effect of -2. But the regression weight is 5. Measured per-current-resident, it looks like -1, but the weight is 10. Either way, the weighted contribution is the same 10-permit decline.

This does not mean the coefficient must be identical across denominators. The coefficient is still a per-capita coefficient, so changing the population base changes the exposure denominator. In the simplest two-period difference-in-differences case, with treated places T and control places C, using denominator N_{ib} gives

$$\hat{\beta}_b = \frac{\sum_{i \in T}(P_{i,post} - P_{i,pre})}{\sum_{i \in T} N_{ib}} - \frac{\sum_{i \in C}(P_{i,post} - P_{i,pre})}{\sum_{i \in C} N_{ib}}.$$

The numerator is a change in aggregate permit counts which is invariant to the choice of population normalization and weighting. The denominator is the population base used to scale those counts. Using the smaller 1980 denominator will make the coefficient larger in absolute value.

The full specification is more complicated because it includes place-by-event, event-by-year, and geography-by-year fixed effects. The regression does not compare raw permit rates directly; it compares permit rates after subtracting the relevant fixed-effect means. Those means are themselves calculated using the normalized outcome and the regression weights.

Thus changing from 1980 population to event-baseline population changes the place, event-year, and geography-year averages that are subtracted from that observation. The 1980 denominator makes a given permit decline in treated places larger in absolute value, but it also changes the fixed-effect means used to residualize the outcome and treatment comparisons. In this sample, that fixed-effect channel moves in the opposite direction from the raw denominator channel.

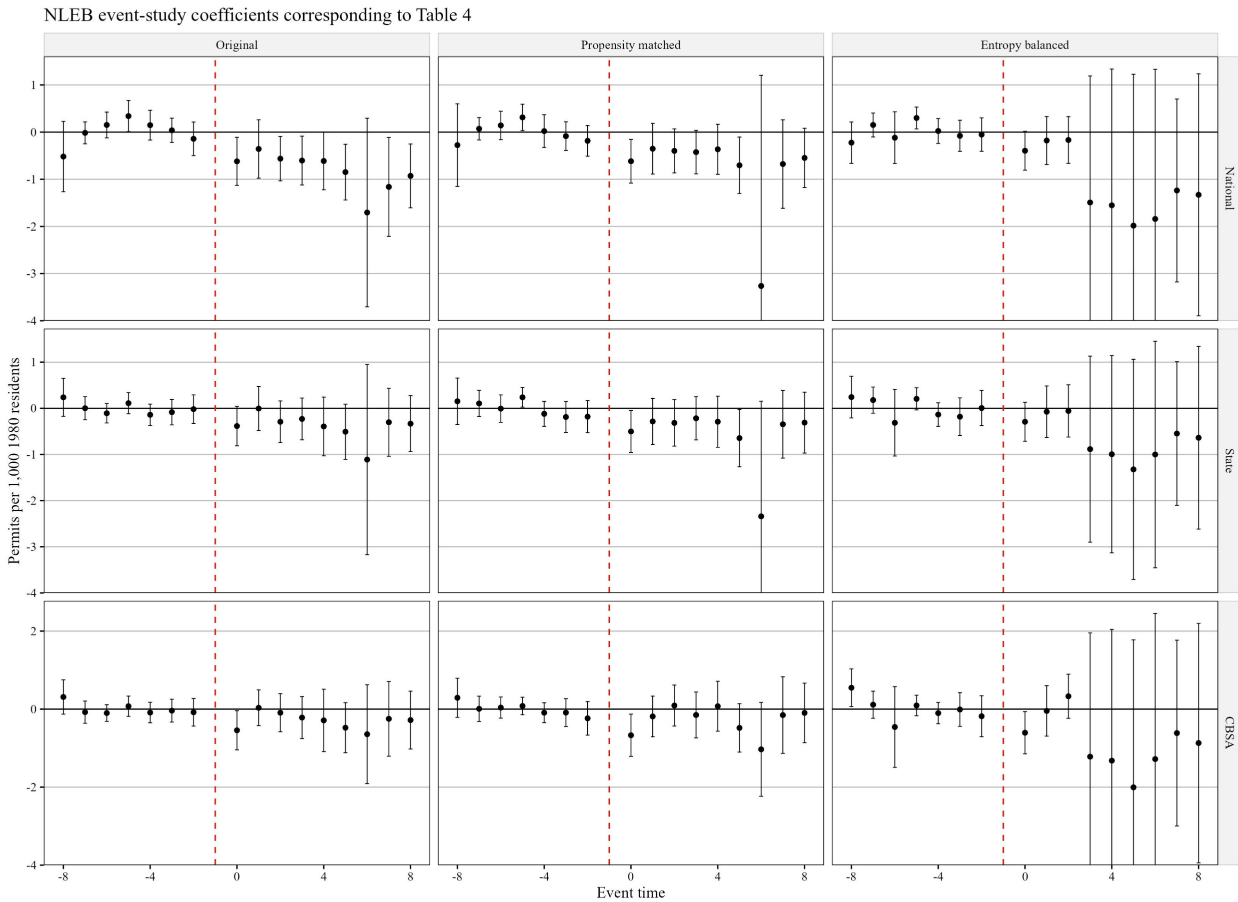
Empirically, the two specifications give nearly the same answer. The preferred baseline CBSA estimate is -0.521 permits per 1,000 1980 residents. Re-estimating the same specification with permits normalized by event-baseline population and weighted by event-baseline population gives -0.508 permits per 1,000 event-baseline residents.

The choice of population normalization also potentially matters for aggregation. The baseline aggregate calculation converts the estimated effect into permit counts using the same 1980 population scale as the main regression tables. I also repeat the calculation in contemporaneous population units. Under the preferred exponential stock rule and 0.7 deterrence share assumption, the balanced-panel total is 4.24 million lost permits using the baseline 1980 scale and 4.58 million using the contemporaneous scale. The aggregate conclusion is therefore not sensitive to scaling later listings by 1980 population.

11.2 Event-Study Robustness Grids

The appendix figures below plot the event-study coefficients corresponding to every geography-by-year fixed-effect and sample-construction cell in Tables 4, 6, 7, and 9.

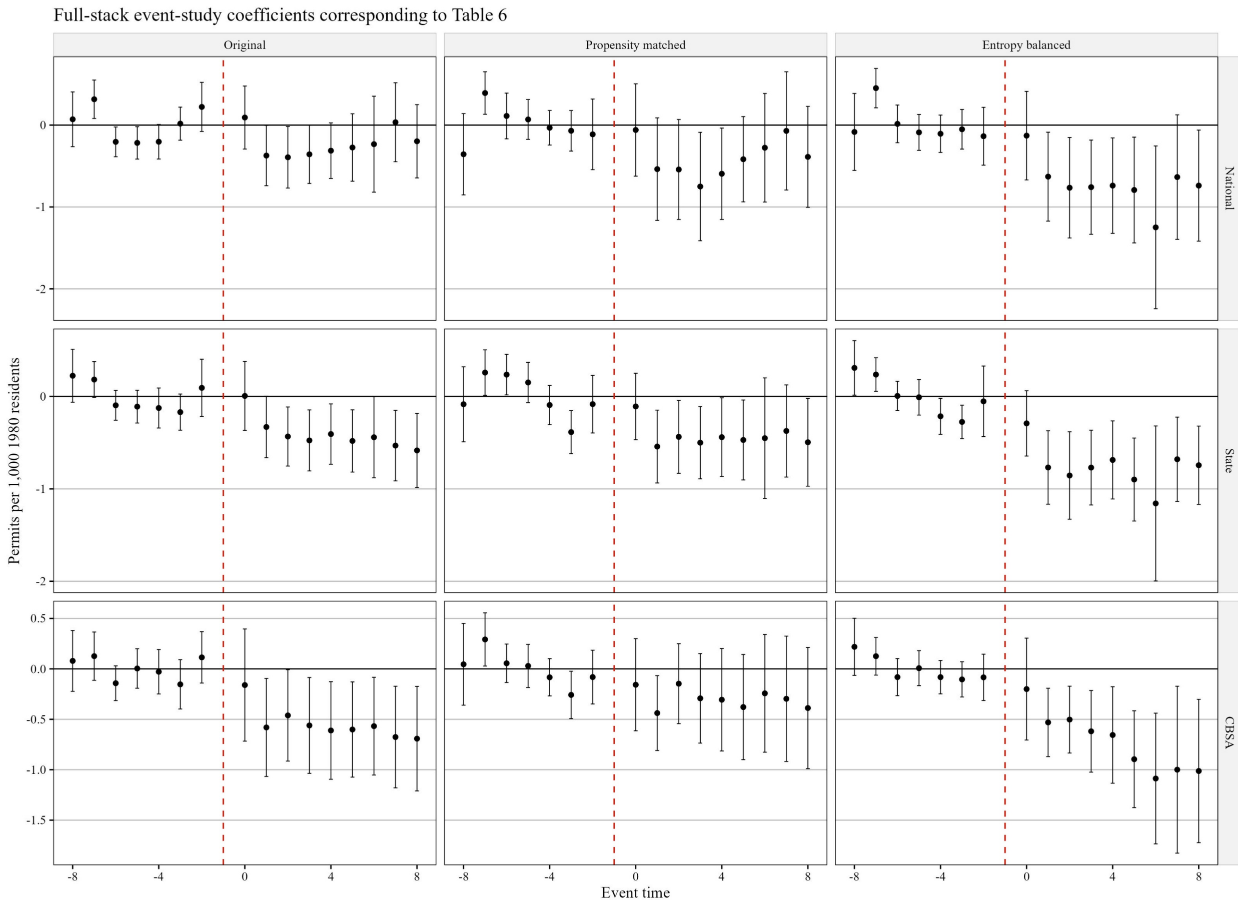
Figure 11. NLEB Event-Study Coefficients Corresponding to Table 4



Notes: This figure plots the event-study coefficients behind the nine Table 4 cells.

- *Panels:* Rows vary geography-by-year fixed effects. Columns vary sample construction.
- *Units:* Coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each panel estimates equation 1 for the corresponding table cell. The omitted year is the year before treatment. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

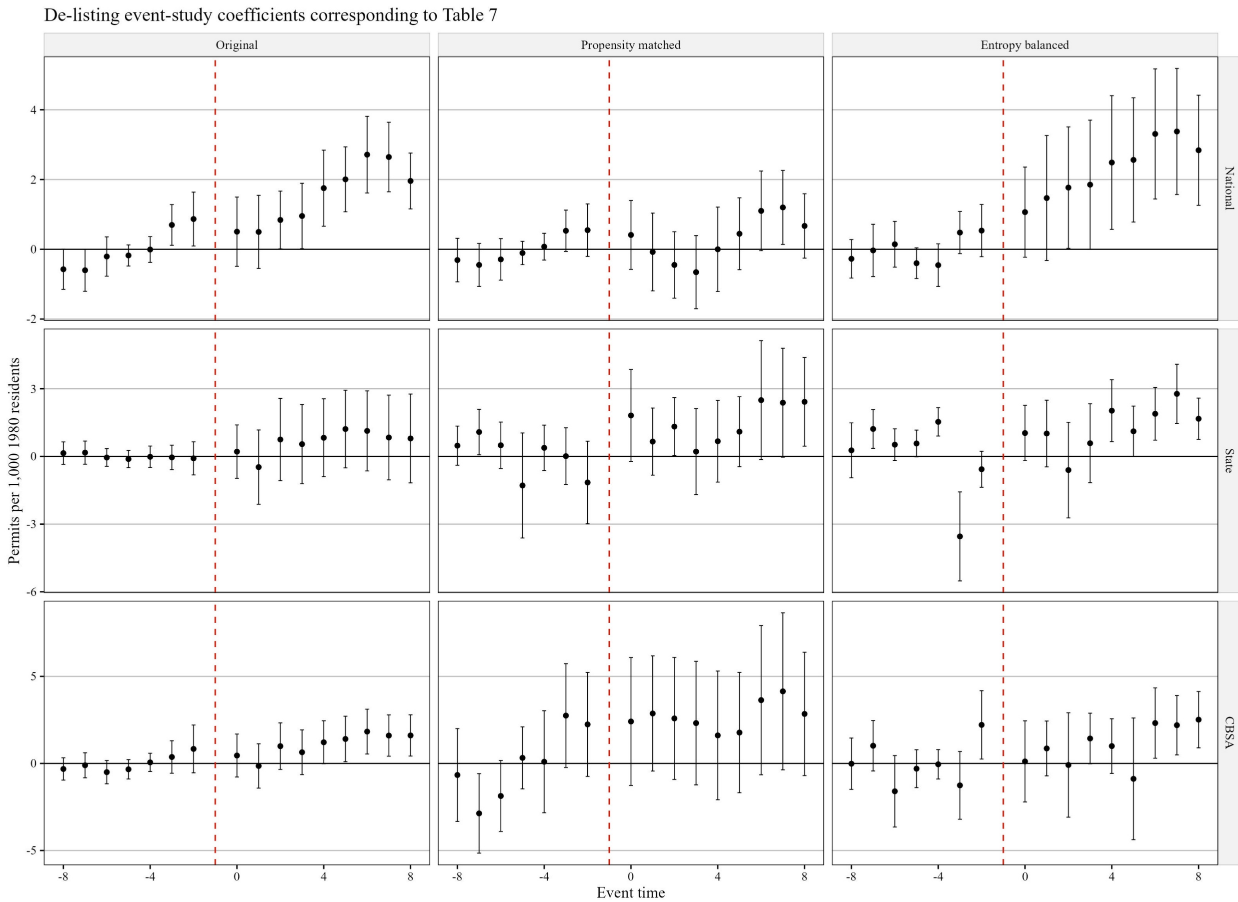
Figure 12. Full-Stack Event-Study Coefficients Corresponding to Table 6



Notes: This figure plots the event-study coefficients behind the nine Table 6 cells.

- *Panels:* Rows vary geography-by-year fixed effects. Columns vary sample construction.
- *Units:* Coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each panel estimates equation 3 for the corresponding table cell. The omitted year is the year before treatment. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

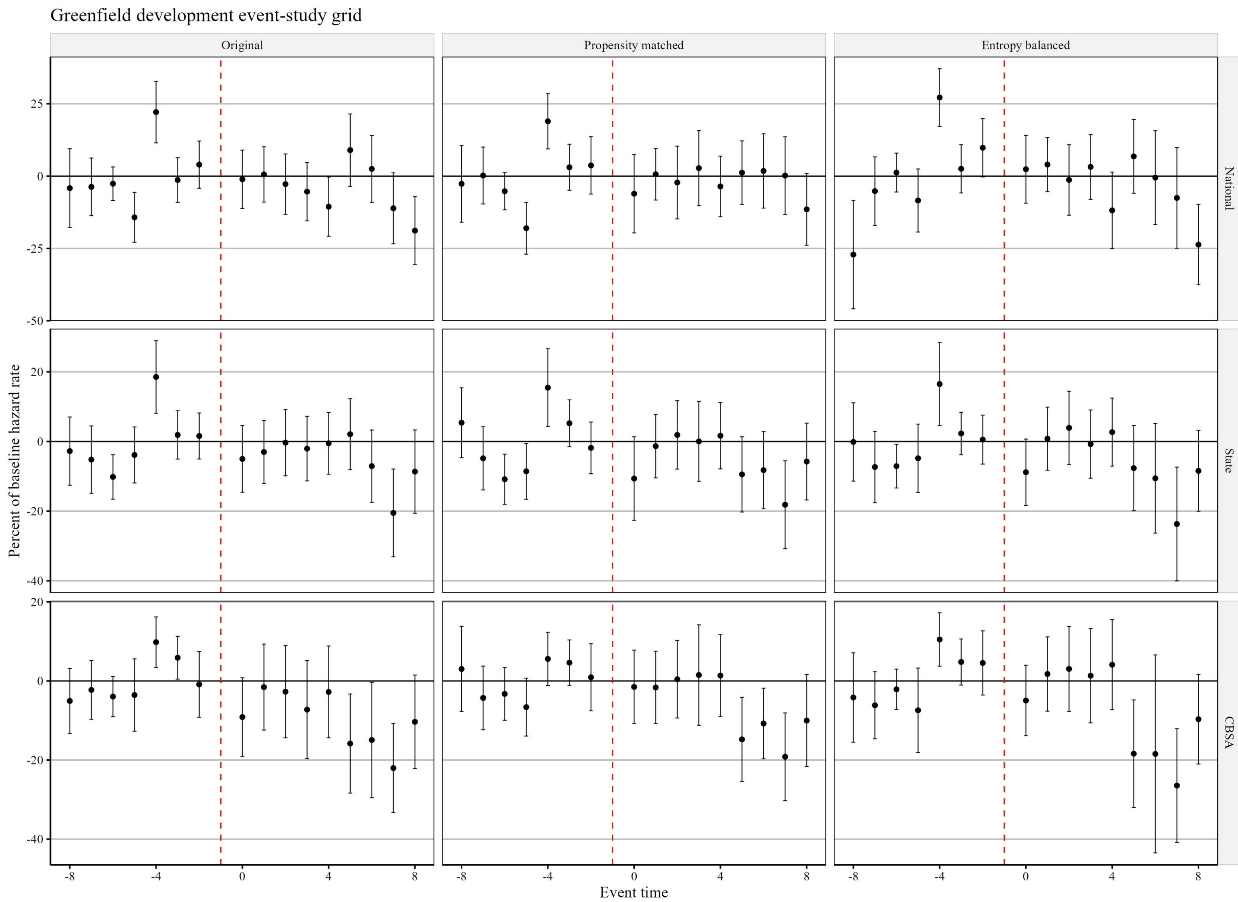
Figure 13. De-listing Event-Study Coefficients Corresponding to Table 7



Notes: This figure plots the event-study coefficients behind the nine Table 7 cells.

- *Panels:* Rows vary geography-by-year fixed effects. Columns vary sample construction.
- *Units:* Coefficients are measured in annual permits per 1,000 1980 residents.
- *Specification:* Each panel estimates the de-listing analog of equation 3 for the corresponding table cell. The omitted year is the year before treatment. Regressions are weighted by 1980 population.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

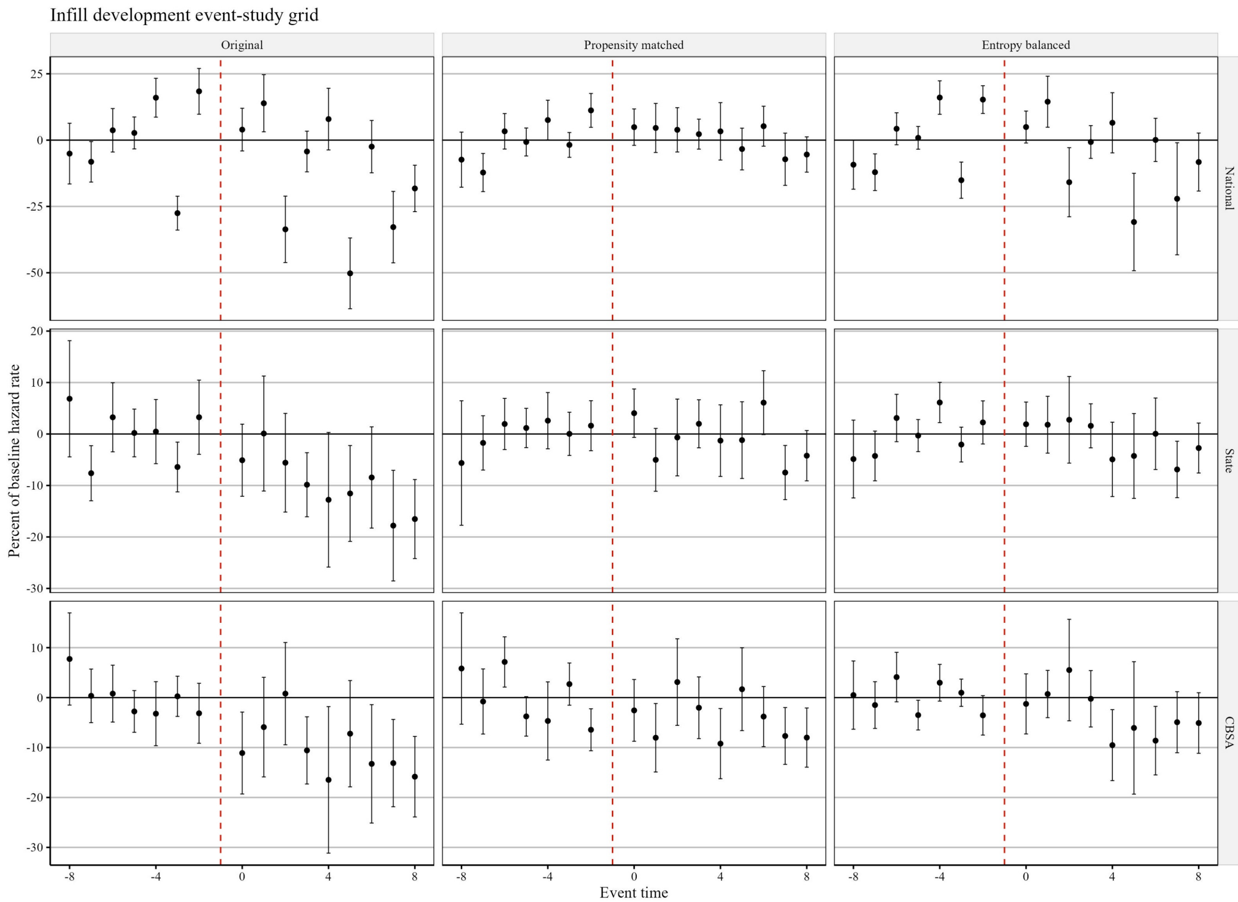
Figure 14. Event-Study Coefficients for NLCD Greenfield Development



Notes: This figure plots greenfield-transition event-study coefficients behind Table 9.

- *Panels:* Rows vary geography-by-year fixed effects. Columns vary sample construction.
- *Units:* Coefficients are reported as percent changes relative to the pre-listing mean greenfield hazard rate.
- *Specification:* Greenfield is forest, shrub/scrub, or wetland pixels becoming developed. The omitted year is the year before treatment. Weights are 1980 population times the source-pixel share.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

Figure 15. Event-Study Coefficients for NLCD Infill Development



Notes: This figure plots infill-transition event-study coefficients behind Table 9.

- *Panels:* Rows vary geography-by-year fixed effects. Columns vary sample construction.
- *Units:* Coefficients are reported as percent changes relative to the pre-listing mean infill hazard rate.
- *Specification:* Infill is developed open-space pixels becoming higher-intensity developed. The omitted year is the year before treatment. Weights are 1980 population times the source-pixel share.
- *Inference:* Standard errors are clustered by place. Confidence intervals are 95%.

11.3 Frank et al. Replication

Frank et al. study the ESA as a national land-use policy shock, but through land markets rather than permitting offices. Their paper ("The Cost of Species Protection: The Land Market Impacts of the Endangered Species Act" [11]) uses the same set of nationwide polygons for species habitats and critical habitats and constructs a similar staggered difference-in-difference regression that compares areas just inside species habitats to otherwise similar areas just outside of them.

Their main outcomes are the volume of real estate transactions and prices. They find precise null effects on both measures around the borders of species habitats and, after accounting for economic selection on the part of the USFWS, larger but still mild effects on prices and transactions around critical habitat areas. On the supply side, they find no evidence of decreased building permit flows in some descriptive statistics on the county-level Building Permits Survey, and noisy results suggestive of increased approval times from BuildZoom and U.S. Army Corps of Engineers (USACE) timelines.

Their headline price result is not in tension with a clear negative quantity response. An ESA listing can shift housing supply by raising expected compliance costs while simultaneously reducing demand for developable land by destroying development option value on affected parcels (and sometimes increasing housing demand through amenities). Thus, the price effect is attenuated or even sign-ambiguous, while quantity still falls.

Much of their permitting evidence lines up with the legal mechanism behind my permit-flow results. Their data on federal permitting timelines are consistent with large fixed costs and delay at the top-end of ESA compliance. They find that for the median habitat conservation plan permit, compliance is a multi-year, multi-million-dollar process, and that federal permitting timelines lengthen under more onerous permit categories after species listings. In their BuildZoom timing evidence, approval times rise by 25 days inside species habitats and by 100 days inside critical habitat, though these estimates are noisy. As a rough bridge to my estimates, I take a result from Gabriel and Kung's study of Los Angeles permit timelines and flows. They find that reducing approval time by 25% (163 days) increases completed units by about 33% [13]. Linearly extrapolating predicts that a 25-day delay might raise permit flows by 5% and a 100-day delay by 20%. My estimate of -0.52 permits per thousand residents is in the center of this range at just under a 10% reduction in permit flows for the average place in my panel.

I also find matching results when it comes to species listing heterogeneity. As shown in the heterogeneity analyses in Table 8 below, some types of species have much larger effects than others and even within these groups there is significant heterogeneity. Some species of mammals and birds travel, nest, and hunt in broad ranges and are hard to rule out in any given plot of land. Other species are confined to small, easy to detect areas, like clams that are stationary in particular riverbeds. These ecological differences lead to big differences in the effect of listing on permit flows because the legal bite of listings ultimately depends on where the individual members of the species actually are and how hard they are to avoid.

This points to a possible issue with Frank et al.'s identification strategy. The authors are running a close border-discontinuity design that compares plots on either side of a 10 kilometer buffer around the

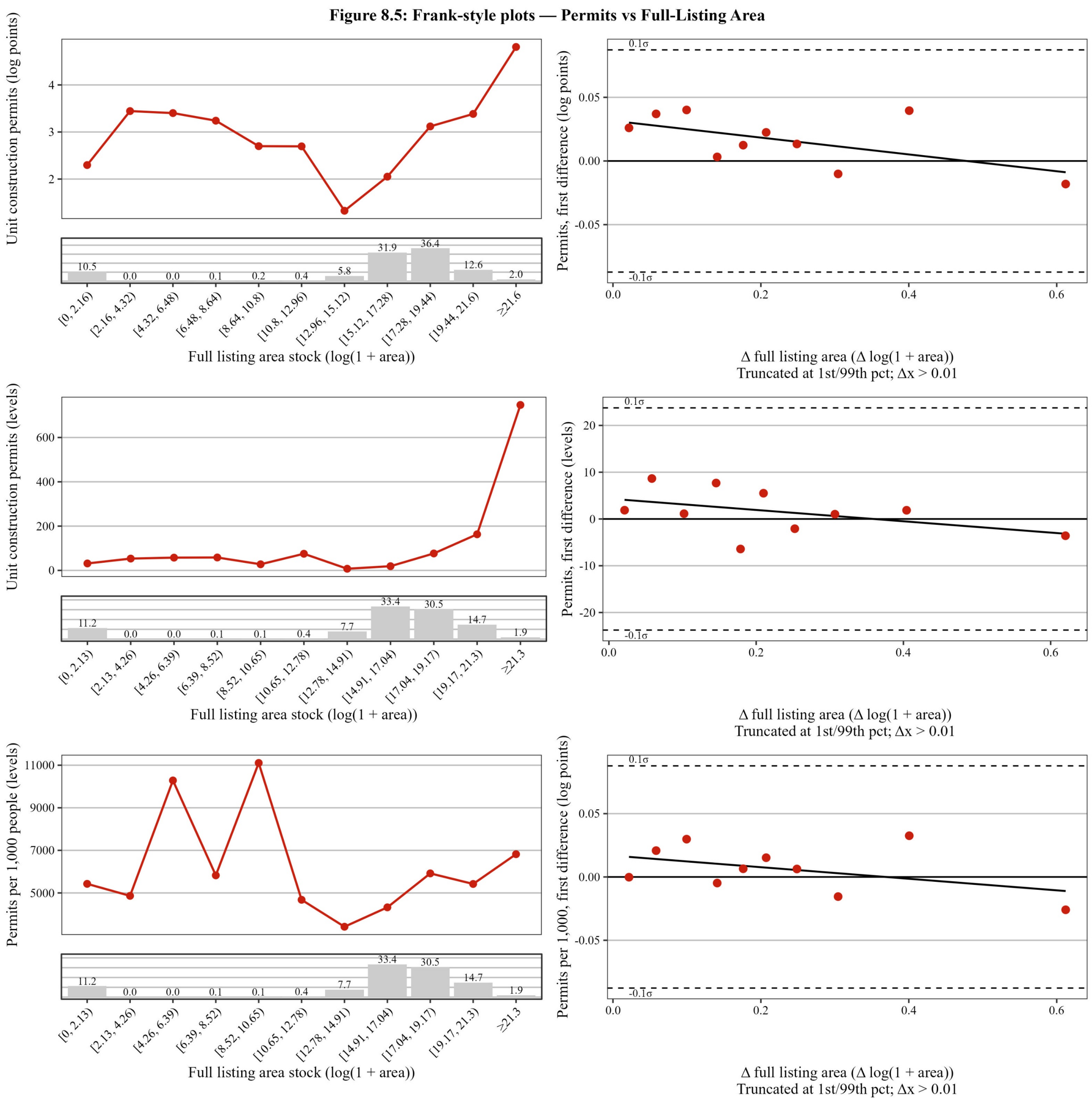
USFWS' published habitat map, but these habitat maps have no legal authority in and of themselves. The legal mechanism of the Endangered Species Act hinges on "actual injury or death to members of an endangered or threatened species" [40]. The habitat maps merely suggest where that actual injury is most likely, but there is no sharp discontinuity of enforcement around the borders. Critical habitats are more explicitly defined areas, but for a project without a federal nexus, there are no inherent extra standards within critical habitats and the extra costs come from the greater likelihood of actually finding and disturbing endangered species there.

Certain species, like the Florida Panther, Mexican Long Nosed Bat, or Northern Spotted Owl can range over dozens of kilometers in a single day of hunting, so it is not clear that a land owner 5 kilometers from the published habitat map would be much more confident that construction on their land would avoid illegal take than a land owner 5 kilometers inside it. The place-level data on permit flows is more geographically granular than counties, but is still sparse enough to avoid weighting too much on these ultra-close comparisons, and all of the main results are robust to installing a 10-km buffer around species habitats.

The starkest area of disagreement between our papers is in Frank et al.'s results on building permit flows, summarized in Figure 10 in their paper [11]. However, this apparent disagreement is mostly about differing estimands and identification rather than contrasting results. Frank et al. do not run their causal border DiD on BPS permit flows because the county-level BPS data are not detailed enough to support it. Their permit flows results are descriptive statistics from binscatter correlations between listing stock and first-differences of permit flows after listing.

In Figure 16, I replicate these descriptive results using the same data and treatment definition in the main specification in Table 6. The binscatter correlation between listing stock and permit flows is mostly flat, or even positive, and while the first-difference is weakly negative, the magnitude of the change is small, similar to Frank et al.'s results.

Figure 16. Frank et al. Replication



Notes: This figure reproduces Frank et al.'s descriptive permit plots in this paper's place panel.

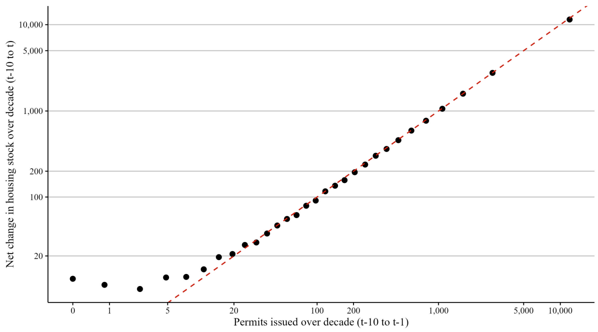
- *Panels:* Levels panels plot binned means against $\log(1+\text{Full stock})$ and show the distribution of observations across bins. Difference panels plot binned means and OLS fits against consecutive-year changes in $\log(1+\text{Full stock})$, retaining positive exposure changes above 0.01 and trimming tails.
- *Units:* Reference bands are ± 0.1 standard deviations of the differenced outcome.

The positive correlation in levels is reconcilable with the negative difference-in-difference effect because this correlation is dominated by permanent cross-place differences in geography, urbanization, long-run growth, and zoning. The causal effect of listings within each place is swamped by these differences across treated and untreated places. The first difference results are matched to the main effect in sign, but not in magnitude. This is because the one-year differences they run only recover the impulse of a shock, not the long run level effect identified by event studies. Indeed, Frank et al. find that the effects get five times larger in the 4th and 5th differences. More fundamentally, Frank's differenced plot is not a difference-in-differences estimator: it relates $\Delta permits$ to $\Delta \log(1 + \text{species stock})$ without subtracting contemporaneous changes in control places. Thus, cyclical shocks might make a treated place's permits rise after listing, even though their permit flows might have risen by even more in the absence of the listing, as in Figure 3b, for example.

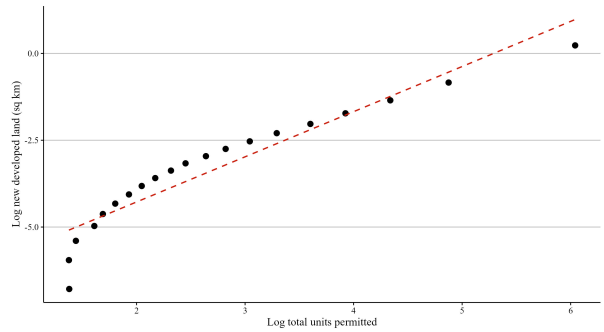
11.4 Relationship Between Permit Flows and Changes in Physical Infrastructure

The Building Permits Survey measures the flow of housing permits, but ultimately this paper is concerned with the supply of housing that is actually built. These may not be simply related if, for example, developers apply for and hold permits without exercising them as a form of speculation on land. In fact, the data reflects a tight, nearly 1-to-1 linkage between changes in permit flows and changes in the observed housing stock of permit-issuing places. Figure 17 displays three binscatters relating permits to measures of changes in physical construction. First, cumulative permit flows over a decade are compared to decadal changes in housing stock. Outside of places with nearly zero permit flow, this relationship is tightly clustered around 1-to-1. The two binscatters on the bottom relate annual permit flows to annual changes in a measure of developed area. New developed pixels flowing in from undeveloped land uses on the left, or growth of low-developed land uses into more highly developed types on the right.

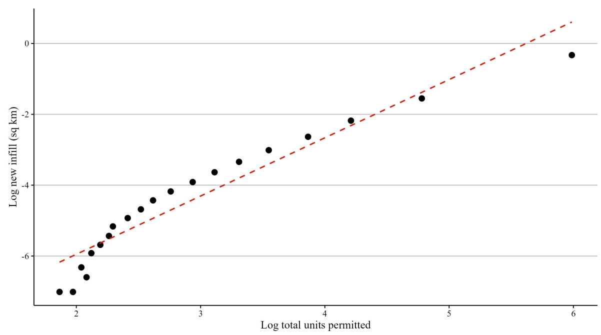
Figure 17. Relationship Between Permit Flows and Changes in Physical Infrastructure



(a) Decadal Permits v Housing Stock



(b) Annual Permits v Extensive Developed Area



(c) Annual Permits v Intensive Developed Area

Notes: This figure relates permit flows to actual additions to housing and developed land.

- *Panels:* Panel A relates decadal permits to decadal housing-stock changes. Panel B relates annual permits to new developed land from undeveloped NLCD classes. Panel C relates annual permits to transitions from lower- to higher-intensity developed NLCD classes.
- *Units:* Points are binned means for BPS permit-issuing places linked to Census/NHGIS geography.

11.5 Species List

Table 14. Full Listing Events: Top 70 by Eligible Treated Places

Species	Listing date	Area	Total places	Eligible treated	In sample	Species	Listing date	Area	Total places	Eligible treated	In sample
Northern Long-Eared Bat	2015-05-04	1516489	6331	3673	Yes	Rabbitsfoot	2013-10-17	91742	224	37	Yes
bog turtle	1997-11-04	86960	1256	1174	Yes	Scaleshell mussel	2001-10-09	20753	41	37	Yes
rufa red knot	2015-01-12	1849692	3069	942	Yes	Black-capped Vireo	1987-10-06	237654	186	36	Yes
Piping Plover	1985-12-11	833025	1304	422	Yes	Cactus ferruginous pygmy-owl	2023-08-21	95152	64	36	Yes
Yellow-billed Cuckoo	2014-11-03	1277194	614	405	Yes	Northern Aplomado Falcon	1986-02-25	120934	53	36	Yes
Least tern	1985-05-28	812763	853	362	Yes	Barbour's map turtle	2024-07-12	24076	38	34	Yes
Hine's emerald dragonfly	1995-01-26	71640	372	355	Yes	Southwestern willow flycatcher	1995-02-27	132213	226	33	Yes
Canada Lynx	2000-03-24	613133	329	241	Yes	Dakota Skipper	2014-11-24	85531	133	30	Yes
Eastern Massasauga (=rattlesnake)	2016-09-30	242051	1317	232	Yes	Foothill yellow-legged frog	2023-09-28	61945	99	27	Yes
Pascagoula map turtle	2024-07-12	259704	255	208	Yes	Streaked Horned lark	2013-11-04	15907	92	25	Yes
North American wolverine	2024-01-02	532122	152	123	Yes	Green sea turtle	2016-05-06	225110	424	24	Yes
Louisiana black bear	1992-01-07	231330	215	119	Yes	Gulf sturgeon	1991-09-30	67666	119	24	Yes
Rusty patched bumble bee	2017-03-21	22932	661	117	Yes	Lake Erie water snake	1999-08-30	3117	22	22	Yes
Topeka shiner	1998-12-15	44665	126	117	Yes	dusky gopher frog	2001-12-04	15945	22	22	Yes
Dismal Swamp southeastern shrew	1986-09-26	250142	212	103	Yes	Bull Trout	1998-06-10	6894	120	20	No
Texas fawnsfoot	2024-07-05	82150	134	102	Yes	Neosho madtom	1990-05-22	14948	28	18	No
Dwarf wedgemussel	1990-03-14	14024	205	101	Yes	American burying beetle	1989-07-13	173768	128	17	No
Eastern Black rail	2020-11-09	360048	511	95	Yes	Plymouth Redbelly Turtle = Plymouth Redbelly Cooter	1980-04-02	1321	27	16	No
Karner blue butterfly	1992-12-14	17205	112	95	Yes	Gila chub	2005-11-02	43712	22	16	No
Mitchell's satyr Butterfly	1991-06-25	27098	183	87	Yes	Virginia northern flying Squirrel	1985-07-31	14454	20	16	No
Roseate tern	1987-11-02	913086	306	78	Yes	Northern spotted owl	1990-06-26	180502	217	14	No
Longsolid	2023-04-10	58486	280	65	Yes	Mexican spotted owl	1993-03-16	298639	80	14	No
Peppered chub	2022-03-30	98002	130	58	Yes	Snuffbox mussel	2012-03-15	63414	335	13	No
Arkansas River shiner	1998-11-23	72658	55	55	Yes	Suwannee alligator snapping turtle	2024-07-29	20012	24	12	No
Fanshell	1990-06-21	21292	150	54	Yes	Round hickorynut	2023-04-10	48797	181	11	No
Valley elderberry longhorn beetle	1980-08-08	37704	65	47	Yes	Spectaclecase (mussel)	2012-04-12	37892	181	11	No
Wood stork	1984-02-28	102436	231	46	Yes	Atlantic pigtoe	2021-12-16	27573	62	11	No
Rayed Bean	2012-03-15	21867	333	45	Yes	Ozark cavefish	1984-11-01	8437	27	11	No
Copperbelly water snake	1997-01-29	15508	115	41	Yes	Vernal pool fairy shrimp	1994-09-19	130654	181	10	No
Pallid sturgeon	1990-09-06	254894	327	40	Yes	Anthony's riversnail	1994-04-15	5217	16	10	No
Idaho springsnail	1992-12-14	215996	92	40	Yes	Fender's blue butterfly	2000-01-25	12982	52	9	No
Winged Mapleleaf	1991-06-20	27316	56	40	Yes	Black pinesnake	2015-11-05	27887	34	9	No
Silverspot	2024-03-18	94029	40	40	Yes	Fresno kangaroo rat	1985-01-30	14499	30	9	No
Clubshell	1993-01-22	27137	300	39	Yes	Pacific marten, Coastal DPS	2020-11-09	28358	22	9	No
California tiger Salamander	2004-08-04	47507	113	38	Yes	Cherokee darter	1994-12-20	3429	10	9	No

Notes: This table lists the largest Full listing events by eligible treated places.

- *Columns:* Area is measured in square kilometers of cleaned habitat. Total places counts balanced-panel BPS places intersecting the habitat. Eligible treated applies the event-window treatment rules before final sample filters. In sample indicates whether the event enters the stacked event-study regression.

Table 15. De-listing Events

Species	De-listing date	Area	Total places	Eligible treated	In sample	Species	De-listing date	Area	Total places	Eligible treated	In sample
Longjaw cisco	1983-09-02	1040419	5244	570	Yes	Tubercled blossom (pearlymussel)	2023-11-16	5243	29	0	No
Bald Eagle	2007-08-08	2155635	3545	515	Yes	Lake Erie water snake	2011-09-15	3117	20	0	No
Aleutian Canada goose	2001-03-20	6843414	709	105	Yes	Southern acornshell	2023-11-16	4730	20	0	No
American peregrine falcon	1999-08-25	596607	326	84	Yes	Virginia northern flying Squirrel	2013-03-04	14454	20	0	No
Least tern	2021-02-12	812763	860	80	Yes	Braken Bat Cave meshweaver	2022-09-23	3252	19	0	No
Dismal Swamp southeastern shrew	2000-02-28	250142	212	69	Yes	Green blossom (pearlymussel)	2023-11-16	8850	19	0	No
Gray wolf	2017-05-01	977923	270	67	Yes	Concho water snake	2011-11-28	29408	15	0	No
Idaho springsnail	2007-09-05	215996	92	38	Yes	Turgid blossom (pearlymussel)	2023-11-16	7079	12	0	No
Arctic peregrine Falcon	1994-10-05	135758	235	11	Yes	Yellow blossom (pearlymussel)	2023-11-16	3145	9	0	No
Delmarva Peninsula fox squirrel	2015-12-16	4393	18	10	Yes	San Clemente sage sparrow	2023-01-25	2809	6	0	No
Kirtland's Warbler	2019-11-08	119758	209	7	Yes	San Miguel Island Fox	2016-09-12	6624	5	0	No
Island night lizard	2014-04-01	21954	101	3	Yes	Santa Cruz Island Fox	2016-09-12	6624	5	0	No
Snail darter	2022-11-04	11891	33	3	Yes	Santa Rosa Island Fox	2016-09-12	6624	5	0	No
Blue pike	1983-09-02	626334	3658	2	Yes	Flat pigtoe	2023-11-16	2654	4	0	No
Oregon chub	2015-03-23	24822	39	2	Yes	Magazine Mountain shagreen	2013-06-14	1895	4	0	No
Sampson's pearlymussel	1984-01-09	244256	961	0	No	Stirrupshell	2023-11-16	2654	4	0	No
Santa Barbara Song Sparrow	1983-10-12	965755	410	0	No	San Marcos gambusia	2023-11-16	1714	3	0	No
Tecopa pupfish	1982-01-15	965755	410	0	No	Apache trout	2024-10-07	9837	2	0	No
Louisiana black bear	2016-03-11	231330	216	0	No	Modoc Sucker	2016-01-07	32549	2	0	No
Brown Pelican	2009-12-17	267119	206	0	No	Okaloosa darter	2023-07-29	473	2	0	No
Black-capped Vireo	2018-05-16	237654	191	0	No	Hawaiian (='lo) Hawk	2020-02-03	13160	1	0	No
Upland combshell	2023-11-16	17692	61	0	No	Hualapai Mexican vole	2017-06-23	1741	1	0	No
Utah valvata snail	2010-09-24	61560	47	0	No	Kanab ambersnail	2021-07-26	946	1	0	No
Lesser long-nosed bat	2018-05-18	93189	39	0	No	Scioto madtom	2023-11-16	3	1	0	No
Bachman's warbler (=wood)	2023-11-16	19425	37	0	No	Borax Lake chub	2020-07-13	2285	0	0	No

Notes: This table lists de-listing events by eligible treated places.

- *Columns:* Area is measured in square kilometers of habitat. Total places counts balanced-panel BPS places intersecting the habitat. Eligible treated applies the de-listing event-window treatment rules before final sample filters. In sample indicates whether the event enters the de-listing event-study sample.