8-20-2021

Estimating the impact of Critical Habitat designation on the values of undeveloped and developed parcels

Saleh Mamun

Erik Nelson
Bowdoin College, enelson2@bowdoin.edu

Christoph Nolte

Estimating the impact of Critical Habitat designation on the values of undeveloped and developed parcels

Saleh Mamun[1,2], Erik Nelson[3], Christoph Nolte[4,5]

Abstract: We use differences-in-differences (DID) estimators to measure the impact that Critical Habitat (CH) establishment had on undeveloped and developed parcel prices throughout the US between 2000 and 2019. In a national-level analysis we found that CH "treatment" had a positive impact on developed *and* undeveloped parcel sale prices relative to sale price trends in nearby but "untreated" control parcels. The finding that CH treatment had a positive impact on undeveloped parcel prices was surprising as we had hypothesized that the impact of CH on undeveloped parcel prices would be negative due to the additional regulatory costs and development uncertainty the CH regulation imposes on land developers. However, when we used relevant subsets of CH areas to measure CH's effects we often found results that were inconsistent with the national-level trends. Therefore, the impact of CH on land prices cannot be reduced to a simple, consistent narrative.

Keywords: Critical Habitat; Endangered Species Act; differences-in-differences; land prices; ZTRAX data; pooled OLS DID; panel DID; Mahalanobis matching

[1] Department of Applied Economics, University of Minnesota, St. Paul, MN.
[2] Natural Resources Research Institute, University of Minnesota – Duluth, Duluth, MN.
[3] Department of Economics, Bowdoin College, Brunswick, ME.
[4] Department of Earth & Environment, Boston University, Boston, MA.
[5] Faculty of Computing & Data Sciences, Boston University, Boston, MA.

Introduction

When a species (or population) is listed under the Endangered Species Act (ESA) the US Fish and Wildlife Service (FWS) or the National Marine Fisheries Service (NMFS) "are required to consider whether there are geographic areas that contain essential features that are essential to conserve the species. If so, the [FWS or NMFS] may propose designating these areas as critical habitat" (USFWS 2017). After public comment on the proposed critical habitat (CH) area, the regulatory agency can choose to finalize CH area. Any finalized CH area contains "the physical or biological features that are essential to the conservation of [listed] species and that may need special management or protection" (USFWS 2017). If the FWS or NMFS does propose and finalize a CH area for a listed species then federal funding or required federal authorization of any activity in the area is not supposed to proceed unless it is deemed "consistent with conservation goals of the ESA" (USFWS 2017).

Activities on private land that require federal authorization or use federal dollars are numerous. For example, many private development projects require a water discharge permit from the US Army Corps of Engineers according to Section 404 of the Clean Water Act (Auffhammer et al. 2020); affordable housing developers typically need federal funds to be profitable; and farmers often apply for Conservation Reserve Program or Wildlife Habitat Incentives Program payments (Melstrom 2020). Land-based projects in CH areas that somehow rely on federal permits or monies *and* are initially found in noncompliance with ESA rules can 1) be modified in accordance with regulations or 2) canceled. Either outcome generates additional costs for the land owner. And even when projects in CH areas are found in compliance with ESA regulations from the beginning, the delays and extra time associated with the additional federal scrutiny mean higher costs for the project developer than a similar project in non-CH areas (Sunding 2003).

Further, in some cases, federal scrutiny is not the only regulatory burden that project developers face in CH areas. For example, the California Environmental Quality Act requires state-level scrutiny of proposed projects in CH areas (Auffhammer et al. 2020). In addition, project developers may perceive that CH will induce restrictions on land use and development and impose costly project delays even when the FWS or NMFS has announced that they foresee no restrictions in the Federal Register (FR) notice that officially establishes the CH. In these cases, project developers will undervalue land in a CH relative to similar land right outside the CH border even though the reasons for such a conclusion appear to be unwarranted.

> *Hypothesis 1: Ceteris paribus, observed sale prices of undeveloped parcels in CH areas will be less than in nearby non-CH areas.*
>
> All else equal, the cost of a development project is higher in a CH area than a non-CH area due to additional federal- and local government-level regulatory scrutiny, more limited subsidization possibilities, and potential non-compliance issues. Further, many developers may perceive development will be more costly in CH areas. Therefore, all else equal, a developer will be willing to pay *less* for an undeveloped parcel in CH areas than in nearby non-CH areas.[6]

Conversely, house prices could be positively affected by CH designation. Consider two neighborhoods, each populated with a smattering of 5-acre rural residential parcels surrounded by undeveloped parcels. Suppose these residential parcels are marketed to growing families that value living near open space and viewing wildlife. Now suppose one of these otherwise

---

[6] This hypothesis does not consider the following possibility. Suppose conservation organizations become interested in pursuing land conservation activities in CH areas once they learn of the area's essentialness for wildlife conservation. If this additional competition over undeveloped parcels in an CH area was particularly intense then undeveloped parcels prices could be *higher* in CH areas than in nearby non-CH areas, all else equal, despite the additional regulatory scrutiny in the CH area (Armsworth et al. 2006).

identical neighborhoods is in a CH area and the other is not. We suspect that the houses in the CH area will have a higher price for two reasons. First, CH regulations could retard, reduce, or, in some cases, stop the development of neighboring open space that US home buyers value (Geoghegan 2002, Kiel et al. 2005, Black 2018). Second, CH designation signals to home buyers that the area around the house has unique and valuable environmental and wildlife conditions. Presumably, Americans that are willing to pay a premium for open space would be willing to pay extra to live in unique wildlife conditions.

> *Hypothesis 2. Ceteris paribus, observed sale prices for houses in CH areas will be greater than in nearby non-CH areas.*
>
> Past research has shown American home buyers are willing to pay more, all else equal, for houses surrounded by open space and unique environmental conditions. CH designation makes it more likely that the undeveloped space around homes in the designated area will remain undeveloped or at least less developed, all else equal. Therefore, all else equal, a home buyer will be willing to pay *more* for a house in CH areas than in nearby non-CH areas.[7]

In both hypotheses the control group is made up of parcels in "nearby non-CH areas." By "nearby" we generally mean parcel sales within 5 km of a CH's border. Therefore, our definition of "nearby" means our hypothesis testing will not capture any economic impacts of CH establishment that extend beyond the CH area and its 5 km buffer. For example, housing prices could rise in a CH's region because of (anticipated) reductions in regional housing supply due to CH regulations (Sunding et al. 2003, Kiel 2005). Given that most CHs and their 5 km buffers make up a small part of a regional housing market, this price change would cover the

[7] This hypothesis does not consider the following possibility. Assume the higher premium for homes near open space creates a race among developers to create more housing in a CH area despite the additional cost and hassle of building in the area. This uncoordinated race could lead to the destruction of most local open space. Therefore, the aggregate effect of the race could mean falling housing prices due to the increase in the local housing stock and the reduction in coveted open space.

CH, its buffer, and area beyond. In other words, an empirical analysis based on our definition of "nearby" non-CH areas should be able to identify any price premium among home buyers to live within a CH area versus immediately outside the CH area, all else equal, but will not be able to identify the more geographically widespread price impacts of a reduction in the region's housing supply.

The DID framework we describe below and ZTRAZ data from Zillow provided us the opportunity to test both hypotheses. Further, when we limited the nearby non-CH control sales to those that were affected by the ESA in general we were able to test the hypothesis that CH regulations affected land prices above and beyond general ESA regulations. The hypotheses of additional regulatory affect from CH relies on the assessment that CH adds additional protection for listed species that other ESA regulations do not afford the species in their geographic ranges outside their CH areas:

> Without critical habitat designation, [ESA regulations are] only required to meet the minimal goal of avoiding extinction of the species [via the jeopardy standard], rather than the higher goal of recovery from endangerment [reached through the adverse modification standard]," the goal of the ESA (p. 60, Armstrong 2002).[8]

---

[8] However, others, including some USFWS administrators, have argued at times that CH does not add any additional protection for species and therefore CH designation is unnecessary and merely administrative .

> [In some cases t]he FWS has foregone designation of critical habitat for most listed species on the basis that designation would not provide any net benefit to the conservation of the species. They are seeking to abandon the requirement to designate critical habitat because they believe that critical habitat is not an efficient or effective means of securing the conservation of a species [see 62 Fed. Reg. at 39131, 64 Fed. Reg. 31871]. (p. 69, Armstrong 2002).

According to his line of argument, the additional federal scrutiny that development activities are supposed to generate in CH areas are applied across a listed species' entire geographic range, not just its CH area. If this latter sentiment guides FWS in their application of ESA and CH regulations and land developer and owner behavior in CH areas then our empirical analysis should find no additional economic burden (i.e., lower undeveloped parcel land values) in CH areas versus nearby non-CH areas in listed species' range space.

If CH does not generate additional effects beyond general ESA regulations then the DID estimator will be equal to 0 when control sales are limited to nearby non-CH areas that are in ESA species range space.

Previous literature

Past work has investigated the impact of CH on parcel values and the pace at which vacant parcels are developed. Using a spatially-explicit regional economic model that assumes that lands designated as CH cannot be used to produce additional housing, Quigley and Swoboda (2007) predict that CH designation increases the value of undeveloped parcels in the region's non-CH areas and prompts the development of some nearby non-CH area parcels that would have otherwise remained undeveloped. Further, the model's restriction on housing in CH areas causes regional housing prices to increase. Therefore, consistent with our second hypothesis, Quigley and Swoboda (2007) find an increase in housing prices in CH areas. However, the anticipated increase in housing prices due to the supply shock also applies to the region's non-CH areas, obviating any price differential between the regulated and non-regulated areas. Quigley and Swoboda (2007) do not consider the possibility that housing prices could differ in regulated versus nonregulated areas due to a general preference for living near open space.

Based on a survey of developers and a regional economic model, Sunding (2003) and Sunding et al. (2003) predicted that the California's Gnatcatcher's CH would create an additional cost of $4,000 per housing unit, delay housing projects by 1 year, and reduce project output by 10% in the regulated area due to CH-related permitting, redesign, and mitigation.

They also predicted that CH areas for California's listed vernal pool species would add $10,000 to the cost of each housing unit and delay completion of housing projects by 1 year in the regulated area. Further, they predicted that the equilibrium housing price in the regions with the vernal pool species CHs would increase by $30,000 due to developers charging more to cover their additional regulation-induced costs and the region's decreased housing supply. Their predictions that development costs would be higher in CHs, and therefore, undeveloped parcel prices in CHs would be lower than prices outside of CHs, all else equal, is consistent with our first hypothesis. However, just like Quigley and Swoboda (2007), their prediction that the regulation-induced housing shock would equally affect housing prices in and outside of CH areas means they do not consider the possibility that housing prices could differ, all else equal, on either side of a CH border.

Several empirical studies have corroborated our theoretical prediction that CH "treatment" decreases the sale price of affected undeveloped parcels. For example, looking at two CH designations in California, Auffhammer et al. (2020) found that the average price of undeveloped parcels in areas designated as CH fell relative to undeveloped parcel prices in non-CH areas. Further, List et al. (2006) showed that prices of undeveloped parcels in an area of Arizona proposed for CH regulation fell relative to prices for undeveloped parcels in a nearby area not proposed for CH regulation.

Finally, Zabel and Paterson (2006) tested the hypothesis that CH designation depresses development activity by comparing 1990 to 2002 building permit issuances inside and outside of California CH areas. The treated area was comprised of 39 CHs finalized between 1979 and 2003 and the control area included the non-CH areas of various administrative units in the state. The authors found that a median-sized California CH area experienced a 23.5% decrease in the supply of housing construction permits in the short run and a 37.0% decrease in the long run relative to the control area.[9] Zabel and Paterson (2006) surmised that development in CH areas decreased due to the higher development costs and developmental barriers created by CH regulations. Their finding that housing development was depressed in CH areas is consistent with our first hypothesis that developers are less interested in CH-regulated land, and therefore, prices for undeveloped land in CH areas will be lower than undeveloped land in non-CH areas, all else equal.

We improve upon the previous empirical literature in several ways. First, unlike the papers noted above, which only consider California and Arizona CHs, we expand the scope of the analysis to landscapes across the US. Second, we look at the impact of CH designation on undeveloped land and housing prices separately given that CH likely affects the value of each asset type very differently. Third, as we noted above, previous literature predicts that housing prices will be affected by CH but empirical evidence in support of these predictions is scarce; the estimated impact of CH on undeveloped land price is more prevalent. However, the previous literature's proposed mechanism for this impact – a restriction in regional housing supply – means that the impact of the CH regulation on housing prices *within* a region cannot be identified: houses inside and immediately outside of a CH area belong to the same regional housing market and therefore are similarly affected by the supply shock. In this study, we

[9] This result confirms that Quigley and Swoboda (2007)'s assumption of no new houses in CH areas to be too restrictive.

empirically identify the incremental impact of CH on housing prices by comparing prices before and after CH establishment on both sides of CH borders within the same regional market.

Data

In this analysis the unit of analysis is the sale of a tax assessor parcel. We obtained digital maps of parcels from twelve open-source state-level datasets and two commercial providers (Loveland and Boundary Solutions, Inc.). The year of the parcel maps vary by state and county. Generally speaking, most parcel maps we use are from 2019, but some open source parcels might be older. Data on parcel sales and characteristics at the time of sale come from the Zillow Transaction and Assessment Database (ZTRAX, version: Oct 09, 2019) (Zillow 2019). ZTRAX contains tax assessor data (parcel numbers, owner names, geographic coordinates, assessed values, FMV estimates, last sale information, numbers of rooms, including bedrooms and bathrooms, build dates, and dates of last modification/renovation) and sales-related data (sale dates, sale prices, inter-family transfer flags). ZTRAX records have been linked to digital parcel boundaries based on assessor parcel numbers, using a customized algorithm for syntax pattern matching and conversion (Nolte 2020). We do not use ZTRAX data that cannot be linked to parcels on our parcel map due to subsequent parcel subdivisions or consolidation.

We calculated parcel size in hectares with digital parcel maps using the Albers Equal Area projection for the lower 48 United States (EPSG:5070). We extracted parcel average slope and elevation from the National Elevation Dataset (USGS 2017a). Land cover on each parcel as of 2011 was estimated using the 2011 National Land Cover Database (NLCD) (Homer et al. 2015).

We used the National Hydrography Dataset (USGS 2017b), buffering, and polygon intersections to estimate lake frontage for each parcel as of 2017. Frontage was estimated by buffering the respective polygons at 25 m, intersecting them with parcels, and dividing the area of the resulting intersections by 25 m. We retain frontage for all rivers, as well as for all perennial lakes and reservoirs larger than 1ha. Each parcel's proximity to coastal waters is measured as percent ocean area within a 2,500 m radius of the parcel (North American Water Polygons; ESRI 2009). Further, we computed the percentage of each parcel's wetland coverage as of 2018 with the National Wetlands Inventory Seamless Wetlands Dataset (USFWS 2018).

Each parcel's travel time to major cities was found with a global raster dataset developed by the European Commission's Joint Research Centre (Nelson 2008). The dataset uses a globally consistent algorithm to estimate travel times to cities with a population of 50,000 people or more at a resolution of 30 arc seconds (NAD 83, EPSG: 4269), incorporating road networks, terrain, land cover, and other data all as of 2000 (http://forobs.jrc.ec.europa.eu/products/gam/sources.php). Parcel distance to highways and paved and unpaved roads is based on the 2019 TIGER roads dataset (USCB 2019).

We obtained footprints for 125.2 million buildings from Microsoft's open-source building footprint dataset (Microsoft 2018) and we used the data to compute the number of buildings on each parcel, the percentage area of the parcel covered by buildings, and the density of building footprints within 5 km of each parcel, all circa 2012.

Finally, we used data on the long-term protection of parcels from the Protected Area Database of the United States (PAD-US 2.0) (USGS 2018) for fee ownership, and the National Conservation Easement Database (NCED) (The Trust for Public Land & Ducks Unlimited 2020) for conservation easements. The exceptions are: 1) New England, where superior coverage is offered by the New England Protected Open Space database (Harvard Forest 2020), and 2) Colorado, where superior coverage is provided by the Colorado Ownership, Management, and Protection (COMaP) database (Colorado Natural Heritage Program 2019). Using this data, we compute the percentage area within 1 km of each parcel that is protected via fee simple ownership or an easement as of the year 2010.

A few other data notes. We omitted arm-length sales from our dataset because they do not convey market value of parcels. In addition, exact coordinates of the properties are crucial in determining many of the geospatial data used in this analysis. We followed best practices as described by Notle et al. (2021) to ensure we best matched sales data to parcels. All data were processed and combined using the Private-Land Conservation Evidence System (Nolte 2020).

Methods

We test hypotheses 1 and 2 with difference-in-difference (DID) models. “Treated” sales – sales of parcels with no buildings at the time of sale (undeveloped; hypothesis 1) or sales of parcels classified as “rural-residential with building footprint” at the time of sale (developed; hypothesis 2) – are 2000 to 2019 sales that took place within a CH boundary either before or after the CH’s boundary had been published in the Federal Register (FR). In contrast, 2000 to 2019 sales of undeveloped or developed parcels that have never been inside a CH boundary but are near a CH boundary (even if the boundary did not exist at the time of the sale) are "control" sales. For exceptions to these assignments, ways in which we vary the treated and control sets, and for the definition of "near" a CH boundary see the section "Parcel sales and the date of treatment used in this analysis" below.

We use the following pooled OLS model with two-way fixed effects to test the impact of CH on undeveloped or developed parcel prices,

$$V_{jt} = \varphi_{j\in c}\sigma_t + \left(\boldsymbol{\beta}_{jt}\mathbf{X}_j + \boldsymbol{\gamma}_{jt}\mathbf{Z}_j\right) + \delta\mathbf{1}[Treat]_j + \theta\mathbf{1}[After]_{jt} + \mu\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt} + \epsilon_{jt} \quad (1)$$

The dependent variable V_{jt} is the log of the per-hectare real sale price of parcel j sold on date t (2019 USD). The first explanatory term, $\varphi_{j\in c}\sigma_t$, is a region-year indicator that fixes the regional location and year of each sale. The term $\boldsymbol{\beta}_{j\in rt}\mathbf{X}_j + \boldsymbol{\gamma}_{j\in rt}\mathbf{Z}_j$ is the 2000 to 2019 national-level hedonic price function *or* the region r – year t hedonic price function if all variables in $\mathbf{X}_j$ and $\mathbf{Z}_j$ are pre-multiplied by region-year dummies. Therefore, the hedonic price function $\boldsymbol{\beta}_{j\in rt}\mathbf{X}_j + \boldsymbol{\gamma}_{j\in rt}\mathbf{Z}_j$ can account for idiosyncratic real estate market conditions across regions and years (Bishop et al. 2020). The remaining explanatory variables in (1) indicate whether parcel j is in an area that became CH sometime between 2000 and 2019 ($\mathbf{1}[Treat]_j$) and whether a sale of parcel j in year t occurred before or after the CH j is associated with was established ($\mathbf{1}[After]_{jt}$). For treated j $\mathbf{1}[After]_{jt}$ indicates if the sale of j at time t occurred after the establishment of the CH that houses j and for control j the variable $\mathbf{1}[After]_{jt}$ indicates if the sale of j at time t occurred after the establishment of the CH that j is nearby. This is a "pooled" OLS model because parcel j can have multiple realizations of V_{jt} over the study time frame.

The vector $\mathbf{X}_j$ contains variables on parcel j's building characteristics at the time of the building(s)'s last know modification, including the number of rooms, the number of baths, the gross area of the building(s), and the calendar year the structure or structures were built. The vector $\mathbf{X}_j$ is empty when we estimate (1) over a set of undeveloped parcel sales. The vector $\mathbf{Z}_j$ contains variables on parcel j's land characteristics including its area in hectares, its average slope, its average elevation, whether it has lake frontage or not, and its percentage of area in wetlands as of 2018. The vector $\mathbf{Z}_j$ contains also contains information on land characteristics near parcel j, including the percentage of area within 2.5 km of the parcel that is in coastal waters, the percentage of the area within a 1 km of the parcel that was protected as of 2010, the percentage of area within 5 km of the parcel that was "built up" as of circa 2012, the travel time from the parcel to the nearest major city by car as of 2000, distance between the parcel and the closest highway as of 2019, and distance between the parcel and the nearest paved road as of 2019.

Finally, μ, the coefficient on $\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt}$, measures the average impact of CH on the sale price of treated developed or undeveloped parcels, whatever the case may be, relative to price trends on control parcels. Specifically,

$$\hat{\mu} = \underbrace{(E[V|\mathbf{X},\mathbf{Z},After=1,Treat=1] - E[V|\mathbf{X},\mathbf{Z},After=0,Treat=1])}_{\text{Impact of treatment on treated}}$$
$$- \underbrace{(E[V|\mathbf{X},\mathbf{Z},After=1,Treat=0] - E[V|\mathbf{X},\mathbf{Z},After=0,Treat=0])}_{\text{Impact of treatment on control}} \qquad (2)$$

Under various assumptions, $\hat{\mu}$ is the unbiased estimator of the Average Treatment Effect on the Treated (ATT),

$$ATT = E[V|\mathbf{X},\mathbf{Z},After=1,Treat=1] - E[V|\mathbf{X},\mathbf{Z},After=1,Treat=1,No\ CH] \qquad (3)$$

where the second term of (3) is the unobserved counterfactual of CH never being applied to an area on the landscape that was actually treated. In other words, ATT measures the average impact that CH had on the value of a treated parcel relative to a counterfactual where it was never treated. When we estimate (1) over one just one CH (e.g., the jaguar's CH) or over a pool of CHs that were all established at the same time, $\hat{\mu}$ is the unbiased estimator of ATT when 1) conditional parallel trends; 2) homogenous treatment effects in **X,Z**; 3) no **X**,**Z**-specific trends across sales grouped according to every unique combination of $\mathbf{1}[Treat]_j$ and $\mathbf{1}[After]_{jt}$; and 4) "common support" all hold (Cunningham 2021, Angrist and Pischke 2009, Daw and Hatfield 2018; Text SI 1). When we estimate (1) over the a pool of CHs with different establishment dates then two more conditions must be met for $\hat{\mu}$ to be an unbiased estimator of ATT: 1) variance weighted parallel trends are zero and 2) no dynamic treatment effects (Cunningham 2021; Text SI 1). Many of these assumptions will not hold when we estimate (1). Therefore, in all likelihood, the various $\hat{\mu}$ we calculate are biased ATT estimates.

Parcel sales and the date of treatment used in this analysis

Our exact interpretation of $\hat{\mu}$ depends on 1) the sales that we estimate model (1) over and 2) the treatment date we choose for sales of parcels that are located within a CH. We vary the set of sales we include in the dataset and the date of treatment to robustly explore the impact that CH designation has had on US parcel values.

Treated sales

Sales between 2000 to 2019 of parcels within a CH boundary either before or after the CH's boundary had been published in the Federal Register (FR) are generally eligible for inclusion in our estimates of (1). (We did not include sales from 1999 or earlier because we do not believe that ZTRAX's sales data from that era are reliable.) Here we describe the set of parcel sales that, while meeting the general eligibility requirements, are *not* included in our dataset because their inclusion would unnecessarily complicate our efforts to identify the impact of CH on parcel values. See Figs. SI 1 – 7 for some maps of CH areas.

First, if a parcel is in more than one CH, we, with one exception, excluded its sales from our analysis. For example, if a parcel is in a CH established in 2005 and then in another CH established in 2010, its observed sales were excluded from our analysis. The exception to this exclusion rule is for parcels that are in multiple CHs that were all established at the same time. For example, if a parcel is in three CHs all established on the same date in 2005 but was never before or never again affected by CH designation then its sales were eligible for inclusion in our analysis. We excluded parcels that were affected by multiple, non-synchronous CHs because their sales would complicate identification of the CH impact on parcel value. For example, suppose a sale of *j* took place when it was covered by one CH but its next sale took place when it was covered by two CHs. We believe that these two treated sales are incomparable given they took place under different regulatory environments.

Second, the sales of parcels that changed from undeveloped to developed status at some point between 2000 and 2019 were excluded from our analysis. For example, suppose parcel *j* was undeveloped when it sold in 2010 but it was classified as developed when it sold again in 2015. In this case, all of *j*'s sales were excluded from our analysis. We do this because, as noted in the literature review, there is evidence that building costs are higher in CH areas than in non-CH areas. Therefore, the price for a house in a CH area built after CH establishment could be higher than an almost identical house built before establishment due to the former home's developer passing on higher building costs to the home buyer. By eliminating all parcels that transitioned from undeveloped to developed in our database we avoid the possibility of our DID estimator being affected by this dynamic (as we note below, we also eliminated all control parcels that changed status).

Third, the sales of parcels covered by a CH that had a "complex" regulatory process were excluded from our dataset. A CH had a complex regulatory process if its proposed or finalized boundaries changed at least once. For example, the California population of the Peninsular bighorn sheep (*Ovis canadensis nelson*) had its CH first proposed in the FR on 7/5/2000 (USDOIFWS 2000) and had this proposed CH finalized in the FR on 2/1/2001 (USDOIFWS 2001). However, on 8/26/2008 the FWS proposed reducing the population's CH area by approximately 189,377 ha (USDOIFWS 2008). This proposed change was finalized on 4/14/2009 (USDOIFWS 2009). Because the FWS does not provide digital maps of former CH area[10] we cannot be certain which parcels *j* were part of the finalized CH from 2/1/2001 to 4/14/2009. Therefore, we ignored the Peninsular bighorn sheep and other listed species that have had similar complex CH processes in our analysis. This means only the sales of parcels in a

[10] Further, the lack of maps of former CH areas means that we cannot be certain that are effort to only analyze parcels treated once and only once with CH has been successful. A property that is only affected by one CH according to the current set of CH maps may have previously been affected by other CHs that have since changed their boundaries. However, we believe such parcels are rare in our dataset.

“simple” CH – CHs with one proposal FR notice and one final FR notice – established between 2000 and 2019 are eligible for inclusion in our analysis.

Fourth, because our dataset only observes building characteristics on developed parcels after the last known modification, any otherwise eligible sale of a treated developed parcel that occurred before the last recorded modification was dropped from our dataset. If we did not do this our DID estimates would include regressions of developed parcel sale price on a set of building characteristics that may not have yet existed at the time of sale.

Subsets of treated sales

In the previous section we described the set of sales from treated parcels eligible for inclusion in estimates of (1). If we estimate (1) over all eligible sales from across the US we are assuming the impact of CH on parcel values does not differ according to the species that is the cause of CH regulations. For example, an estimate of (1) over all eligible treated sales of undeveloped parcels assumes that a northeastern US CH for a well-known animal species and a southwestern US CH for an obscure plant species have the same impact on affected undeveloped parcel values, all else equal.

However, there are various reasons to suspect that one group of CHs will create different parcel value impacts than another group of CHs even after we control for parcel characteristics and times of sales. For example, regulators might enforce CH regulations more strictly for species they perceive as more popular or that are more sensitive to changes in their habitat. In such cases, we would expect the economic impact of treatment to be more severe than in an average case. On the other hand, regulators may be less inclined to strictly regulate when the economic impact of regulation could be very high, the species is not well known, the CH area is very large relative to the amount of actual habitat in the area, or the species can easily navigate pockets of habitat destruction. In such cases, we would expect the economic impact of treatment to be less severe than in an average case.

Further, some state land use regulators may be more inclined to use CH as a guide for the imposition of state-level regulations that affect parcel values. For example, CH establishment in California has triggered regulatory agencies in that state to impose further restrictions in the affected areas (Auffhammer et al. 2020). Presumably regulators in other states – particularly those in states with a stronger libertarian streak – are less likely to impose additional state-level regulations in CH areas.

Moreover, there are two distinct CH shapes and we suspect that the economic impact of CH will differ significantly across these two classes of CH shape. One class of CH shapes are those that follow the contours of streams and coastlines. These CHs, typically designed for listed fish, clams, snails, and sea turtles, will *only* affect stream- or coastal-front parcels. The other class of CHs shapes, those that follow the contours of terrestrial features, are more likely to affect a wider variety of parcels.

Finally, we suspect the impact of CH on parcel values for some individual CHs can vary dramatically from the average impact across all CHs. For example, a CH that covers an idiosyncratic landscape may engender very different economic impacts than the average or representative CH.

To examine whether different sets of CHs generate different economic impacts we estimate (1) across different subsets of treated sales where inclusion in the subset is determined by the CHs that affect the sales. For each distinct subset of treated sales we estimate (1) twice, once with developed parcel sales and then again with undeveloped parcel sales.

In one sub-national level analysis case we estimate (1) over all eligible sales from California CHs. We suspect CH treatment in California could create unique economic effects as California regulators are known to impose additional land-use regulations in CH areas. We also estimate (1) with sales in riparian species CHs to investigate whether CH impact is different on landscapes that follow stream and coastline features. We also estimate (1) with sales just from plant CHs, again with sales just from amphibian CHs, and again with sales just from terrestrial animal CHs (mammals, birds, and reptiles) to test if CH impact differs across broad taxonomic groups. Finally, we estimate (1) for sales treated by a single CH, including the Jaguar, the Gunnison sage-grouse, and the Atlantic salmon. In these cases, we are examining whether treatment effect differs across the idiosyncratic landscapes each of these CHs cover. We chose these individual CHs because the number of 2000 to 2019 treated sales in each case is large enough to generate estimates of model (1).

Control sales

Sales between 2000 and 2019 of parcels that are *near* a CH area that was established between 2000 and 2019 but have never been in a proposed or finalized CH area are *generally* eligible for inclusion in an estimate of model (1) as control sales.[11] In addition, just as we did

[11] The lack of digital maps that would allow us to identify parcels that once were in proposed or finalized CH but no longer are does complicate our identification strategy a bit. For example, our dataset may include a control property *j* that is not currently affected by CH but once was.

with treated parcels, sales of 1) a developed parcel near a CH that occurred after 1999 but before the last known modification and 2) a parcel that changed developed status between 2000 and 2019 (e.g., parcel *j* was undeveloped when it sold in 2010 but the same parcel was classified as developed when it sold again in 2015) were dropped from the potential control set.

In some cases, we restricted control set eligibility to nearby sales of parcels that took place when the parcels were in at least one ESA listed species' geographic range (USFWS 2021). If we define the control set based on this rule then the DID coefficient term explicitly measures the economic impact of CH relative to the economic impact of the ESA regulation in general. If we do *not* exclude control sales based on the range criteria then the DID coefficient measures the economic impact of CH relative to non-CH parcels, which may or may not be affected by ESA regulations in general.

Of course, we further restricted the set of control sales used in an estimate of (1) to sales of those parcels that were *near* the subset of treated parcels included in the estimate of (1). For example, if we were estimating (1) over undeveloped parcels in plant CHs then only the sales of undeveloped parcels *near* the plant CHs were included as controls in the estimate, assuming the sales met all other control eligibility requirements.

The attentive reader will notice we have not yet defined which sales are "near to" or "nearby" a CH, the definition of our control sales. In one case, *all* sales within 5 km of CH polygons are "near to" or "nearby" CHs. In an alternative approach, "nearby" untreated sales that best "matched" treated sales in nearby CH polygons were considered "near to" or "nearby" these CHs. The algorithm we used to find a CH's "matched" control set went thusly. First, we counted the number of eligible untreated sales in each CH polygon's 5 km buffer. If this number was 5 times or more than the number of sales in the CH polygon then we used a Mahalanobis matching algorithm to match two eligible buffer sales to each sale within the CH polygon. For example, if a CH contained 1,000 sales eligible for inclusion in our dataset then the matched set for that CH included 2,000 of the at least 5,000 control sales from the 5 km buffer eligible for inclusion in our dataset. For a few CH polygons, the 5-km buffer count of eligible untreated sales did not meet the 5-fold threshold. In these cases, the set of potential matches included the CH polygon's entire county and, if the number of potential matches was still short of the threshold after including untreated sales from the entire county, adjacent counties (see Text SI 2 for more details on the matching analysis).

We would use a matched control set rather than an unmatched control set to reduce the likelihood of omitted variable bias. The FWS has interpreted the CH rule to say that they can take CH establishment's expected economic ramifications into account when making establishment decisions. Therefore, estimates of model (1) could suffer from omitted variable bias if the FWS did use 1) parcel values or 2) a parcel value-explaining variable omitted from model (1) to help decide which lands to include in CHs.[12] Auffhammer et al. (2020) claim that there is no evidence that CH boundaries in California were affected by parcel value concerns. However, we cannot be sure that boundaries of non-California CHs were not explained by parcel values or some omitted variable that also explains parcel values. Narrowing the control set so that it only includes sales from nearby untreated parcels that best match the distribution

[12] Other forms of bias can affect the DID estimator when we use unmatched controls. For example, only stream-front parcels are included in the stream-based CHs. We assume stream-front parcels are more expensive than nearby parcels not on the stream, all else equal, as homeowners are generally willing to pay more for waterfront parcels. Therefore, a control set that includes sales of parcels that are not located on the steam could lead to a biased model (1) DID estimator as the treated set and control set are made up of fundamentally different goods.

of characteristics found in the treated parcels is one approach to reducing the impact of potential omitted variable bias in DID models.

> The objective of matching is to reduce potential confounding by improving the comparability of units in the treatment and control groups. In the context of [DID], researchers identify a subset of potential confounders and match units from the treatment and control group on measures of these variables prior to the intervention. The effect of the intervention is then estimated using this matched sample (p. 4139, Daw and Hatfield 2018).

In our case, by finding sales in the buffer similar to those sales in the CH we exclude control properties that look like those that FWS may have purposely chosen to exclude from CH regulation despite having the ecological properties that warranted inclusion. In other words, we have a "truer" counterfactual when we use matched sales.

Variables from vectors $\mathbf{X}_j$ and $\mathbf{Z}_j$ provide the cofounders for the matching analysis as they explain parcel value and are likely to help explain treatment assignment as well (see Text SI 2 for the specific list of cofounders used in the matching analysis).[13] Presumably, the FWS forms expectations on economic ramifications of CH establishment – and therefore, possibly makes CH boundary decisions on the margin – by looking at the same parcel and landscape characteristics that we do in equation (1). See Figs. SI 1 – 7 for maps of sales from various CHs and their matched controls.

The date of treatment

[13] However, estimating a DID model with a matched control set can generate biases in the DID coefficient μ that an unmatched control set – assuming "common shocks" and "parallel trends" between the treated and the full, unmatched control set – does not. For example, Daw and Hatfield (2018) note that matched samples can cause "regression to the mean" bias.

Our estimates of (1) also vary according to the date we used to demarcate pre- and post-treatment. We could have used the date that the CH was finalized in the FR as the treatment date. This date demarcates the period where development was affected by CH regulations versus the period it was not. However, we could have also used the day the CH area was proposed in the FR as the treatment day. At this point in time developers and real estate agents would have learned that the proposed area was very likely to become CH area in the next year or so. However, in most cases, we chose to limit pre-CH sales to those that occurred before the proposal data and post-CH sales to those that occurred after the CH had been finalized. Under this "fuzzy" DID analysis, sales that occurred between CH proposal and finalization dates are ignored.[14]

Results

We estimated (1) over various sets of parcel sales. For the first set of estimates (Table 1) we used *all* treated sales and their relevant controls as long as the sales occurred in ESA listed species range space at the time of sale (USFWS 2021). In this first set of national-level estimates we added model modifications one-by-one from a base of no modifications to gauge the marginal impact of each modification. First, we limited controls to matched controls only (Table 1, column 2). Second, we added clustered standard errors to the model as well (Table 1, column

[14] The type of designation – proposed or finalized – creates very different incentives for developers of undeveloped lots (less so for owners of already developed land). A CH proposal could incentivize developers to quickly buy an empty lot and began development before CH regulations kick in. If the data matches this narrative then we might find the opposite of hypothesis 1 for undeveloped lots between the date of proposal and finalization: developers might pay the listed price or even a small premium to be able to quickly buy the land and develop before regulations kick in. We hope to explore this possibility by estimating (1) over undeveloped property sales that occur between CH proposal and finalization in further research.

3). Finally, we also used region – year specific hedonic price functions instead of a national-level hedonic price function (Table 1, column 4). We performed this incremental analysis when developed parcel sales were the dependent variable and again when undeveloped parcel sales were the dependent variable. In every case, we estimated (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization.

Table 1. Estimates of model (1)'s DID coefficient across *all* treated and relevant control parcel sales

	(1)	**(2)**	**(3)**	**(4)**
Developed parcels	0.0445***	0.0282**	0.0282**	0.0063
	(0.0079)	(0.0127)	(0.0099)	(0.0199)
Undeveloped parcel	-0.0557**	0.0841***	0.0841*	0.0683*
	(0.0228)	(0.0317)	(0.0443)	(0.0359)
Matched controls	No	Yes	Yes	Yes
Clustered SEs	No	No	Yes	Yes
Census division-time hedonics	No	No	No	Yes
ESA listed species range	Yes	Yes	Yes	Yes
N (developed)	1,662,017	88,199	88,199	88,199
N (undeveloped)	303,769	34,390	34,390	34,390

Notes: The region x year fixed effect in each estimate was county x year. When "Census division-time hedonics" is 'Yes' there is a unique hedonic function for each US Census division-year combination. If clustered, the standard errors are clustered at the US Census division-level. If "ESA listed species range" is 'Yes' then all sales occurred on parcels that were in ESA species range space at the time of sale. All estimates of (1) used a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization. See Table SI 1 for the descriptive statistics associated with each unique estimate in Table 1.

When there are no model modifications, estimates of model (1) support our two hypotheses: all else equal, CH treatment made developed parcels more expensive and undeveloped parcels cheaper than they would have been otherwise where all parcel sales – treated and control – are affected by ESA regulations in general. However, when we limited control sales to those nearby sales that best matched treated sales, the impact of CH on the direction of undeveloped parcel prices was reversed (Table 2, column 2). Specifically, developed parcels on average were 8.8% *more expensive* than they would be otherwise without CH treatment but general ESA treatment.[15] Developed parcel prices continued to be positively affected by CH treatment even after the switch to matched control sales, albeit the positive price shock shrunk in magnitude. As expected, adding clustered standard errors to model (1) reduced the statistical significance of our estimated DID coefficients but not enough to move them beyond the critical p-value of 0.1 (Table 2, column 3). Finally, when we used US census region-year specific hedonic price functions to capture unique regional and temporal market trends the estimated DID coefficients shrunk both in magnitude and statistical significance. In this most modified version of the national-level model, both hypotheses are no longer supported by the data. CH treatment's impact on the price of developed parcels was not statistically different than 0 and the average developed parcel price was 7.1% *higher* than it would have been otherwise without CH treatment (assuming general ESA treatment).

Table 2 contains estimates of model (1)'s DID coefficients across sub-national sets of sales. In Panel A of Table 2 we report estimated model (1) DID coefficients over individual species CHs. In Panel B we report estimated model (1) DID coefficients over CH collections defined by taxonomy, CH shape, California, or some combination of these characteristics. In Table 2 we only present estimated DID coefficients using treated sales and their controls that occurred in ESA listed species range space at the time of sale. However, we toggle back and forth between all eligible controls and only matched controls in Table 2 results. Once again, in

[15] Because V_{jt} is the log of the per-hectare real sale price, the impact of a change in $\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt}$ is, on average, a $100[e^{\mu}-1]$% change in V (in 2019 USD), all else equal.

every case, we estimated (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization.

Table 2. Estimates of model (1)'s DID coefficients across subsets of treated and relevant control sales

CH set	Developed parcels		Undeveloped parcels		(1)	(2)	(3)	(4)	SEs clust.
Panel A									
Jaguar	-0.1581 (0.1005)	0.0594 (0.1107)	-0.2113 (0.2225)	-0.6697** (0.324)	7,707	712	2,798	302	No
Gunnison sage-grouse	-0.4649*** (0.1481)	-0.2031 (0.2064)	-0.2848 (0.2374)	-0.0243 (0.3225)	2,070	833	703	575	No
Atlantic salmon	-1.2039** (0.5172)	-0.9685*** (0.2218)	-0.0899 (0.595)	-0.1117 (0.4175)	1,769	4,017	278	539	No
Panel B									
Amphibians	-0.4208** (0.0763)	-0.1495* (0.0566)	0.0247 (0.2737)	0.2371 (0.1782)	142,224	4,534	21,085	1,920	Yes
Riparian zone species	0.0813 (0.062)	0.0455 (0.038)	0.1047 (0.1139)	0.2435** (0.0855)	545,456	25,915	174,340	14,126	Yes
Plants	0.7274** (0.1437)	-0.0539 (0.031)	-0.0142 (0.1386)	-0.1071* (0.0411)	895,906	728	68,727	647	Yes
Terrestrial animals	0.0718** (0.0238)	0.0233** (0.0076)	-0.0786 (0.0759)	-0.041 (0.0387)	486,000	61,550	96,437	19,566	Yes
California		-0.019 (0.0512)		0.3415*** (0.1283)		4,087		1,744	No
California - Amphibians		-0.0294 (0.0544)		0.2766** (0.1237)		3,630		1,659	No
Matched controls	No	Yes	No	Yes					
Census division-time hedonics	No	No	No	No					
ESA listed species range	Yes	Yes	Yes	Yes					

Notes: The region x year fixed effect in each model estimate was county x year. If clustered, the standard errors are clustered at the US Census division-level. If "ESA listed species range" is 'Yes' then all sales occurred on parcels that were in ESA species range space at the time of sale. In every case we estimated (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization. See Table SI 1 for the descriptive statistics associated with each unique estimate in Table 1.

Very few patterns in estimated DID coefficient signs and magnitudes emerged in our analysis of subnational sales. In some cases estimated DID coefficients are statistically significant and positive, in other cases, statistically significant and negative. Compared to the national-level estimates, some estimated subset DID coefficients have large magnitudes. For example, developed parcel prices in the Atlantic salmon CH were estimated to have fallen 62.0% on average due to treatment relative to trends in their matched controls. Further, undeveloped parcel prices across the California CHs in our dataset are estimated to have increased 40.7% on average due to treatment relative to trends in their matched controls.

The large swings in estimated DID coefficient values when we toggled back and forth between all control sales and matched only was the one consistent pattern that emerged from our estimates of (1) over various subsets of CHs. Generally, limiting control sales to those that best matched sale conditions in treated sales *reduced* the absolute magnitude of DID coefficients when we estimated (1) over developed parcels and *increased* the absolute magnitude of DID coefficients when we estimated (1) over undeveloped parcels. However, in no case did the toggling create a *statistically significant* change in estimated DID coefficient sign.

Hedonic price function sanity checks

Model (1) is a hedonic parcel model with treatment controls. Therefore, if model (1) is properly specified then the signs on $\mathbf{X}_j$ and $\mathbf{Z}_j$'s estimated coefficients will be consistent with the larger hedonic parcel model literature. Namely, parcels nearer urban amenities (Ardeshiri et al. 2018), transportation networks (Seo et al. 2014), water (e.g., Dahal et al. 2019), and protected areas (e.g., Kling et al. 2015) are typically found to be more valuable, all else equal, than parcels further from these landscape features. Further, parcels higher in elevation (e.g., Wu et al. 2004, Sander et al. 2010) but on flat land are more valuable than low laying land that is slopped (e.g., Ma and Swinton 2012), all else equal. Finally, structures that are larger, have more rooms, more bathrooms, and are newer are more valued than smaller and older structures, all else equal (e.g., Morancho 2003, Sander and Polasky 2009).

In Table 3 we indicate the fraction of times an estimated coefficient on a parcel or structural variable has the expected sign across estimates of model (1) summarized in Tables 1 and 2. Recall that one version of the national-level estimate of (1) included regional-year specific hedonic variable coefficients (Table 1, column 4). Therefore, from these versions of model (1) we have multiple hedonic coefficient estimates for each variable in $\mathbf{X}_j$ and $\mathbf{Z}_j$, one for each unique region-year combination. For example, in Table 3, column (1) we indicate the fraction of region-year specific hedonic coefficients from the national-level model that have the expected sign when the dependent variable was developed parcels sales, all eligible controls were used, and the ESA range filter was on. Columns (2), (4) and (5) give similar "hit" rates under different variations of the national-level model with region-year specific hedonic coefficients. In the model estimates summarized in Table 2 we do not use region-year specific hedonic coefficients. Instead, the models summarized in Table 2 give us nine hedonic coefficients each time we use matched controls (Table 2, columns 2 and (4)). Therefore, in columns (3) and (6) of Table 3 we present the hedonic coefficient estimate "hit" rates across the nine estimates when developed parcel sales are the dependent variable (column 3) and when undeveloped parcel sales are the dependent variable (column 6) and only matched controls were used and the ESA range filter was on.

Table 3. Fraction of estimated hedonic price function explanatory variable coefficients that are of expected sign in estimates of (1) across all CHs (national-level estimates) and subsets of CHs

		(1)	(2)	(3)	(4)	(5)	(6)
		Developed			**Undeveloped**		
	Exp. sign	**All**	**All**	**CH subsets**	**All**	**All**	**CH subsets**
$\mathbf{Z}_j$							
Lake frontage	+	0.65	0.59	0.44	0.59	0.67	0.78
Elevation	+	0.22	0.46	0.70	0.38	0.55	0.78
Slope	-	0.84	0.80	0.80	0.87	0.82	1.00
Travel time to major cities	-	0.64	0.62	0.75	0.52	0.65	0.89
Min. distance to highway	-	0.75	0.71	0.88	0.69	0.67	0.60
Min. distance to paved road	-	0.64	0.54	0.50	0.77	0.60	0.70
% building footprint w/in 5 km radius	+	0.76	0.82	0.80	0.94	0.80	1.00
% coast w/in 2.5 km radius	+	0.94	0.76	0.71	0.91	0.77	0.57
% protected w/in 1 km radius	+	0.63	0.51	0.40	0.60	0.58	0.30
$\mathbf{X}_j$							
Number of rooms	+	0.51	0.45	0.50			
Number of baths	+	0.80	0.82	1.00			
Building Year	+	0.12	0.26	0.10			
Building gross area	+	0.61	0.55	0.00			
Matched controls		No	Yes	Yes	No	Yes	Yes
Census division-time hedonics		Yes	Yes	No	Yes	Yes	No
ESA listed species range filter		Yes	Yes	Yes	Yes	Yes	Yes

Notes: A cell and its text has green shading if the "hit" rate for that hedonic variable is greater than 50%.

We found that the land characteristic variables in vector $\mathbf{Z}_j$ generally shifted both developed and undeveloped parcel values in expected ways. However, the structural variables in vector $\mathbf{X}_j$ in many cases were just as likely, if not more, to shift developed parcel prices in ways that did no align with expectations. In particular, we found that newer structures, all else equal, were less pricy than older buildings. This suggests that much of the newer housing stock in CH areas and their nearby control areas were designed as more affordable options than their older neighbors.

Robustness checks

Panel estimator

In model (1) we do not explicitly link multiple sales that took place on the same parcel. For example, if parcel *j* was sold two times, once in 2008 and again in 2014, then model (1) does not explicitly control for the fact that these two sales occurred on the same parcel. Instead we treat this panel data as if it were cross-sectional. However, we could re-write (1) so that multiple sales from the same parcel are explicitly linked. The panel data version of model (1) is,

$$V_{jt} = \rho_j + \varphi_{j\in c}\sigma_t + \omega\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt} + \epsilon_{jt} \tag{2}$$

where ρ_j is the parcel fixed-effect, $\varphi_{j\in c}\sigma_t$ is the county-year fixed effect, ω is the DID panel estimator, and (2) is only estimated over the treated and control parcels *j* that sold at least twice in our study time frame.

We estimated a panel DID model to explore the possibility of omitted variable bias in our default model (Kolstad and Moore 2020). In model (2) the parcel-level fixed effects ρ_j control for *all* time-invariant parcel-level characteristics that affect V_{jt}, not just those parcel-level characteristics that we happened to include in $\mathbf{X}_j$ and $\mathbf{Z}_j$. Therefore, model (2) may inspire more confidence in the causal interpretation of the DID coefficient because *all* time-invariant parcel-level variables, including those that were omitted in model (1), and therefore may be sources of bias in our previous estimates of the DID coefficients, are controlled for in model (2).

Table 4. Estimates of model (2)'s DID coefficient across all CHs in our dataset.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Developed				**Undeveloped**			
Panel A								
Estimate of (2) using panel dataset formed from "All CHs" set	0.0840*** (0.025)	0.0851*** (0.0247)	0.0618*** (0.0088)	0.0586*** (0.0099)	0.1234* (0.054)	0.1291** (0.0522)	0.1053 (0.0589)	0.0643 (0.0794)
Panel B								

Estimate of (2) using panel dataset formed from "All CHs" set – complete observations only	0.0968*** (0.0219)	0.0987*** (0.0215)	0.0755*** (0.0152)	0.0712*** (0.014)	0.1454** (0.044)	0.1488** (0.0448)	0.1469*** (0.0284)	0.1175* (0.0531)
Panel C								
Estimate of (1) using panel dataset formed from "All CHs" set – complete observations only	0.0216 (0.0567)	0.0216 (0.0585)	0.0600*** (0.0161)	0.0640*** (0.0126)	-0.0615 (0.0663)	-0.0522 (0.0672)	0.1021** (0.0328)	0.1615*** (0.0354)
Matched controls	No	No	Yes	Yes	No	No	Yes	Yes
ESA listed species range	No	Yes	No	Yes	No	Yes	No	Yes
N (Panel A)	1,469,755	1,446,448	62,549	62,207	166,411	164,670	20,984	20,919
N (Panel B)	1,042,748	1,023,719	42,857	42,826	142,262	140,990	16,583	16,506
N (Panel C)	1,042,748	1,023,719	42,857	42,826	142,262	140,990	16,583	16,506

Notes: Panels A and B use county-year and parcel fixed effects and cluster standard errors at the US Census division level. Panel C uses county-year and parcel fixed effects and cluster standard errors at the US Census division level. "Research shows that fixed effects estimators can generate much more accurate estimates when combined with matching..." (p. 4, Melstrom 2020). In every case we estimated models (2) and (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization. See Table SI 3 for the descriptive statistics associated with each unique estimate of models (2) and (1) in Table 4.

In Table 4, Panel A we present DID coefficients from estimates of model (2) using the nation-wide panel of sales and different combinations of control sets and the ESA range filter (in every case we use the fuzzy treatment timing). In Table 4, Panel B we present results from a similar analysis but in this case the panel dataset was pared to only include parcel sales with observations for every variable in $\mathbf{X}_j$ and $\mathbf{Z}_j$. This requirement of complete panel data had a negligible impact on model (2) results with the exception of undeveloped parcel sales with matched control sales and the ESA range filter (compare Table 4, column (6)'s Panel A to B). In this case, the change in the dataset increased the DID estimate almost two-fold and caused a statistically insignificant effect to become statistically significant at the p = 0.1 level.

We estimated model (2) with a complete data panel in order to better identify the impact of modeling assumptions on DID estimates. Recall model (1) only includes sales that have data for every variable in $\mathbf{X}_j$ and $\mathbf{Z}_j$. Therefore, by limiting the panel to repeated sales with data for every variable in $\mathbf{X}_j$ and $\mathbf{Z}_j$ we ensure that estimates of model (1) and (2) over this dataset include the exact same set of sales observations. In other words, the differences in Table 4, Panel B and C estimates cannot be due to differences in dataset composition. Instead they will be due to the different ways the panel model (Panel B) and the pooled OLS model (Panel C) control for sale covariates.

Using our preferred model set-up, matched controls and ESA range filter, we found that modeling assumptions – pooled OLS or panel DID – did not markedly change results. First, hypothesis 2 is supported regardless of modeling assumptions: developed parcels are more expensive due to CH treatment than they would be otherwise (compare Table 4, column (3)'s Panels B and C). Second, our first hypothesis is not supported regardless of modeling assumptions: undeveloped parcels are more expensive due to CH treatment than they would be otherwise (compare Table 4, column (6)'s Panels B and C). Finally, when we compare Table 4, Panel C results to Table 1, columns (3) and (4) results – pooled OLS results with matched controls, ESA range filter, and otherwise no paring of observations – we see that the main themes of model (1)'s results did not change when we limited observations to those with repeated sales and complete data. In all cases, hypothesis 2 is generally supported and hypothesis 1 is not.

Investigating the impact of staggered treatment timing on DID coefficient estimation

Recent econometric literature has discussed the possibility of biased DID estimators when treatment timing is staggered (Goodman-Bacon 2018; de Chaisemartin and d'Haultfoeuille 2020, Text SI 1). Per Callaway and Sant'Anna (2019), we eliminate the potential of bias in model (1)'s DID estimator due to staggered treatment by estimating the model over a cohort of CHs that were proposed and finalized at approximately the same time. We have identified three such contemporaneous treatment and sale sets. Model (1) DID estimates across these three sets of contemporaneously sales and their matched controls are given in Table 5. Further, to reduce the potential impact of other unobserved policy changes affecting prices we only included treated sales and relevant matched control sales that occurred within two years of treatment (pre-treatment sales are not limited by time).

Table 5. Estimates of (1)'s DID coefficient when treatment time is not staggered

Cohort	CH set	(1) Developed parcels	(2)	(3) Undeveloped parcels	(4)	(1) N	(2)	(3)	(4)
1	CH proposal: 6/06–11/06 CH finalization.: 8/07–12/07 Post-treat. sales: 12/07–12/09	0.0784*** (0.0000)	0.0787 (0.133)	-0.1750*** (0.0000)	-0.2322*** (0.0000)	780	781	527	529
2	CH proposal: 8/11–11/11 CH finalization: 7/12–11/12 Post-treat. sales: 11/12–11/14	-0.4091*** (0.0249)	-0.4099*** (0.0253)	-0.1081 (0.1306)	-0.1576 (0.1309)	708	708	499	499
3	CH proposal: 5/12–10/12 CH finalization: 7/13–12/13 Post-treat. sales: 12/13–12/15	-0.0004 (0.0239)	0.0037 (0.0213)	0.0400 (0.1883)	0.0440 (0.1195)	5068	5064	1858	1852
	Matched control	Yes	Yes	Yes	Yes				
	ESA listed species range	No	Yes	No	Yes				

Note: Standard errors are clustered at the US Census division-level. In every case we estimated model (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization. See Table SI 4 the roster of species CHs in each set.

Using contemporaneous treatment and sale cohorts to estimate (1) we found no consistent impact of CH on parcel values. First, the evidence that CH establishment has had a positive impact on already developed parcel is mixed; we find statistically significant positive, statistically significant negative, *and* statistically insignificant DID coefficients (Table 5, columns (1) and (2)). However, in this cases, CH establishment never increased the value of treated undeveloped parcels in a statistically significant manner. Therefore, the results in columns (3) and (4) of Table 5 are more consistent with hypothesis 1 than most of our previous modeling.

Investigating the impact of definition of "undeveloped" parcels on DID coefficient estimation

Some parcels flagged as "developed" in our database are likely to be viewed by developers as still "developable." For example, consider an 80-acre parcel with one house on it. We consider this parcel "developed" given that it has a nonzero building footprint observation. Suppose the conversion of the parcel to a subdivision would generate millions of dollars in net revenues for a developer. In this case, the "developed" parcel would be very attractive to a developer as the cost of removing the one house would be negligible compared to the value of the parcel after subdivision. Therefore, we experimented with dividing our developed parcels into two types: those that we believe could generally be re-developed at little relative cost (like the fictitious 80-acre parcel described above) and those that we believe would be much costlier or impractical to re-develop. We assume this latter category would for the most part be made up of developed parcels where the new owners would use the parcel as is or implement marginal changes at most.

We believe a parcel's building to parcel area (BPR) ratio statistic provides the best means of separating developed parcels into these two types. We assume developed parcels with a low BPR – lower than 0.063 across all parcels categorized as 'Rural Residential' – were like our fictitious 80-acre parcel with one house: very "developable" (this threshold is at the 90^{th} percentile of the BPR ratio distribution across "Rural Residential" parcels, building code

RR102 in our dataset). On the other hand, we assumed plots with BPRs greater than these thresholds were much less likely to be bought for re-development.

Table 6. Estimates of (1)'s DID coefficient using alternative definitions of developed parcels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CH set	**Developed parcels**		**Developed parcels (high BPR)**		**Developed parcels (low BPR)**		**Undeveloped parcels**	
All	0.0218** (0.0069)	0.0282** (0.0099)	0.0155 (0.0134)	0.0177 (0.0156)	-0.0124 (0.0461)	0.0040 (0.0347)	0.0810* (0.042)	0.0841* (0.0443)
N	88498	88199	75554	75470	12944	12729	34463	34390
Matched control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ESA listed species range	No	Yes	No	Yes	No	Yes	No	Yes

Note: Standard errors are clustered at the US Census division-level. In every case we estimated model (1) with a fuzzy DID where the fuzzy period begins at CH proposal and ends at CH finalization. See Table SI 2 for the descriptive statistics associated with each unique estimate in Table 1.

In Table 6 we present the estimate of model (1) across *all* CHs in our database using the alternative definitions of developed parcels (this is the same set of sales that generated the estimated DID coefficients in Table 1's Panel A). For easy reference, we recreate estimated model (1)'s DID coefficients from Table 1's Panel A, column (3) in Table 6's columns (2) and (8). We found that the developed parcels with a high BPR have estimated DID coefficient magnitudes like those associated with the entire pool of developed parcels, albeit the latter estimated DID coefficients are not statistically significant (compare DID estimates from columns (1)-(4) of Table 6). We also found that the economic value of developed parcels with a low BPR were essentially not affected by CH treatment. In other words, parcels that we deemed still developable despite a building or two did not register, on average, a price shock from CH treatment. To summarize, this alternative analysis weakens the data's support for hypothesis 2 as parcels with high building footprint intensity no longer have statistically significant positive DID coefficients. Further, this alternative national-level analysis produced some results that were less diametrically opposed to hypothesis 1 than the original national-level analysis (assuming matched controls). At least parcels with low BPR – “developed” parcels deemed still very developable – did not receive a positive prick shock from treatment like completely vacant parcels did (assuming matched controls in both cases; compare columns (5)-(6) of Table 6 to columns (7)-(8)).

Conclusion

We found that CH treatment had a mixed impact on parcel values. The impact of treatment on *developed* parcel values relative to value trends in control developed parcels was positive when all CHs in our dataset were considered (see Table SI 5 for all CHs in our dataset). This national-level trend in treated developed parcels held whether we used a pooled OLS DID or a panel DID model. However, we also found that in some subsets of CHs, the average *developed* parcel price fell in response to treatment, all else equal. In other words, we found inconsistent support for our contention that observed sale prices for already developed parcels in CH areas should be greater than in nearby non-CH areas, all else equal.

Conversely, we found little support for our hypothesis that observed sale prices for undeveloped parcels in CH areas will be less than in nearby non-CH areas, all else equal. In fact, more often than not, we found reactions that were opposite of what we expected in undeveloped parcel price trends. At the national-level, once we limited our control set to the matched set, CH treatment caused undeveloped parcel prices to relatively increase, not fall.

This national-level trend in treated undeveloped parcel prices held whether we used a pooled OLS DID or a panel DID model. And only a few of the CH subsets created the expected impact of treatment on undeveloped parcel prices. Why treatment seemed to *increase* undeveloped parcel prices relative to the matched control parcels within 5 km of the CH boundaries is unclear. One potential reason is that relative demand for undeveloped land in CHs generally did not fall after treatment and therefore developers had to pay the higher land prices created by a more limited supply of developable land in the CH area. Or maybe market participants were cognizant of the higher prices that developed land in CH areas has generally commanded, leading to higher prices for undeveloped land in anticipation of this higher than average return on investment.

In conclusion, whether CH treatment increased or decreased parcel land values was case-specific. In some cases, we found evidence that supports are hypotheses and in other cases we did not. Therefore, we cannot make a definitive statement about the impact of CH on parcel land values.

Discussion

Potential bias in our DID estimators

We already noted that our DID estimators are unbiased estimators of the ATT if and only if various data and modeling assumptions hold. However, even in the unlikely event that all the DID data and modeling assumptions held, there would be other reasons to suspect that the DID estimators generated by models (1) and (2) are biased. We detail some of the other potential sources of DID estimator bias here.

As we mentioned in the introduction, the establishment of CH boundaries is guided by the spatial distribution of the species and known habitat. Within that area,

> [w]here a landowner seeks or requests Federal agency funding or authorization for an action that may affect a listed species or critical habitat, the consultation requirements of section 7(a)(2) would apply, but even in the event of a destruction or adverse modification finding, the obligation of the Federal action agency and the landowner is not to restore or recover the species, but to implement reasonable and prudent alternatives to avoid destruction or adverse modification of critical habitat. (p. 2542, USDOIFWS 2013)

We have hypothesized that the consultation requirement and the possibility of a "destruction or adverse modification finding" dampens developer demand for land in the CH, all else equal, and accordingly, dampens prices for those lands, all else equal. However, a more nuanced model of CH impact on undeveloped parcel values would surmise that *only some* developable parcels would be likely to require the developer to "implement reasonable and prudent alternatives to avoid destruction or adverse modification of critical habitat" and that these specific parcels would be especially undesirable relative to those "treated" parcels that were unlikely to engender a destruction or adverse modification finding when subject to a development plan. Therefore, a critical unobserved variable in our analysis of undeveloped parcel prices indicates each parcel's probability of a destruction or adverse modification finding if considered for development. Let this probability be given by M_{jt}. Model (1) with this omitted variable is,

$$V_{jt} = f(\varphi, \sigma, \boldsymbol{\gamma}, \mathbf{Z}_j) + \delta \mathbf{1}[Treat]_j + \theta \mathbf{1}[After]_{jt} + \varphi \mathbf{1}[Treat]_j \mathbf{1}[After]_{jt} + \rho M_{jt} + \epsilon_{jt} \quad (3)$$

If $cov\left(M_{jt}, \mathbf{1}[Treat]_j\right) \neq 0$ then model (1)'s μ for undeveloped parcel is a biased DID estimator of undeveloped parcel's "true" DID estimator φ. Suppose the form of covariance between M_{jt} and $\mathbf{1}[Treat]_j$ can be represented with the equation,

$$M_{jt} = a + b\mathbf{1}[Treat]_j + c\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt} + \varepsilon_{jt} \tag{4}$$

Then (3) becomes,

$$V_{jt} = f\left(\varphi, \sigma, \boldsymbol{\gamma}, \mathbf{Z}_j\right) + \delta\mathbf{1}[Treat]_j + \theta\mathbf{1}[After]_{jt} + \varphi\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt}$$
$$+\rho\left(a + b\mathbf{1}[Treat]_j + c\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt} + \varepsilon_{jt}\right) + \epsilon_{jt} \tag{5}$$

$$= f\left(\varphi, \sigma, \boldsymbol{\gamma}, \mathbf{Z}_j\right) + \rho a + (\rho b + \delta)\mathbf{1}[Treat]_j + \theta\mathbf{1}[After]_{jt} +$$
$$(\rho c + \varphi)\mathbf{1}[Treat]_j\mathbf{1}[After]_{jt} + \left(\rho\varepsilon_{jt} + \epsilon_{jt}\right) \tag{6}$$

Therefore, model (1)'s $\hat{\mu}$ is an estimate of $\rho c + \varphi$ when $cov\left(M_{jt}, \mathbf{1}[Treat]_j\right) \neq 0$. If we assume,

- that the value of an undeveloped parcel declines as the likelihood of a destruction or adverse modification finding on the parcel increases, all else equal ($\rho < 0$); and
- $cor\left(M_{jt}, \mathbf{1}[Treat]_j\right) < 0$ given FWS' documented efforts to exempt some developable parcels from CH regulation that are likely to otherwise have a destruction or adverse modification finding ($c < 0$)

then $\hat{\mu}$ will be larger than the "true" DID coefficient φ when $cov\left(M_{jt}, \mathbf{1}[Treat]_j\right) \neq 0$. For example, if $\hat{\mu} = -10.0$ and $\hat{\rho}\hat{c} = 2.0$ then $\hat{\varphi} = -12.0$ In other words, the impact of CH on undeveloped or bare parcel value is likely more negative than we find above when $\hat{\mu} < 0$ and likely less positive than we find above when $\hat{\mu} > 0$.

We do believe that $cov\left(M_{jt}, \mathbf{1}[Treat]_j\right) < 0$ is very likely for an unknown number of CHs in our analysis. Even though (Auffhammer et al. 2020) claim there is no widespread evidence that FWS significantly modified CH boundaries to reflect economic concerns in two California CHs, we have serval reasons to suspect this is not true more generally. First, there are many documented cases of FWS working with large developers on ESA-compliant habitat conservation plans and other mitigation strategies to avoid designating their land and projects as CH. For example, during the creation of CH for the Pacific coast population of the western snowy plover, the FWS worked with Lawson's Landing Inc. and Oxfoot Associates to make Dillon Beach in Marin County California CH-compliant. In exchange Dillon Beach was not made part of the snowy Plover's CH (USDOIFWS 2005). Presumably, the FWS is working with these companies to avoid a likely "destruction or adverse modification finding." Second, there are some documented cases of slight changes to CH boundaries in response to economic realities. For example,

> [w]e modified the boundaries of this critical habitat designation around the City of Gunnison. We refined the boundary to leave out areas of medium to high-intensity development, airport runways, and golf courses. In all other areas, lands covered by buildings, pavement, and other manmade structures, as of the effective date of this rule, are not included in this designation, even if they occur inside the boundaries of a critical habitat unit, because such lands lack physical and biological features essential to the conservation of Gunnison sage-grouse, and hence do not constitute critical habitat as defined in section 3(5)(A)(i) of the Act. (p. 69313, USDOIFWS 2014).

In other words, FWS regulators appear to be looking for ways to avoid designating areas with high M_{jt} as CH. Therefore, it is likely $cov\left(M_{jt}, \mathbf{1}[Treat]_j\right) < 0$ in an unknowable number of cases. And even though using matched control sales can reduce the potential of omitted variable bias (i.e., if the parcels that are treated tend to have a lower M_{jt} so would the matched controls if they happened to be treated), it may not completely eliminate it.

Omitted variables are not the only potential source of bias in our estimates of DID coefficients. CHs that have been revised several times due to new information, new scientific data, political pressure, or court cases are not included in our study because they make identification of the economic impact of CH more difficult (i.e., certain parcels may pop in and out of CH rapidly). However, we suspect that these complex CHs are likely to have had a greater impact on land prices than the "simple" CHs that we currently include in our study. Presumably, their imposition of high economic cost is one of the main reasons for "complex" CHs unsettled path to finalization.[16] For example, the originally proposed Canada Lynx CH included 27,530 sq. km. of commercially managed forest land in Maine. However, for economic reasons, the final CH excluded all of this Maine forest land because of the Maine forestry industry's (voluntary) conservation strategy for the Canada Lynx and FWS' desire to "preserve partnerships" with Maine's forest industry. Then, three years later the FWS published a new "final" Canada Lynx CH that included most of this previously excluded Maine forest land. We would not be surprised if a study of the most recent final Canada Lynx CH uncovered evidence of negative impacts on affected forest land values given that the "new" CH covered land deemed very import to Maine's forest industry. Therefore, by not including the potentially more expensive CHs like the Canada Lynx, our estimates of the DID coefficients μ and ω likely underestimate the actual cost that CH imposes on the American land values.

[16] Not every complex CH case is driven by economic concerns. For example, Preble's meadow jumping mouse's original "final" CH area was expanded 75% due to "habitat considerations"

Although we cannot measure how consequential bias in the DID estimator μ is to our results, we are confident that our overall conclusion regarding the impact of CH on land values would not change even if we could correct for all sources of bias: CH treatment has had an inconsistent impact on US parcel values.

Identification issues

In addition, there are several data issues that hamper our ability to precisely identify the impact of CH on parcel prices. First, even though we claim that "simple" CHs have consistent boundaries between the proposal and finalization stage that is not always the case. Minor changes can be made between these two stages. For example, in the FR that announces the final designation of CH for Gunnison Sage-Grouse the term "[w]e modified the boundaries of this critical habitat designation around the City of Gunnison" refers to boundaries set forth in the proposed CH (USDOIFWS 2013, 2014). We use fuzzy DID timing in order to avoid having to account for sales that took place on land proposed for CH; in our model either a parcel was certainly regulated by CH or not. However, market participants may not realize that land proposed for regulation was not finalized as CH. Therefore, there may be some post-CH establishment control sales in our model estimates that were affected by the perception of CH regulation.

Second, during the course of our research we learned that some of the digital maps of CH areas available from the FWS' website only approximate the actual CH areas (personal communication with Maura Flight, 6/25/21). Official CH areas are described with coordinates and printed maps in FR notices and in some cases the digital representation of these areas do not exactly follow official boundaries. Therefore, our analysis likely has some false positives (parcels that we treat as “treated” but in fact are not) and false negatives (parcels that we treat as “not treated” but in fact are). We will investigate the impact of digital map measurement error in future iterations of this research.

A third identification issue is created by the perceived versus actual regulatory “bite” of the CH rule. It is debatable whether the CH rule adds any regulatory bite above and beyond more general ESA regulations in CH areas that include occupied habitat. The destruction or modification of occupied habitat is prevented by ESA regulations; in these areas CH regulations may be superfluous. However, CH area can also cover unoccupied habitat. In these areas the CH regulation may be impactful because the take and jeopardy provisions of the ESA are less relevant in these landscapes. In unoccupied habitat areas the CH regulation may be the only relevant barrier to habitat destruction or modification. Therefore, the better DID model for investigating the impact of CH on land values may be to treat CH areas that include unoccupied habitat as “treated” areas and all other ESA-affected areas, including CH area over occupied habitat, as the control areas.

Acknowledgments
The authors acknowledge the Minnesota Supercomputing Institute (MSI) at the University of Minnesota for providing resources that contributed to the research results reported within this paper. URL: http://www.msi.umn.edu. We thank Zillow, Inc. for having made ZTRAX available free of charge for U.S. academic, nonprofit, and governmental researchers. Christoph Nolte acknowledges support from the Department of Earth & Environment at Boston University, the Junior Faculty Fellows program of Boston University's Hariri Institute for Computing and Computational Science, and The Nature Conservancy. The authors also acknowledge the insights provided by Katherine Fosburgh, Maura Flight, Robert W. Paterson, and the participants from the 2021 PLACES webinar.

References

Angrist, J. and Pischke J.-S. 2009. Mostly Harmless Econometrics. Princeton, NJ: Princeton University Press.

Ardeshiri, Ali, Ken Willis, and Mahyar Ardeshiri. 2018. Exploring preference homogeneity and heterogeneity for proximity to urban public services. Cities 81: 190-202. https://doi.org/10.1016/j.cities.2018.04.008

Armstrong, A. 2002. Critical Habitat Designations under the Endangered Species Act: Giving Meaning to the Requirements for Habitat Protection. SC Envtl. LJ, 10, p.53.

Armsworth, Paul R., Gretchen C. Daily, Peter Kareiva, and James N. Sanchirico. 2006. Land market feedbacks can undermine biodiversity conservation. PNAS. 103 (14) 5403-5408. https://doi.org/10.1073/pnas.0505278103

Auffhammer, Maximilian, Maya Duru, Edward Rubin, and David L. Sunding. 2020. The Economic Impact of Critical-Habitat Designation: Evidence from Vacant-Land Transactions. Land Economics 96 (2) (03): 188-206. https://doi.org/10.3368/le.96.2.188.

Callaway, Brantly, and Sant'Anna, Pedro H. C. 2019. Difference-in-Differences with Multiple Time Periods. Available at SSRN: https://ssrn.com/abstract=3148250 or http://dx.doi.org/10.2139/ssrn.3148250

De Chaisemartin, C. and d'Haultfoeuille, X. 2020. Two-way fixed effects estimators with heterogeneous treatment effects. American Economic Review 110(9): 2964-96.

Colorado Natural Heritage Program. 2019. The Colorado Ownership, Management and Protection Map (COMaP) [WWW Document]. URL https://comap.cnhp.colostate.edu.

Cunningham, Scott. 2020. Causal Inference. The Mixtape. Yale University Press: New Haven.

Black, Katie Jo. 2018. Wide open spaces: Estimating the willingness to pay for adjacent preserved open space. Regional Science and Urban Economics 71: 110-121. https://doi.org/10.1016/j.regsciurbeco.2018.06.001

Dahal, Ram P., Robert K. Grala, Jason S. Gordon, Ian A. Munn, Daniel R. Petrolia, and J. Reid Cummings. 2019. A hedonic pricing method to estimate the value of waterfronts in the Gulf of Mexico. Urban Forestry & Urban Greening 41: 185-194. https://doi.org/10.1016/j.ufug.2019.04.004.

Daw, J. R., & Hatfield, L. A. 2018. Matching and Regression to the Mean in Difference-in-Differences Analysis. Health services research, 53(6), 4138–4156. https://doi.org/10.1111/1475-6773.12993

ESRI. 2009. North America Water Polygons [WWW Document]. URL https://bit.ly/2vFchmY.

Geoghegan, Jacqueline. 2002. The value of open spaces in residential land use. Land Use Policy 19(1): 91-98. https://doi.org/10.1016/S0264-8377(01)00040-0.

Goodman-Bacon, Andrew. 2018. Difference-in-Differences with Variation in Treatment Timing. NBER Working Paper No. 25018 September 2018

Harvard Forest. 2020. New England Protected Open Space [WWW Document].

Homer, C., Dewitz, J., Yang, L., Jin, S., Danielson, P., Xian, G., Coulston, J., Herold, N., Wickham, J. & Megown, K. 2015. Completion of the 2011 National Land Cover Database for the conterminous United States – representing a decade of land cover change information. Photogramm. Eng. Remote Sensing 81: 345–354.

Kiel, K. 2005. Environmental Regulations and the Housing Market: A Review of the Literature. Cityscape 8(1): 187-207. https:// www.jstor.org/stable/20868574.

Kling, R.W., Findley, T.S., Gahramanov, E. et al. 2015. Hedonic valuation of land protection methods: implications for cluster development. J Econ Finan 39: 782–806. https://doi.org/10.1007/s12197-014-9279-1

Kolstad, Charles D., and Frances C. Moore. 2020, Estimating the economic impacts of climate change using weather observations. Review of Environmental Economics and Policy 14(1): 1-24.

List, J.A., Margolis, M., Osgood, D.E., 2006. Is the Endangered Species Act Endangering Species? Working Paper 12777. National Bureau of Economic Research, Cambridge, MA.

Ma, S., & Swinton, S. M. 2012. Hedonic valuation of farmland using sale prices versus appraised values. Land Economics 88(1): 1-15.

Melstrom, R.T., 2021. The Effect of Land Use Restrictions Protecting Endangered Species on Agricultural Land Values. American Journal of Agricultural Economics 103(1): 162-184.

Microsoft, 2018. U.S. Building Footprints [WWW Document]. URL https://github.com/microsoft/USBuildingFootprints (accessed 2.2.20).

Morancho, Aurelia Bengochea. 2003. A hedonic valuation of urban green areas." Landscape and urban planning 66(1): 35-41.

Nelson, A. 2008. Travel Time to Major Cities: A Global Map of Accessibility. Global Environment Monitoring Unit, Joint Research Centre of the European Commission, Ispra, Italy.

Nolte, C. 2020. High-resolution land value maps reveal underestimation of conservation costs in the United States. Proc. Natl. Acad. Sci. 117: 29577–29583.

Nolte, C, Boyle, K., Chaudhry , A., Clapp, C., Guignet, D. , Hennighausen, H. , Kushner, I. , Liao, Y., Mamun, S., Pollack, A. , Richardson, J., Sundquist, S. , Swedberg, K. , Uhl, J.H. 2021. Studying the impacts of environmental amenities and hazards with nationwide property data: best data practices for interpretable and reproducible analyses. SSRN available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3900806

Quigley, John M., and , Aaron M. Swoboda. 2007. The Urban Impacts of the Endangered Species Act: A General Equilibrium Analysis. Journal of Urban Economics 61(2): 299-318.

Sander, Heather, Stephen Polasky, and Robert G. Haight. 2010. The value of urban tree cover: A hedonic property price model in Ramsey and Dakota Counties, Minnesota, USA." Ecological Economics 69(8): 1646-1656.

Sander, H. A., & Polasky, S. 2009. The value of views and open space: Estimates from a hedonic pricing model for Ramsey County, Minnesota, USA. Land Use Policy 26(3): 837-845.

Seo, Kihwan, Aaron Golub, Michael Kuby. 2014. Combined impacts of highways and light rail transit on residential property values: a spatial hedonic price model for Phoenix, Arizona. Journal of Transport Geography 41: 53-62. https://doi.org/10.1016/j.jtrangeo.2014.08.003.

Sunding, David L. 2003. Economic impacts of critical habitat designation for the coastal California Gnatcatcher. California Resource Management Institute Working Paper.

Sunding, D., Swoboda, A., & Zilberman, D. 2003. The economic costs of critical habitat designation: Framework and application to the case of California Vernal Pools. California Resource Management Institute.

The Trust for Public Land & Ducks Unlimited. 2020. The National Conservation Easement Database [WWW Document]. URL https://www.conservationeasement.us/

U.S. Census Bureau (USCB). 2019. 2019 TIGER/Line Shapefiles (machine-readable data files) [WWW Document]. URL https://www.census.gov/cgi-bin/geo/shapefiles/index.php.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2000. Endangered and Threatened Wildlife and Plants; Proposed Determination of Critical Habitat for the Peninsular Bighorn Sheep. Proposed rule. Federal Register / Vol. 65, No. 129 / Wednesday, July 5, 2000.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2001. Endangered and Threatened Wildlife and Plants; Final Determination of Critical Habitat for Peninsular Bighorn Sheep. Final rule. Federal Register / Vol. 66, No. 22 / Thursday, February 1, 2001.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2005. Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for the Pacific Coast Population of the Western Snowy Plover. Final rule. Federal Register / Vol. 70, No. 188 / Thursday, September 29, 2005.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2008. Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for the Peninsular Ranges Population of Desert Bighorn Sheep (Ovis canadensis nelsoni). Proposed rule. Federal Register / Vol. 73, No. 166 / Tuesday, August 26, 2008.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2009. Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for Peninsular Bighorn Sheep and Determination of a Distinct Population Segment of Desert Bighorn Sheep. Final rule. Federal Register / Vol. 74, No. 70 / Tuesday, April 14, 2009.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2013. Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for Gunnison Sage-Grouse. Proposed Rule. Federal Register / Vol. 78, No. 8 / Friday, January 11, 2013.
U.S. Department of the Interior, US Fish and Wildlife Service (USDOIFWS). 2014. Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for Gunnison Sage-Grouse. Final rule. Federal Register / Vol. 79, No. 224 / Thursday, November 20, 2014.
U.S. Fish and Wildlife Service (USFWS). 2017. Critical Habitat. What is it? https://www.fws.gov/endangered/esa-library/pdf/critical_habitat.pdf.
U.S. Fish & Wildlife Service. (USFWS). 2018. National Wetlands Inventory: Seamless Wetlands Data [WWW Document]. URL https://www.fws.gov/wetlands/data/data-download.html.
U.S. Fish & Wildlife Service (USFWS). 2021. USFWS Refined Range Maps for Threatened and Endangered Species. http://www.ecos.fws.gov. Accessed Feb 19, 2021
U.S. Geological Survey (USGS) 2017a. National Elevation Dataset [WWW Document]. URL https://lta.cr.usgs.gov/NED
U.S. Geological Survey. (USGS) 2017b. National Hydrography Dataset (NHD) USGS National Map [WWW Document]. URL https://nhd.usgs.gov/NHD_High_Resolution.html.
U.S. Geological Survey. 2018. Protected Areas Database of the United States (PAD-US).
Wu, J., Adams, R. M., & Plantinga, A. J. 2004. Amenities in an urban equilibrium model: Residential development in Portland, Oregon. Land Economics 80(1): 19-32.
Zabel, J.E., Paterson, R., 2006. The effects of critical habitat designation on housing supply: an analysis of California housing construction activity. J. Reg. Sci. 46: 67–95.

Zillow. 2019. ZTRAX: Zillow Transaction and Assessor Dataset, 2019-Q4 [WWW Document]. URL http://www.zillow.com/ztrax

Appendix

Table SI 1. Descriptive statistics associated with estimates of model (1) from Tables 1 and 2

	Parcel type	ESA range filter	All	Jaguar	Gunnison sage-grouse	Atlantic salmon	Amphibians	Riparian zone	Plants	Terr. animals	CA	CA – Amph.
Number of CHs	Dev.	No	62	1	1	1	8	38	17	5	12	3
		Yes	61	1	1	1	8	38	17	5	12	3
	Undev.	No	70	1	1	1	9	41	21	7	15	4
		Yes	70	1	1	1	9	41	21	7	15	4
Number of treated sales	Dev.	No	31,049	236	279	1,412	1,549	9,587	247	21,195	1,368	1,213
		Yes	30,928	236	279	1,344	1,514	9,484	247	21,195	1,368	1,213
	Undev.	No	12,591	100	198	188	685	5,256	218	47,325	608	579
		Yes	12,569	100	198	179	672	5,234	218	47,341	608	579
Number of control sales	Dev.	No	57,449	476	553	2,813	3,087	16,576	481	40,355	2,720	2,418
		Yes	57,271	476	554	2,673	3,020	16,431	481	40,355	2,719	2,417
	Undev.	No	21,872	202	376	378	1,277	8,934	430	54,463	1,139	1,083
		Yes	21,821	202	377	360	1,248	8,892	429	54,462	1,136	1,080
Average of log of treated sale price before treatment (SD)	Dev.	No	11.5664 (1.7912)	10.0123 (1.223)	10.5806 (1.8259)	8.0478 (2.2737)	11.3268 (1.7748)	10.1934 (1.8086)	9.9434 (1.8397)	12.0306 (1.5277)	11.5465 (1.7697)	11.5767 (1.7558)
		Yes	11.5789 (1.7815)	10.0123 (1.223)	10.5727 (1.821)	7.8955 (2.1764)	11.3502 (1.7775)	10.2139 (1.7953)	9.9442 (1.8208)	12.0307 (1.528)	11.5473 (1.767)	11.5782 (1.7535)
	Undev.	No	10.2167 (2.1914)	8.2661 (1.3988)	9.1611 (2.082)	6.0474 (1.9574)	9.6667 (1.8943)	8.7034 (1.9769)	8.8349 (1.8168)	11.0758 (1.7697)	9.7995 (1.8927)	9.8519 (1.8642)
		Yes	10.2236 (2.1907)	8.2617 (1.3972)	9.1269 (2.1044)	5.7514 (1.7362)	9.6776 (1.9022)	8.7035 (1.977)	8.8481 (1.8132)	11.0786 (1.769)	9.8035 (1.8943)	9.8549 (1.8664)
Average of log of treated sale price after treatment (SD)	Dev.	No	13.505 (2.1639)	11.9015 (1.0339)	12.3126 (2.1204)	11.5854 (1.4018)	12.9139 (2.2311)	11.5402 (1.8151)	12.0753 (2.0815)	14.6722 (1.363)	12.9222 (2.3156)	13.0816 (2.307)
		Yes	13.5049 (2.1643)	11.8966 (1.0394)	12.3056 (2.1243)	11.5946 (1.4017)	12.9118 (2.234)	11.5378 (1.8135)	12.0712 (2.082)	14.6738 (1.3602)	12.9215 (2.3169)	13.0809 (2.3088)
	Undev.	No	11.676 (2.8379)	10.6695 (1.9777)	10.7847 (2.5448)	9.8107 (2.1056)	11.8296 (2.1517)	9.7982 (2.2976)	10.9925 (2.4206)	13.4788 (2.0322)	11.7613 (2.1975)	11.9717 (2.0825)
		Yes	11.677 (2.8402)	10.5659 (1.8745)	10.8279 (2.5568)	9.8178 (2.1201)	11.8115 (2.1246)	9.7964 (2.3017)	10.9758 (2.4232)	13.4821 (2.0286)	11.7623 (2.1847)	11.9765 (2.0631)
Average of log of control sale price (SD)	Dev.	No	12.3018 (2.1682)	10.8496 (1.4018)	11.6009 (2.2165)	11.2153 (1.9012)	11.8195 (2.0318)	10.865 (1.9534)	11.5625 (2.0532)	12.9016 (1.9643)	12.0468 (2.0538)	12.0519 (2.0512)
		Yes	12.3086 (2.1646)	10.8466 (1.402)	11.5816 (2.2163)	11.2573 (1.8793)	11.8324 (2.0357)	10.8735 (1.9477)	11.5623 (2.046)	12.9021 (1.964)	12.0463 (2.0518)	12.0521 (2.0491)
	Undev.	No	10.8388 (2.6513)	9.5266 (2.0928)	10.2671 (2.5575)	9.323 (2.4431)	10.5814 (2.3061)	9.2388 (2.2382)	10.3631 (2.4529)	11.9843 (2.2674)	10.612 (2.2921)	10.6914 (2.2627)
		Yes	10.8437 (2.6543)	9.4551 (2.0099)	10.2922 (2.6012)	9.3244 (2.468)	10.593 (2.2904)	9.2362 (2.2431)	10.3316 (2.4442)	11.9913 (2.2656)	10.6152 (2.2852)	10.6973 (2.2553)

Table SI 2. Descriptive statistics associated with estimates of model (1) from Table 6

	ESA range filter	High BPR	Low BPR
Number of CHs	No	44	57
	Yes	42	56
Number of treated sales	No	26,575	4,474
	Yes	26,537	4,391
Number of control sales	No	48,979	8,470
	Yes	48,933	8,338
Average of log of treated sale price before treatment (SD)	No	11.8554 (1.5669)	9.0855 (1.6738)
	Yes	11.8581 (1.5643)	9.0931 (1.6648)
Average of log of treated sale price after treatment (SD)	No	14.2352 (1.6163)	10.6438 (1.587)
	Yes	14.2346 (1.6173)	10.6409 (1.5842)
Average of log of control sale price (SD)	No	12.7024 (1.9692)	9.985 (1.7798)
	Yes	12.703 (1.9688)	9.994 (1.7787)

Table SI 3. Descriptive statistics associated with each estimate of models (2) and (1) from Table 4.

			Not matched	Not matched	Matched	Matched
	Parcel type	**ESA range filter?**	**Estimate of (2) using panel dataset formed from "All CHs" set**	**Estimates of (2) and (1) using panel dataset formed from "All CHs" set – complete observations only**	**Estimate of (2) using panel dataset formed from "All CHs" set**	**Estimates of (2) and (1) using panel dataset formed from "All CHs" set – complete observations only**
Number of CHs	Dev.	No	89	84	46	41
		Yes	87	82	45	41
	Undev.	No	85	82	45	44
		Yes	82	80	49	48
Number of treated sales	Dev.	No	36,689	25,578	22,045	14,616
		Yes	36,637	25,533	21,986	14,574
	Undev.	No	12,868	9,632	7,552	5,719
		Yes	12,814	9,597	7,558	5,696
Number of control sales	Dev.	No	1,433,066	1,017,170	40,504	28,241
		Yes	1,409,811	998,186	40,221	28,252
	Undev.	No	153,543	132,630	13,432	10,864
		Yes	151,856	131,393	13,361	10,810
Average of log of treated sale price before treatment (SD)	Dev.	No	11.3132 (1.5208)	11.3675 (1.5279)	11.569 (1.7480)	11.6603 (1.7074)
		Yes	11.292 (1.5214)	11.341 (1.5290)	11.6156 (1.7432)	11.7063 (1.7068)
	Undev.	No	9.9874 (1.9308)	10.0391 (1.9306)	10.5206 (1.9600)	10.6893 (1.8945)
		Yes	9.9726 (1.9364)	10.0208 (1.9350)	10.5274 (2.0085)	10.7052 (1.9420)
Average of log of treated sale price after treatment (SD)	Dev.	No	13.1487 (1.7611)	13.1697 (1.7678)	13.4964 (2.1811)	13.641 (2.1499)
		Yes	13.1388 (1.7602)	13.1566 (1.7667)	13.5164 (2.1913)	13.6349 (2.1771)
	Undev.	No	12.0495 (2.3415)	12.16 (2.3027)	11.6176 (2.7564)	12.1071 (2.6689)
		Yes	12.0452 (2.3428)	12.1564 (2.3026)	11.6417 (2.8007)	12.1545 (2.7214)
Average of log of control sale price (SD)	Dev.	No	12.2862 (1.8827)	12.2822 (1.8780)	12.18 (2.1069)	12.2965 (2.0848)
		Yes	12.2804 (1.8859)	12.2725 (1.8821)	12.2402 (2.1108)	12.3471 (2.0945)
	Undev.	No	11.3083 (2.4151)	11.4231 (2.4055)	10.9332 (2.4142)	11.2022 (2.3496)
		Yes	11.3064 (2.4202)	11.4193 (2.4098)	10.9661 (2.4873)	11.2493 (2.4328)

Table SI 4. CHs in each cohort where a cohort includes CHs that were proposed and finalized at the approximate same time

Cohort	1	2	3
CH proposal period	6/06 – 11/06	8/11 – 11/11	5/12 – 10/12
CH finalization period	8/07 – 12/07	7/12 – 11/12	7/13 – 12/13
End date of property sale included	12/31/2009	11/30/2014	12/31/2015
	Yadon's piperia (Piperia yadonii)	DeBeque phacelia (Phacelia submutica)	Jollyville Plateau Salamander (Eurycea tonkawae)
	Chipola slabshell (Elliptio chipolaensis)	Pagosa skyrocket (Ipomopsis polyantha)	Diamond Darter (Crystallaria cincotta)
	Fat threeridge (mussel) (Amblema neislerii)	Narrow pigtoe (Fusconaia escambia)	Fluted kidneyshell (Ptychobranchus subtentus)
	Gulf moccasinshell (Medionidus penicillatus)	Tapered pigtoe (Fusconaia burkei)	Slabside Pearlymussel (Pleuronaia dolabelloides)
	Ochlockonee moccasinshell (Medionidus simpsonianus)	Chucky Madtom (Noturus crypticus)	Franciscan manzanita (Arctostaphylos franciscana)
	Oval pigtoe (Pleurobema pyriforme)	Rush Darter (Etheostoma phytophilum)	
	Purple bankclimber (mussel) (Elliptoideus sloatianus)	Yellowcheek Darter (Etheostoma moorei)	
		Laurel dace (Chrosomus saylori)	

Table SI 5. CHs included in our dataset

See Table SI 5.xlsx

Text SI 1

See Text SI 1.docx

Text SI 2: Matching procedure (Mahalanobis distance matching)

1. Collect all the sales from 42 states
2. Remove multiple property sales
3. Create variables: Create variables that we will use.
4. Filter out data:
   a) Remove if multiple CH
   b) Remove if sales before 2000
   c) Remove if proposed publication date (proposed_date) before 2000
   d) Remove if not ‘simple’ CH
5. Property type:
   a) Residential property with building: building code is RR and building gross area is more than 0 square feet.
   b) Property without building: building gross area is not more than 0 square feet.

Matching procedure

   a) Define treatment and control
      i) First step is to look for control within the buffer of polygons in the county. If control observation is less than 5 times of treatment or number of control observations is less than 100, then we look for control in step ii.
      ii) Second step is to look for control within the county. Again, we use the same criteria. If it fulfills the rejection criteria, we go to step iii.
      iii) We look for control in neighboring counties.
   b) Matching algorithm:
      i) Number of match (M): 2 [one to two matching]
      ii) Treatment variable for matching: *‘polygon’*
      iii) co-variates used for matching: *'bld_n_rooms', 'bld_n_baths', 'bld_yr_combined', 'bld_gross_area', 'lake', 'ha', 'slope', 'elev', 'travel_weiss', 'rd_dist_hwy', 'rd_dist_pvd', 'p_bld_fp_5000', 'p_wet', 'cst_2500', 'p_prot_2010_1000', 'property_type'*
      iv) Exact variable: *'property_type'*
      v) Ties: TRUE
      vi) Replace: FALSE
      vii) Weight: 2 [Mahalanobis distance]

SI Figures

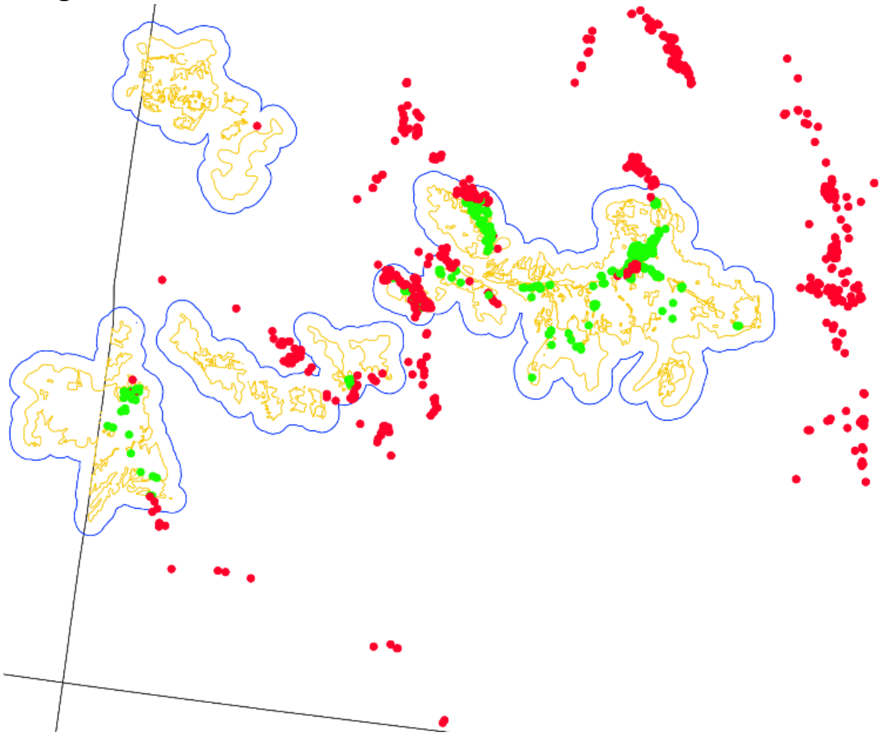

Figure SI 1. Map of Gunnison sage-grouse CH and control sales between 2000 and 2019. Each green dot represents a parcel in the Gunnison sage-grouse CH that had at least one sale between 2000 and 2019. Each red dot represents a parcel that had at least one matched control sale between 2000 and 2019. The orange boundary is the CH borders while the blue boundary represents 5 km buffer. Sales of any parcel that were ineligible for inclusion in our dataset for other reasons are not shown here.

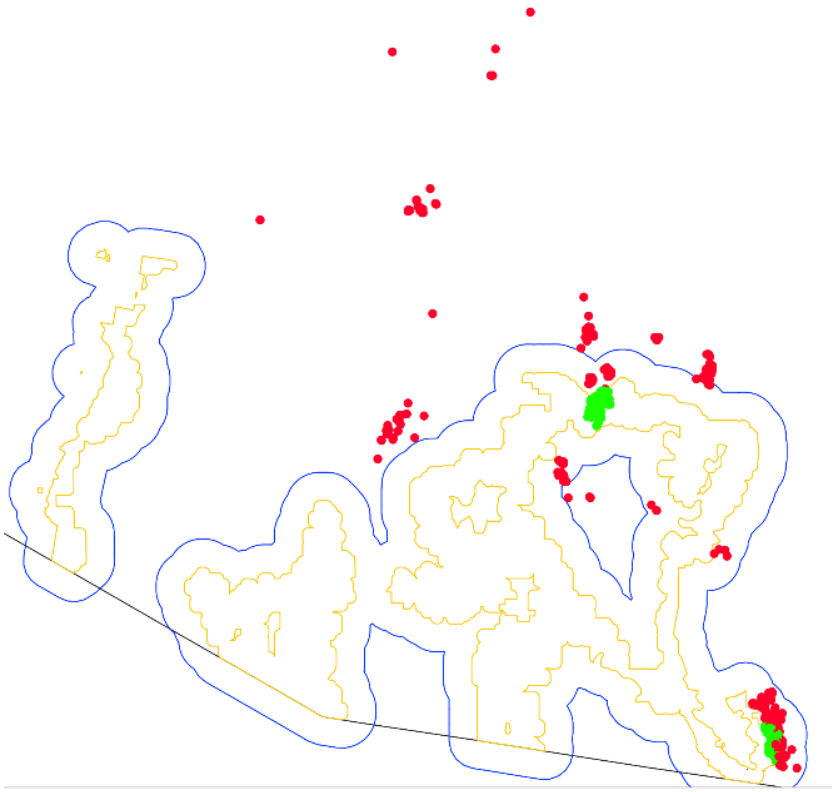

Figure SI 2. Map of Jaguar CH and control sales between 2000 and 2019. Each green dot represents a parcel in the jaguar CH that had at least one sale between 2000 and 2019. Each red dot represents a parcel that had at least one matched control sale between 2000 and 2019. The orange boundary is the CH borders while the blue boundary represents 5 km buffer. Sales of any parcel that were ineligible for inclusion in our dataset for other reasons are not shown here.

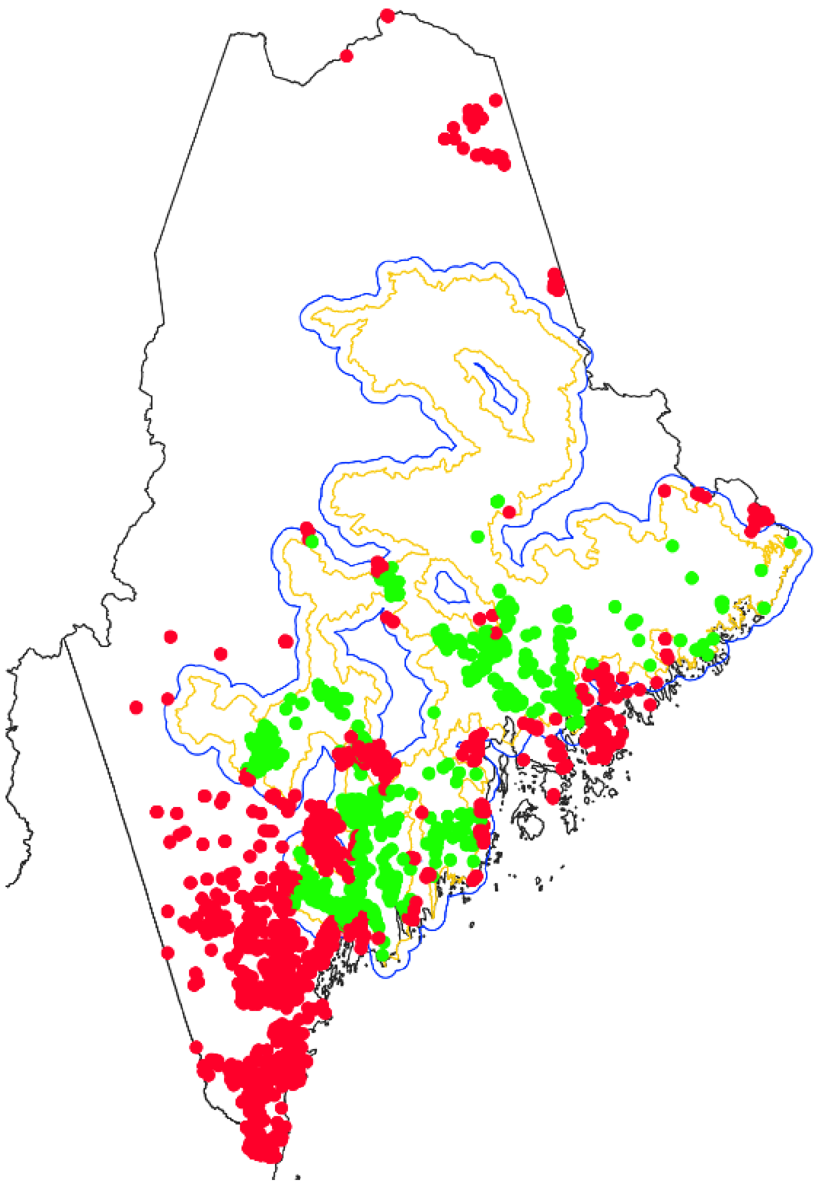

Figure SI 3. Map of Atlantic Salmon CH and control sales between 2000 and 2019. Each green dot represents a parcel in the Atlantic Salmon CH that had at least one sale between 2000 and 2019. Each red dot represents a parcel that had at least one matched control sale between 2000 and 2019. The orange boundary is the CH borders while the blue boundary represents 5 km buffer. Sales of any parcel that were ineligible for inclusion in our dataset for other reasons are not shown here.

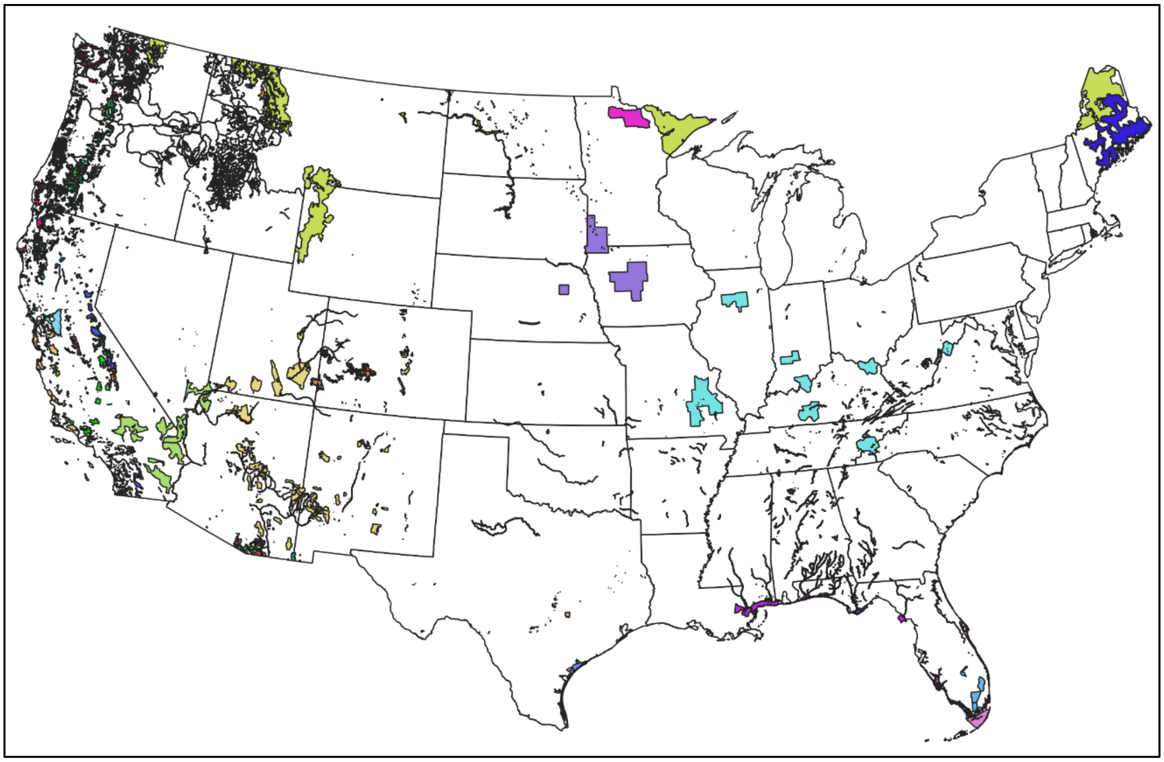

Figure SI 4. Map of all CHs as of 2020. Each CH is different background colors. Many of these CHs are not in our dataset.

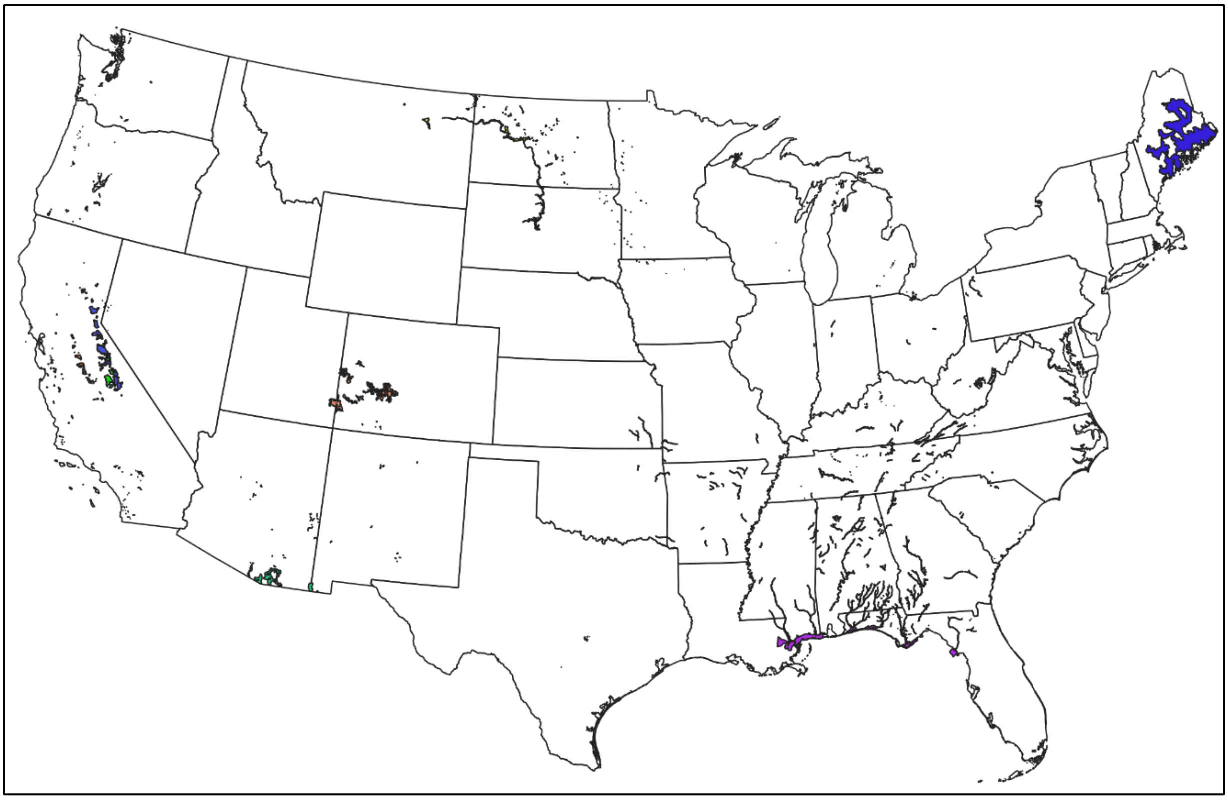

Figure SI 5. Map of all CHs in our dataset. Each CH is different background colors. The bottom map only includes CHs that meet our time restriction (after 2000), are “simple,” have valid treated sales between 2000-2019, and have valid matching sales within 5-km buffer between 2000-2019.

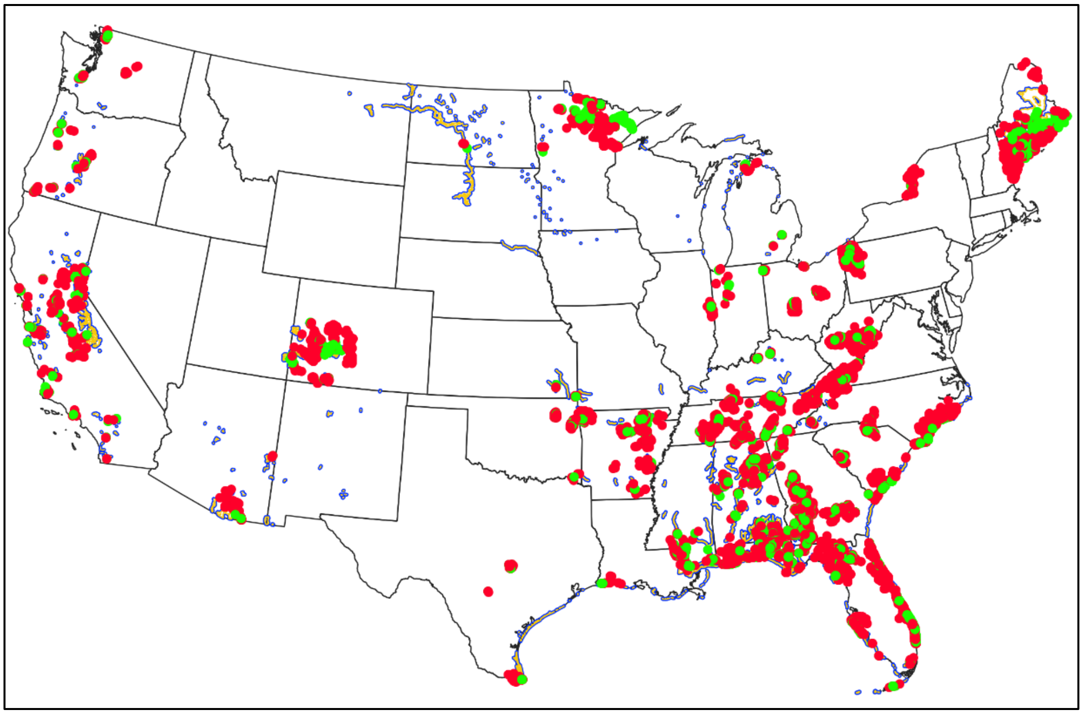

Figure SI 6. Map of CH and control sales between 2000 and 2019, not filtered for ESA range. Each green dot represents a parcel in a CH that had at least one sale between 2000 and 2019. Each red dot represents a parcel that had at least one matched control sale between 2000 and 2019. The polygons in the background are the CH areas included in our dataset. Sales of any parcel that were ineligible for inclusion in our dataset for other reasons are not shown here. Top map does not filter for CH range, bottom map filter for CH range.

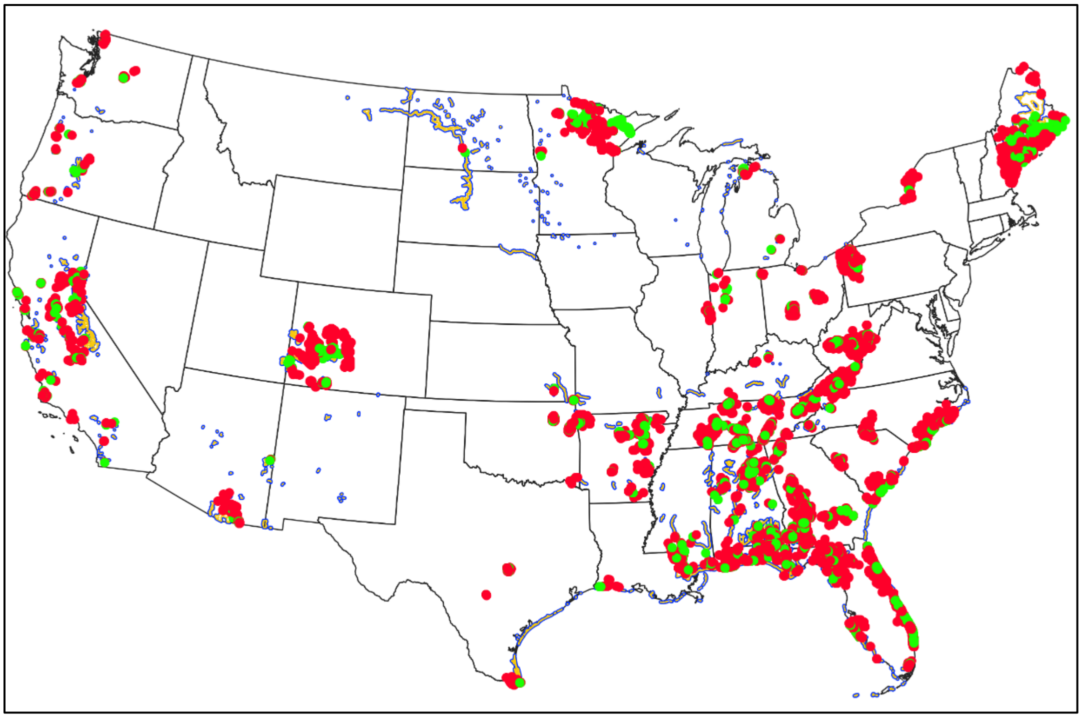

Figure SI 7. Map of CH and control sales between 2000 and 2019, filtered for ESA range. Each green dot represents a parcel in a CH that had at least one sale between 2000 and 2019. Each red dot represents a parcel that had at least one matched control sale between 2000 and 2019. The polygons in the background are the CH areas included in our dataset. Sales of any parcel that were ineligible for inclusion in our dataset for other reasons are not shown here.