

The Effectiveness of Listing under the U.S. Endangered Species Act

An econometric analysis using matching methods

Paul J. Ferraro¹, Craig McIntosh² and Monica Ospina³

PRELIMINARY DRAFT. NOT FOR CITATION. DATA SET INCOMPLETE

NOTE: The emphasis of this paper is on the methods by which one can estimate the effectiveness of the U.S. Endangered Species Act. Our data are still incomplete and therefore the empirical results in this paper should be considered preliminary.

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¹ Assistant Professor, Department of Economics, Andrew Young School of Policy Studies, P.O. Box 3992, Georgia State University, Atlanta, GA 30302-3992; pferraro@gsu.edu

² Assistant Professor, School of International Relations and Pacific Studies, University of California-San Diego, San Diego, CA; ctmcintosh@ucsd.edu

³ Graduate Research Assistant, Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, GA; prcmox@langate.gsu.edu

Abstract

This paper uses a novel dataset to examine the selection and treatment effects of the Endangered Species Act (ESA). We demonstrate that the process through which species are listed has changed strongly over time, invalidating previous work that identifies treatment effects off of the duration of listing. ESA listing and funding decisions for species conservation focus on the most endangered species, and among unlisted species, the most endangered show the greatest recovery during the period 1994-2003. In a simple regression analysis, we demonstrate that outcomes among listed species are strongly differentiated according to the amount of funding that they receive. Using propensity-score matching to select counterfactual unlisted species, we find that listing alone results in outcomes weakly worse than the counterfactual, while listing and funding together significantly improve recovery.

I. Introduction

The Endangered Species Act (ESA) is one of the most controversial pieces of environmental legislation in the United States. Enacted in 1973 and implemented by the Fish and Wildlife Service (FWS),¹ the ESA has the simple but ambitious objective of reversing the decline of endangered species. Supporters call the ESA the “crown jewel” of the nation’s environmental legislation and an absolutely essential tool for protecting biodiversity. Opponents claim that the ESA imposes unreasonable costs on society while delivering few benefits.

Both opponents and supporters are concerned that the ESA, as currently implemented, may be ineffective at halting or reversing the decline of endangered species. Constraints on its effectiveness arise from the ESA’s species-level rather than ecosystem-level focus, vague or contradictory legislative rulings, interest group pressures that warp listing decisions, and landowner actions that preemptively harm species and their habitat in order to avoid regulatory burdens (for an economist’s perspective, see Brown and Shogren 1998; for a biologist’s perspective, see Wilcove et al. 1993).

If indeed the ESA is *ineffective* at improving the status of endangered species, then the parallel debate about its *cost-efficiency* is no longer relevant. Although the ESA and its controversy have been around for decades, there are surprisingly few empirical studies on its effectiveness.

A few studies have attempted to empirically verify widely cited anecdotes (for examples of anecdotes, see Mann and Plummer 1995; Hollingsworth 1998; and Stroup 1995) and theoretical modeling (Innes 1997; Polasky and Doremus 1998) of preemptive habitat destruction. Lueck and Michael (2003) and Zhang (2004), in the context of red-cockaded woodpeckers, and Margolis et al. (2004), in the context of pygmy owls, present data that are consistent with preemptive habitat manipulation. However, it is unclear if empirical results from two contexts can be extrapolated to nationwide trends.

To bolster their case, ESA opponents often point to the paucity of delisted species as evidence of the ESA’s failure (e.g., Mann and Plummer; Gordon et al. 1997). Of the twenty-four delisted U.S. species, sixteen were delisted because they went extinct or because the original data

¹ The National Marine Fisheries Service (NMFS) is the lead agency implementing the ESA for select marine species, but the focus of this paper is on terrestrial and freshwater vertebrates and thus we ignore the role of NMFS.

were in error.² However, as noted by Doremus and Pagel (2001), delisting requires ongoing protective measures, which under current law are often effectively provided only by the ESA itself. Consequently, lengthy residence on the list should be expected for the majority of species.

Opponents, however, are also able to point to the paucity of listed species exhibiting recovery (improved status). According to the FWS's 1992 biannual report on the ESA (FWS 1992), the populations of less than 10% of all listed species were known to be improving. The observation that listed species continue to decline may seem to imply that the ESA is failing, but the observation lacks consideration of the counterfactual: would listed species be even worse off had they not been listed? As we will demonstrate in section IV, listed species tend to be more endangered, on average, than other species. Thus the fact that the average listed species has seen no improvement in its status or does not seem to be doing any better than the average unlisted species does not necessarily imply the ESA is ineffective. The average unlisted species does not represent what the average listed species would look like in the absence of the ESA.

Like their opponents, supporters of the ESA marshal evidence to support their view that the ESA is effective despite its limitations. For example, ESA proponents claim that the ESA may not have led to improvements in the populations of many species, but it has prevented hundreds of extinctions (NRC 1995, Schwartz 1999). However, determining if a species is in fact extinct is difficult. Ascribing the absence of the extinction to the ESA is even more difficult. Suckling et al. (2004) note that of the one hundred eight U.S. species known to have become extinct in the first 21 years following the creation of the ESA, the majority (85) were unlisted species. However, the pool of unlisted species is much larger than the pool of listed species and thus absolute numbers of extinctions do not reveal much.

Building on a simpler analysis by Rachlinski (1997), Taylor et al. (2005) use logistic regression to examine the effect of the number of years listed on the likelihood that a species has been declared to be recovering or declining by the FWS in the 1990s. They find that the longer a species was listed, the more likely it was to be improving and the less likely it was to be declining. Although this analysis makes great improvements over previous arguments for and against the act, it still suffers from two important shortcomings. The first, and less serious, is the

² From http://ecos.fws.gov/tess_public/TESSWebpageDelisted?listings=0, last accessed 3/5/05. Mann and Plummer assert that only the delisting of the American alligator and Aleutian Canada goose (a subspecies of the widespread Canadian Goose) can be attributed to the ESA. Other cases, such as the brown pelican and peregrine falcon that benefited from the ban of the pesticide DDT, recovered from actions taken outside the context of the ESA.

use of FWS measures of species status as the outcome variable. The FWS measures of species status have been criticized as too subjective (NRC) and, given they are not constructed using transparent criteria, may be manipulated to achieve agency objectives.³

A more important shortcoming is the counterfactual used in these analyses. The sample consists of only listed species. Thus the counterfactual is a listed species that has not been listed as long. For this counterfactual to be valid, the listing process must be the same over time. However, there is substantial evidence that the listing process has in fact changed over time (see our results below for empirical evidence; see Doremus (1997) for narrative evidence). Evaluating the direction of the bias is not straightforward. Many of the first listed species may have been close to extinction and thus perhaps hard to recover (e.g., California Condor). The coefficient on “time listed” would thus be biased down. However, many of the first species listed may have also been simply charismatic species that, while certainly threatened, were relatively easier to recover (e.g., birds of prey, which could be recovered without listing through the banning of DDT). The coefficient on “time listed” would thus be biased up.

The only paper to use unlisted species to construct the counterfactual is a working paper by Kerkvliet and Langpap (2002). However, their paper uses all unlisted species as controls, which implicitly assumes that the covariates in their regression model (taxonomic class, size, and level of endangerment) are uncorrelated with the error term. As we will demonstrate below, and as others have argued (Metrick and Weitzman 1996), the decision to list a species under the ESA is not random, but associated with observable characteristics of species that also affect recovery. Thus constructing a counterfactual from the set of all unlisted species will lead to a biased estimate of the ESA’s effect.⁴

All of the evidence marshaled to date for and against the ESA has a common problem: the absence of a well-chosen counterfactual. The analyst interested in evaluating the ESA is faced with a problem of missing data: one cannot observe the change in a listed species’ population in the absence of listing. In the next section, we discuss our approach to constructing

³ Since at least 1994, the FWS has attempted to measure the correlation between the length of time that a species has been listed and its status measure in an attempt to show that the ESA is successful.

⁴ Kerkvliet and Langpap also suffers from two other shortcomings: (1) the time period over which they are attempting to detect a change in status is only four years; and (2) on the right-hand side of their regression, they put the species status rank at the **end** of the period, which has no theoretical basis and also happens to bias the coefficient on “listing” in the positive direction. The bias arises because endangered species tend to have lower status ranks while unlisted species have higher ones (thus to achieve a given status rank at the end of the period, the listed species is likely to have improved and the unlisted species is likely to have declined).

this counterfactual. In section III, we present a brief history of the ESA and describe our data. In section IV, we present evidence that previous efforts at evaluating the ESA have not constructed their counterfactuals well. We also explain why such construction is particularly difficult in the context of the ESA. In Section V, we perform simple regressions that illustrate how listing and funding are related to changes in endangerment that occur over the period 1993-2004. In Section VI, we use propensity score matching methods to construct a counterfactual for listed vertebrate species. We find no evidence that these listed species fare any better than unlisted species from 1993 to 2004. We do, however, find evidence that the *combination* of listing and federal funding for recovery efforts can be effective in improving the status of a species. In Section VII, we discuss alternative interpretations of our results and conclude.

II. The Missing Counterfactual in ESA Analyses

The goal of program evaluation is to construct a proper counterfactual. Let L_i denote the listing decision for the i th species, where $L_i = 1$ when the species is listed and $L_i = 0$ when it is not listed. Let S_{1i} denote the i th species' endangerment status when it is listed and S_{0i} denote status when the species is not listed. Higher values of S are preferred (they imply more secure populations). Thus the expected change in status for a species that is listed (i.e., the average treatment effect on the treated, or ATT) is: $ATT = E[S_{1i} - S_{0i} | L_i = 1]$.

If the ESA is effective, $ATT > 0$. Of course, estimating ATT is not straightforward because we cannot observe the counterfactual, $E[S_{0i} | L_i = 1]$, which is what the species' status would have been had it not been listed. If listing is allocated randomly across species, then we can estimate the counterfactual simply by using the status of unlisted species because: $E[S_{0i} | L_i = 1] = E[S_{0i} | L_i = 0]$ (i.e., the expected status in the absence of the ESA is identical for listed and unlisted species).

However, the decision to list a species under the ESA is determined by observable characteristics of the species and their circumstances. Thus listed and unlisted species, on average, differ in characteristics that may also affect status changes after listing (i.e., propensity to recover or decline). In other words, in a model that regresses changes in a species' status on its ESA listing and other covariates likely to affect recovery, the error term will be correlated with one or more of the covariates, leading to a biased estimate of the effect of listing.

In the presence of such potential bias, the methods of matching provide one way to assess the effect of listing under the ESA. Matching works by, *ex post*, identifying a comparison group that is “very similar” to the treatment group with only one key difference: the comparison group did not participate in the program of interest. Matching mimics random assignment through the construction of a control group post hoc.

We adopt the method of propensity score matching (PSM), developed in Rosenbaum and Rubin (1983, 1984). This method has been used extensively in the labor (e.g., Dehejia and Wahba, 1999, 2002; Heckman *et al.*, 1997, 1998a, 1998b) and development economics literature (e.g., Deininger et al. 2004; Duflo 2001; Jalan and Ravallion 2003), as well as in a few studies in environmental economics (Frederiksson and Millimet 2004; List et al. 2003; Margolis et al.). If the researcher can select observable characteristics so that any two species with the same value for these characteristics will display homogenous responses to the treatment, then the treatment effect can be measured without bias. Measuring the average treatment effect on the treated without bias requires that, given a vector of covariates, Z : $E[S_{0i} / L_i = 1, Z] = E[S_{0i} / L_i = 0, Z] = E[S_{0i} / Z]$. Called the “conditional independence assumption” (CIA), this assumption implies that given Z , the non-treated outcomes are what the treated outcomes would have been had they not been treated.

Note that the CIA implies that selection into treatment occurs only on observable characteristics. In the case of the ESA, this implication is less problematic than in voluntary programs involving humans. Species do not volunteer for listing; the FWS selects them using observable characteristics. Of course, the variables observable to FWS employees are not necessarily observable to the analyst. We will argue below, however, that we have assembled a set of covariates that encompass the relevant observable characteristics that affect the listing decision and the recovery of endangered species in a systematic way.

Matching on Z , however, can be difficult when one has more than a few covariates. Rosenbaum and Rubin (1983) proved that that matching on the probability of participating conditional on the vector of covariates (“propensity score”) could achieve consistent estimates of the treatment effect in the same way as matching on all covariates. The propensity score can be estimated by a simple logit or probit model with the treatment as the dependent variable. The PSM method constructs probabilistically equivalent groups on observed characteristics. If matching is successful, the outcome of the matched, non-experimental control group is

equivalent to the outcome of an experimental control group and thus serves as an appropriate counterfactual.

III. ESA History and Data

ESA History

We summarize relevant aspects of the ESA. For more complete treatments, see Yaffee (1982), Shogren (1999) and SELS (2001). The 1973 ESA was an extension of the 1969 Endangered Species Conservation Act, which itself was an extension of the 1966 Endangered Species Preservation Act. The latter was the first comprehensive federal attempt to address species extinction. The 1966 Act directed the Secretary of Interior to determine which vertebrates were threatened with extinction. Thus some species listed under the 1973 ESA were identified as needing protection in 1967. However, the 1966 Act did not result in substantial actions to address species loss. Moreover, although the Act directed the Secretary to seek scientific advice, it did not require the use of particular criteria in making the listing decision.

The 1973 ESA offered protection to plants as well as animals, authorized protection of species that had not yet reached the very brink of extinction, added prohibitions on private actions harmful to listed species, and subjected all federal agencies to an obligation not to jeopardize the continued existence of listed species (see Doremus for statute section references). Two important sections of the ESA are Section 7 and Section 9. Under section 7, federal agencies must insure that actions they take, fund or authorize are not likely to jeopardize the continued existence of any listed species. Section 9 prohibits commerce in and the "taking" of endangered animal species. The prohibition on taking includes killing or capturing the animal, as well as harm caused by substantial habitat modification. The ESA provides no protection to species that are not officially listed under the ESA.

In the late 1970s, a debate about ESA listing practices developed in Washington (see Doremus). Many believed that listing decisions were often driven more by politics and preferences than by science. There was an official policy to favor "higher" animals in the following order: mammal, bird, fish, reptile, and amphibian (Metrick and Weitzman). The 1979 amendments to the ESA included an explicit requirement that the FWS perform a formal status review to determine whether sufficient "scientific and biological data" existed to justify a listing

proposal. Congress directed the FWS to communicate with "experts in the field" as part of the status review process.

In the 1982 Amendments, Congress required that listing decisions be made "solely" on the basis of the best available scientific and commercial information. Its intention was that listing decisions would be made based on scientific criteria without reference to the taxonomic preferences of FWS staff and the public, without pressure from politicians, and without consideration of economic costs (see discussion in Brown and Shogren, and Doremus). In the ESA listing context, science is synonymous with biology. Doremus (p.#) notes that, "Congress repeatedly equated the two" and that "a listing petition need only present 'biological information.'"

In response to the 1982 amendments, the FWS created formal guidelines to guide its expenditure decisions. The FWS created a "priority system" index, which took into account the degree of threat to the species, the size of the genus to which it belonged (i.e., its contribution to diversity), and its potential for recovery. Note that this index was used to guide expenditure decisions (Brown and Shogren), not listing decisions, but it is likely that the first two components were important factors in listing decisions.

Although Congress made clear that listing decisions should be based on biological considerations alone, a survey of employee preferences in the FWS Office of Endangered Species (Brown 1990, cited by Brown and Shogren: 8) found that employees ranked mammals and birds above fish, amphibians and reptiles. Likewise, in a 1996 study of vertebrate listings since 1973, Metrick and Weitzman found larger animals from higher forms of life (mammal, birds) were more likely to be listed. They note that larger species and higher life forms are likely to be "charismatic," and thus enjoy stronger political support than others. With regard to FWS's ability to ignore political influence, one may believe it unlikely that such influence can be avoided given that the senior staff in the Department of the Interior is appointed, and Congress controls the Department's budget.

Importantly, these factors that affect listing decisions (taxonomy, size, level of endangerment, politics, scientific knowledge and advocacy) are also likely to affect the ability of a species to recover. For example, larger animals and higher taxonomic classes tend to require larger habitat sizes and reproduce more slowly. They are also more likely to be seen as "worthy" of sacrifices by the general population to secure their recovery. Thus the construction of a

counterfactual in the evaluation of the ESA must take into account selection on these observable characteristics.

Data

Outcome. The outcome variable is “change in endangerment status from 1993 to 2004.” We define below what we mean by endangerment status. We choose to begin the period in 1993 for practical reasons. Before 1993, there were no objective measures of endangerment status for both listed and unlisted species of different taxonomic classes.

Treatment. We are interested in estimating the effect of “being listed under the ESA between 1973 and 1993.” As is the practice in empirical analyses of the ESA, we do not distinguish between species listed as “endangered” (in danger of extinction throughout all of a significant portion of its range) or as “threatened” (likely to become endangered in the foreseeable future). The words “endangered” and “threatened” are not precise scientific terms with generally accepted biological meaning, and in practice, both types of listed species are afforded the same protection under the ESA. We also consider a treatment that consists of being listed under the ESA and receiving “substantial” federal funds for recovery efforts (see *Funding* below).

Sample. We limit our study to endangered U.S. terrestrial and freshwater vertebrates that have full species status. We exclude listed species located outside of the 50 United States because (1) the FWS has little or no control over the protection of these species and (2) creating a counterfactual for these species is more difficult given data limitations in these foreign locations. We exclude plants because data on their endangerment status over time is not available for a large set of species. We ignore marine mammals because many are listed and managed by the National Marine Fisheries Service, not the FWS. We ignore subspecies for three reasons: (1) an exhaustive list of subspecies in the United States does not exist; (2) our biological database (Nature Serve), which is the only exhaustive database of all U.S. vertebrate full species, only tracks “selected” unlisted sub-species; and (3) the concept of a “subspecies” is controversial in the biological community. In the FWS literature, full species are supposed to be afforded higher priority than subspecies. We also exclude exotic species because their protection is not a

conservation objective, and we drop species that were listed after 1993 because we wish to construct a counterfactual using only species never listed between 1993 and 2004. Our sample consists of 136 listed species and 295 unlisted species.

Covariates.

Taxonomy and Size: We include the variables that Metrick and Weitzman include: taxonomic class (bird, amphibian, mammal, fish, and reptile), which captures human affinity for species that are more closely related to humans and important biological characteristics such as reproductive capacity and habitat requirements; length, which, along with taxonomic class, is a proxy for “charismatic megafauna” as well as capturing important biological characteristics such as metabolism and habitat requirements; and taxonomic distinctiveness (monotypic or from a small genus with 2-5 species), which captures the species value-added to biodiversity. These covariates plausibly have an effect on both listing and recovery. We obtain these measures from Nature Serve’s Explorer database (<http://www.natureserve.org/explorer/>)

Endangerment Status. The more endangered a species, the more likely the FWS will list it. Moreover, the more endangered a species is, the more difficult it is for the species to recover, and, for species on the brink of extinction, the more difficult it is for the species to decline any further. We use the same measure of endangerment that Metrick and Weitzman used: national endangerment scores from Nature Serve, which tracks all native vertebrates in the United States (the system was originally started in the early 1990s by The Nature Conservancy). Based on the Natural Heritage Methodology (<http://www.natureserve.org/prodServices/heritagemethodology.jsp>), Nature Serve’s system assigns an endangerment score to each species on a scale of 0 (extinct) to 5 (least endangered).

The Nature Serve scoring system is the most comprehensive measure of species endangerment for the set of listed and unlisted vertebrates. Each of the scores has a well-defined meaning and a serious effort is made to apply the scores consistently. A score of 1 implies that the species is “critically imperiled” in its range, having fewer than 6 occurrences in the world, or fewer than 1,000 individuals. A score of 2 implies that the species is “imperiled” in its range, having between 6 and 20 occurrences, or fewer than 3,000 individuals. A score of 3 implies the species is “vulnerable” in range, have fewer than 100 occurrences, or fewer than 10,000 individuals. A score of 4 implies the species is “apparently secure” throughout range (but

possibly rare in parts of its range). A score of 5 implies the species is “demonstrably secure” throughout range (however, it may be rare in certain areas). When a species falls between two scores, it is given an average value (e.g., 2/3 implies 2.5). Species that are reported to still exist but lacking persuasive documentation, or species that have not been observed in some time but have the potential to still exist, receive a score of 0.5. Species that are presumed extinct receive a score of 0. As noted by Metrick and Weitzman, the Nature Serve system is similar to the “degree of threat” measure in the FWS’s priority scoring system: both attempt to measure the absolute endangerment level of the species. However, unlike the Nature Serve system, no specific standards have been published by the FWS to explain why different species are assigned different degrees of threat.

The earliest year in which Nature Serve scores were recorded is 1993. We obtain these scores from the Database on the Economics and Management of Endangered Species (DEMES; Cash et al. 1998), which was an updated version of a database used by Metrick and Weitzman. This measure is the limiting variable in our data set. Not all vertebrates received scores in 1993. We include only species that were “endangered” in 1993: those with scores between 1 and 3.5. We remove from the data any species that were extinct or potentially extinct in 1993 (scores of 0 or 0.5) because it would be difficult, to say the least, for such species to show any change between 1993 and 2004.

Science: The FWS claims to base listing decisions on available scientific evidence. If the science does not warrant listing (either because the science indicates the species is not imperiled or because sufficient data are lacking), the FWS will not propose a species for listing in the Federal Register. Doremus notes that relying on scientific information has been, to different degrees, an important part of the ESA from its birth. Scientific information can affect listing decisions through its direct effect on the FWS, but also indirectly through conversations that the FWS has with scientists who might be interested in seeing “their” species listed (e.g., in the listing decision of the California gnatcatcher, the FWS received criticism for relying on published taxonomic evidence from a scientist who had been one of the petitioners requesting the FWS list the species; reference). Obviously scientific information will also influence the success of a species recovery: the more well understood the species, the more likely the species can be successfully recovered.

We use the annual number of journal articles as a measure of scientific influence on the listing and recovery processes. The number of such articles is not a perfect measure of scientific (and scientist) influence on the processes of listing and recovery. However, Doremus notes that although the FWS is allowed to consult unpublished reports and first-hand observations, it tends to be reluctant to do so. For example, in announcing its 1994 decision to withdraw the proposed listing of the Alabama sturgeon, FWS rejected the use of anecdotal evidence altogether and appeared to renounce any reliance on data not yet published in the peer-reviewed literature. For every species in our database, we used BIOSIS Previews to record the annual number of citations to that species from 1969 to 1993. In our analysis, we use the average annual citations to a species as a measure of scientific influence.

Politics: Interference in the listing and recovery process by federal legislators is commonly assumed, although with the exception of research on the listing process by Ando (1999), data for such interference is lacking. Pro-environment politicians may be more active in seeking, or less active in preventing, the listing of species in their states. Pro-environment politicians may also reflect pro-environmental preferences of their constituents which may make the FWS more inclined to list a species from the state (because there will be less resistance) or less inclined to list the species (because the citizens and politicians have or will take action themselves and the species is less likely to need federal protection). Opposite effects would stem from pressure by pro-land-use politicians and their constituents. Clearly, political influence can affect not only listing, but species recovery as well.

To measure the environmental preferences of federal legislators, Ando used League of Conservation Voter (LCV) scores for Congressional representatives in the late 1980s. We follow her example and collected LCV scores for every House and Senate delegation back to the first year the League published their scorecard (1971). These data were derived from on-line and hard copy content from the League. We construct two measures of environmental preferences. For both the Senate and House delegations, we estimate the average annual LCV score between 1971 and 1993. When a species is found in more than one state, we take an average of the annual scores across states (we do not have precise enough data on the proportions of a species' habitat in each state, and thus weigh each state equally).

Every Senate delegation has two members, but the number of members of each House delegation varies by the size of the state. Presumably the total numbers of pro-environment and

pro-land-use House representatives can matter in listing and recovery outcomes. Using Ando's score cutoffs for designating a representative as "pro-environment" (score>75) or "pro-land-use" (score<25), we estimate the average annual number of pro-environment and pro-land-use congressional representatives that have influence over a given species' habitat.

If one believed that Congressional and Senate delegations did not necessarily reflect the preferences of their constituents, a measure of citizen environmental preferences should be sought. As a measure of the strength of citizen environmental concerns, we use results from a 1991 FWS survey that includes estimates of the number of citizens of each state that recreationally observe wildlife in a non-hunting context. In the end, we find that these measures of citizen preferences do not add any explanatory power after the LCV scores are in the propensity score equation (suggesting that federal representatives reflect their constituents' preferences) and thus we drop this variable from the analysis.

We also collected data on Federal and State land ownership by state (using data assembled by the Wilderness Society: <http://www.nwi.org/Maps/LandChart.html>) and data on the comprehensive of state Endangered Species laws (from <http://www.defenders.org/pubs/sesa01.html>). However, we find that these variables were not important factors in any of our selection models and thus we exclude them in the analysis.

Funding: Some have argued that the problem with the ESA is that few species get more than their names in the Federal Register and a nominal amount of money for recovery efforts (references). Thus, the argument goes, one should not be surprised that the ESA is not able to stem the decline of endangered species; money must accompany listing for there to be an effect. In 1989, Congress began requiring FWS to conduct an annual accounting of "reasonably identifiable" expenditures associated with ESA (1989-1993 expenditures are in the DEMES database). Thus we define another treatment variable to evaluate the ESA: "being listed under the ESA between 1973 and 1993 *and* receiving a substantial amount of recovery funds between 1989 and 1993." We define "substantial" to be greater than \$750,000 over the four-year period. As with the listing decision, there are likely to be selection issues associated with funding decisions. For example, species with the highest spending from 1989 to 1993 include many "charismatic" species, such as the red-cockaded woodpecker and the bald eagle.

IV. Analysis

Table 1 in the Appendix shows how the distribution of 1993 Nature Serve scores differs across treatment status. It is clear by inspection that, despite the common support, the listed species within the sample are more likely to be endangered than are the unlisted.

The first question that we must address in an analysis of a binary treatment is the nature of the selection process itself. Figure 1 plots the average 1993 endangerment score of the species listed in each year over time, and fits a smoothing kernel weighted by the number of observations in each year over the data. We see that the earliest cohorts are more endangered than those selected later. Interestingly, the only year in which selected species were more endangered than in the original cohort was 1983, the year immediately after the passage of the FWS guidelines.

Given that our measure of endangerment is observed in 1993, one interpretation of this relationship is that the ESA has been directly detrimental to the outcomes, and so those listed first are now worst off. However, this explanation seems unlikely, particularly given the early emphasis on protecting species that were on the brink of extinction. Under the alternative explanation that the sequencing of protection has been non-randomly distributed across initial listing status, it is difficult to give any causal interpretation to differences in levels of endangerment across cohorts. This implies that, given any correlation between endangerment and recovery (which we demonstrate in the next section does exist), analyses that identify treatment effects off of the length of time listed will be biased.

Figure 1 and the *History* section above give us strong reasons to think that the determinants of the selection process have changed over time. Because the date listed is not observed in the control species, analysis of changes in the listing determinants over time is not easily performed through interactions. Instead, we divide the enlistees under the ESA into four roughly equally-sized cohorts. The cohorts, designated by the years of listing, are 1967, 1968-1982, and the post-FWS guideline cohorts of 1983-1988 and 1989-1993. If these guidelines were successful in de-politicizing the process, we should see political variables becoming less important over time. In each cohort, our outcome variable is defined as 0 for all controls, and as 1 for the species that are listed in that cohort. Species that have already been listed are eliminated from subsequent analyses, decreasing the numbers of observations for each regression.

The explanatory variables are dummies for each taxonomic class, dummies for being from a very small genus and for being a monotype, and the log length of each species. Our vector of political variables includes the average (up to the end of each cohort) of LCV scores for house and senate representation, and the average annual number of pro-environment and pro-land-use congressional representatives (see section III, *Politics*). The vector of scientific controls is the average number of journal citations in the years up to the end of the cohort, and the 1993 Nature Serve score. We also define a set of ‘charisma’ variables that are the dummies for being a mammal or bird, and the log length of the species. Reported in Table 2 in the Appendix are F-statistics from linear probability regressions with robust standard errors run on each cohort of the hypothesis that the political, scientific and charisma controls are each jointly equal to zero.

While the scientific variables remain strongly significant in every cohort, the influence of the political variables diminishes over time and is entirely absent in the selection process by 1983. The charisma variables, while seeing a brief resurgence in the 1983-89 cohort, also become insignificant in the final listed cohort. From this we can conclude that the FWS guidelines have been largely successful, as their introduction coincides perfectly with the point at which politics ceases to influence listings under the ESA, and the process has become less charisma-driven in the latest cohort.

Next, we proceed to an examination of summary statistics that show how outcomes have differed across listing status and taxonomy. Figure 2 shows the distribution of outcomes among listed and unlisted species across the five major taxonomic classes in the data. An outcome of -1 implies worsening over the study period, and 1 a rebounding species. No unlisted mammals rebound, and outcomes among listed species are better for birds and mammals, while being worse for fish.

Figure 3 shows how federal expenditures in our sample are apportioned across different taxonomic classes. Birds alone receive almost 75% of the expenditures, with an average spending per species (listed and unlisted) of \$4,072,985, followed by mammals at \$1,431,805, reptiles at \$539,898, fish at \$245,549, and amphibians at \$20,189. Spending on mammals and fish is concentrated on the most endangered species, while the most endangered fish and reptiles receive less funding.

V. Regression Estimation

We begin our analysis of treatment effects with some simple regressions that illustrate how listing and funding are related to the changes in endangerment that occur over the period 1993-2004. We consider two binary treatment variables. The first is being listed under the ESA (*listed*), and the second is receiving more than \$750,000 in recovery funding (*funded*). No unlisted species received this level of funding, and so the species receiving high funding are a strict subset of those listed under the ESA.

Tables 3 and 4 present correlations between these treatments and changes in outcomes using various control structures. Estimated with 431 observations using OLS with robust standard errors, these regressions interact the treatment with the 1993 Nature Serve score to show how changes were differentiated across treatment/control status. In all regressions the 1993 score is demeaned in the interaction, so the interpretation is as follows: the coefficient on the uninteracted treatment variables gives correlations with outcomes at the mean initial score; the 1993 score coefficient is measured only in the control; and the interaction shows how outcomes in treatment and control vary across initial status. The coefficient on the 1993 score shows that, in the control group, the most endangered species rebounded most on average.

The tables illustrate that the correlations between listing status and outcomes are sensitive to specification. When the regression is estimated using no controls except 1993 scores, listed species appear to be weakly negatively worse off than unlisted species (Table 3), and funded species appear weakly better off (Table 4). The biological controls include dummy variables for taxonomic class and for being a carnivore, the length of the species, and a dummy for a species from a very small genus.

Introducing biological controls makes listed species appear relatively better off. The implication of this relationship is that taxonomic classes more likely to be listed are also more likely to see declining outcomes. While the effect of listing does not appear to be strongly differentiated according to initial endangerment status, less endangered species appear to fare better under funding.

Given the evidence in the previous section of the importance of political and scientific factors in determining selection decisions, we must control for these factors as well. To this end, we include the average LCV scores for the House and Senate delegations for the period 1971-1993, as well as the percentage of terms in which that score was above or below the Ando

cutoffs. To control for differences in scientific attention to species, we include the average number of citations per year from 1969-1993 in the scientific literature. The additional battery of controls drives down the sign of both treatments, making listing significantly negative and funding insignificant. The implication is that species that are similar in terms of other measures of human involvement have higher counterfactual outcomes, and thus the specific policy interventions analyzed here have weaker apparent effects. The upshot is that listing appears now to have a significant *negative* effect while the positive impact of spending on species recovery is insignificant.

The fact that the earliest listing score we observe comes from 1993 raises a concern. The 1993 score represents a proper pre-treatment outcome for the last-listed species. For those listed earlier, however, this score contains some component of a treatment effect. There is no obvious way to deal with this problem without having a consistent estimate of the treatment effect, and so in the fourth column we re-estimate these regressions excluding the 1993 score and interactions, and we see the coefficient on both treatments move towards zero. Funding remains positive and insignificant, and the effect of listing is now negative and insignificant. The more positive treatment effect of *listed* implies that the counterfactual estimated without the 1993 score is actually *more* endangered. Put differently, it is the failure of highly endangered listed species to bounce back that generates the negative correlation on listing in column 3. So when we omit 1993 status, treated outcomes appear better because they are compared with all untreated units rather than only the highly endangered ones.

Tables 3 and 4 demonstrate outcomes among listed species that are highly differentiated across funding levels, and also indicate negative outcomes associated with listing in the absence of funding. Table 5 re-estimates the same regression as in Table 3, but excludes the 42 listed species that receive high funding, thus comparing listed and low- or non-funded species with unlisted species. Here the effects are dramatic and robust to specification; protection under the ESA without significant financial commitment is associated with markedly worse species recovery. The effect changes little as the biological, political, and scientific controls are added in, but weakens when we omit the 1993 score. While the estimate of the effect on '93-04 status changes falls from -0.53 to -0.17, it remains statistically significant.

Table 6 conducts an analysis of the continuous variable that describes funding per species only within the 136 listed species in the data. While the correlations are very strong and positive

using no controls or only biological controls, they fall out once we control for other proxies of the degree of human protection of the species. So, using only the biological controls, we see that an increase of \$100,000 in funding is associated with a one and a half point increase in outcomes relative to the counterfactual (a listed species with no money), but including political variables and citations knocks this effect out. In particular, citations are strongly correlated with funding among treated species, at 0.58.

Looking only at Table 6, we would be tempted to conclude that funding is not an independently influential determinant of success, but rather a proxy for a group of underlying variables that reflect political and scientific concern. However Table 5 makes it quite clear that the difference between unfunded listed species and unlisted species is not contingent upon the degree of political and scientific concern. From this analysis we conclude that while funding decisions can largely be explained by other covariates, unfunded listed species fare significantly worse than the controls, and species that are both listed and funded fare significantly better.

Several important objections can be raised to the methodologies used for this preliminary set of regressions. First, estimating differences in a single OLS regression conflates the selection and outcome equation, and thus imposes that the determinants and coefficients be the same for these two processes. Second, the regressions rely on the data generating process being linear in the parameters specified. Under any non-linearity in the joint selection and outcome equations, these equations are misspecified and thus inconsistent. Third, if the treated and control units do not share a common support, OLS will estimate marginal coefficients using control units which are not proper counterfactuals for the treatment.

To address these concerns, we switch to propensity-score matching in order to use flexible forms to find the highest-quality counterfactual for each treated unit, and to verify that we have a common support over all of the matching variables. We utilize several different matching methodologies and post-matching regression techniques, but the answers prove very robust to specification.

VI. Treatment Effect Estimation through Matching

We estimate the average treatment effect on the treated (ATT) by comparing the change in endangerment scores from 1993 to 2004 between listed and unlisted species using nearest-neighbor matching (with replacement) across the full vector of control characteristics in column

3 of Tables 3-6. Evidence from Monte Carlo simulations shows that the variance of the nearest-neighbor matching estimator falls very dramatically as we increase the number of matches above one. While a slight tradeoff in terms of match quality is implied, we follow the convention established by Abadie and Imbens (2004) and others of using four nearest neighbors to form our counterfactuals (see also McIntosh 2004).

Abadie and Imbens show that matching estimators in finite samples without exact matching (and two or more continuous covariates) include a conditional bias term. Thus we use their post-matching bias-correction procedure that removes the conditional bias asymptotically (in a footnote, we also report the results without the bias-correction procedure).

In estimating the standard errors of the treatment effect estimate, one cannot use the usual formula for the variance of a difference in means. This formula ignores the additional variability that arises from the estimation of the propensity scores, which can matter for samples of small or moderate size (Heckman et al. 1997). Most analyses in the 1990s and early 2000s used simple bootstrapping methods to estimate the standard errors of the treatment effect. Analysts now realize, however, that simple bootstrapping methods ignore the variance that arises from multiple matches to the same unit (Abadie and Imbens). We use Abadie and Imbens' variance formula for the nearest-neighbor ATT estimator. They also provide a robust estimator that allows for heteroskedasticity, which allows the treatment effect to be nonconstant (i.e., outcome variance differs by treatment status and covariates). This modification is implemented by using a second matching procedure that matches treated units to treated units and control units to control units. As with our estimation of the conditional mean functions, we pair observations to the four closest matches in our estimation of the conditional variance function.⁵

Table 7 presents the treatment effect estimates for the two treatments, *listed* and *funded*. The estimated treatment effect of listing alone is small, negative and not statistically different from zero. The estimated treatment effect of funding, however, is substantial, positive and significantly different from zero. Given our results from Table 5, we also estimate the treatment effect of listing in the absence of substantial funds, *Listed w/o funding* (i.e., the sample is species receiving fewer than \$750,000 in federal expenditures). The estimated treatment effect for listed species that receive little or no funding is substantial, negative and significantly different from

⁵ Matching was implemented with Stata code developed by Abadie et al. (2004).

zero.⁶ Thus the matching analysis is consistent with our analysis thus far: listing by itself has either no effect or a weakly negative effect on species recovery, and substantial federal funding for species recovery has a positive effect on species recovery.⁷ The third row in Table 7 suggests that the weakly negative estimated effect of listing may arise from a sample that combines listed species with little or no funding and species with substantial funding; listing a species but providing it with little funding for recovery efforts may, on average, be detrimental.

One potential criticism of the analysis thus far is the use of the Nature Serve scores as continuous variables. Although these scores represent underlying continuous variables (numbers of occurrences or individuals), one could argue that a more conservative approach would be to treat the scores as ordinal categories and consider changes in a species' status from 1993 to 2004 as represented by three ordinal outcomes: more endangered, unchanged, and less endangered.

A straightforward way of measuring the effect of listing and funding on these outcomes is to conduct nearest-neighbor matching using our vector of controls,⁸ and then to use a simple Pearson's chi-square test to examine the distribution of outcomes in the treatment and the counterfactual. Reported in Table 8 are the outcomes for the units given each treatment (listing and high funding) as well as counterfactual outcomes in the matched controls. Using this more flexible strategy, we find no significant difference between treatment and control outcomes for listed species, with a Pearson's Chi-squared statistic of only 0.99 (upper panel). Since there are more treated units becoming more endangered and fewer rebounding, this is consistent with the weak negative effects estimated above.⁹

A much more powerful effect is seen in the lower panel of Table 8, where we conduct the same exercise for the high funding treatment. Here the Pearson's statistic of 12.1 is highly significant, indicating markedly better outcomes among species that receive substantial funding. The distributions imply that, compared to the poorly-funded controls, well-funded species were more likely to become less endangered and less likely to become more endangered.

⁶ Without bias-adjustment: $ATT_{Listed} = -0.1330$ ($p=0.065$); $ATT_{Funded} = 0.2649$ ($p=0.010$); $ATT_{Listed\ w/o\ Funding} = -0.3311$ ($p<0.001$).

⁷ We tried a variety of matching estimators (e.g., kernel matching) and propensity score equations (e.g., other specifications in Tables 3-6), but the qualitative results were unchanged. If anything, the other estimators make the effect of listing by itself more negative and funding more positive. For example, with a biweight kernel matching estimator: $ATT(listed) = -0.04$; $ATT(funded) = 0.36$.

⁸ In the propensity score equation, we also use dummy variables for the scores rather than treat them as continuous.

⁹ Note also that a similar table with all unlisted species used as the counterfactual generates a test statistic of 12.75 ($p=0.002$), where the difference in the distributions is largely in the number of species that improve (listed: 9%; unlisted: 23%). Thus without matching, the effect of listing seems even more negative.

To conserve space, we do not present a table for the distributions of listed species without substantial funding and their matched counterfactuals, but we note the difference between the treatment and control outcomes is significant ($\text{Chi}(2) = 10.32$) and implies treated species are more likely to become more endangered and less likely to become less endangered.

To generate more precise estimates of the marginal effects of listing and high funding on the three potential outcomes, one can conduct nearest-neighbor matching using our vector of controls, and then estimate a weighted, ordered probit using the matched listed and unlisted species (weighted by the number of times a control is matched with treated units). The strategy has two disadvantages: (1) it imposes the parametric probit model assumptions on the data; and (2) the standard errors of the ordered probit are incorrect and must be modified to reflect the additional variability that arises from the matching process. We have not yet programmed an acceptable way to do this modification and thus the standard errors reported are not correct. However, if the probit model assumptions are valid, the treatment effect coefficients are unbiased.

With the ordered probit model, we estimate that listing: (1) increases the likelihood that a species becomes more endangered by 4.57% ($p=0.150$); and (2) decreases the likelihood that a species becomes less endangered by 2.98% ($p=0.154$). Note that these treatment effect estimates are consistent with the analyses performed above. Listing has a weakly negative effect on species recovery.

Using the same methods, we estimate that, consistent with the results above, listing without substantial federal funding: (1) increases the likelihood that a species becomes more endangered by 14.61% ($p=0.001$); and (2) decreases the likelihood that a species becomes less endangered by 8.17% ($p=0.003$).

In contrast, we estimate that listing with substantial federal funding: (1) decreases the likelihood that a species becomes more endangered by 20.98% ($p=0.01$); and (2) increases the likelihood that a species becomes less endangered by 9.67% ($p=0.057$). Again these treatment effect estimates are consistent with the analyses performed above: listing with funding has a salutary effect on species. The estimates also imply that much of this effect derives more from the prevention of listed species from becoming more endangered and less from making species less endangered.

VII. Discussion and Conclusion

The decision to protect or fund a species is contentious, involving complex tradeoffs of scientific, political and financial concerns. Further, the selection process has changed over time and is not subject to any rigorously observed guidelines. The preceding analysis has found that, controlling for selection bias through several different means, the impact of listing alone is weakly negative and the impact of listing together with substantial funding is positive. A skeptical reader could argue that either result taken on its own is merely a result of selection criteria for which we have failed to control. However, for us to be convinced that both results could arise as a result of selection bias it would have to be the case that the bias in the two decisions works in opposite directions. Specifically, for both of these results to arise from bias, it would have to be the case that the listing decision makers were selecting species that had poor chances of recovery, while funding decision makers were selecting the unobservably good recovery prospects.

In order to understand the degree of bias that may be present in these estimates, we need a clear understanding of how each decision is made. The listing process for the ESA prior to the 1982 FWS guidelines was ill-defined, but the guidelines restricted the allowable determinants of listing. According to Doremus (p.#), “Congress also expressly restricted the scope of listing decisions, requiring that they be made ‘solely’ on the basis of the best available scientific and commercial information. This change was made to ‘prevent non-biological considerations from affecting’ listing decisions. The primary “non-biological” considerations at issue were those included in the administration's economic impact analyses, that is, the economic costs of protecting species.” Thus, the listing process has followed rules which are not explicit but which seek to disallow all ‘non-scientific’ determinants.

Funding decisions from 1989-1993, on the other hand, follow a set of guidelines that are both explicit and include economic costs. Congress requires that expenditures on species should vary with a priority system in which ‘degree of threat,’ ‘recovery potential,’ ‘taxonomy,’ and ‘conflict with development’ are ordered into an 18-point scale (Brown and Shogren). Since recovery potential is included in this metric, we should be concerned that some bias may exist across this selection criterion. However, Metrick and Weitzman show that in reality one of the strongest determinants of funding decisions is whether the species is in conflict with development or other human activity. Those species that are in conflict have substantially higher

spending allotted to them, and this variable alone is a more significant determinant than the priority score that is supposed to be driving spending decisions. Since it seems very likely that, *ceteris paribus*, species facing conflict would be less likely to rebound, this result suggests that the selection bias present in spending decisions may in fact be negative, thus strengthening our finding of a positive impact on expenditures. However, given that the selection process for the two treatments is not identical, it is possible that the selection effects work in opposite directions.

Of the 42 species that received \$750,000 or more during the 1989-1993 interval, the mean 1993 score is 1.8 (i.e., they are highly endangered) and the mean years listed is 15.4 (i.e., they belong to the early, more endangered cohorts). Given a mean 1993 score among our control species of 2.5 (versus 1.57 among all listed species) it is difficult to tell any simple selection story which would result in the patterns in this data. It certainly seems possible that we may have incompletely controlled for the determinants of selection into the ESA, thereby biasing our treatment effects downwards. It is unlikely, however, that unobservable selection determinants into the highly funded group would not only reverse signs from the (ostensibly similar) listing decision, but would be strong enough to generate outcomes significantly higher than the control species, as well as other listed species.

[Rosenbaum bounds on effect of hidden bias]

In conclusion, we present strong evidence that the rancorous debate over listing more species under the ESA may be missing the point. Our analysis suggests that it is not listing which is effective, but rather a high level of expenditures for species recovery. Although we do not observe unlisted species receiving substantial funds, our results imply that listing in the absence of funding does not lead to species recovery. Consequently, our results suggest that resources expended in the long-running battles over listing under the ESA may be more effective if transferred directly into conservation funding.

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Appendix.

Table 1. 1993 Scores by ESA Listing Category

	<u>Unlisted</u>	<u>Listed</u>	<u>Total</u>
1	35	77	112
1.5	10	0	10
2	63	40	103
2.5	10	3	13
3	162	15	177
3.5	15	1	16
Total	295	136	431

Table 2. F-tests for Joint significance of Political and Scientific variables

(z-scores in parentheses).

<u>Cohort:</u>	<u># obs:</u>	<u>Political</u>	<u>Scientific</u>	<u>Charisma</u>
1967	427	3.4 (0.0092)	29.97 (0.0000)	13.6 (0.0000)
1968-82	386	1.99 (.0956)	18.04 (0.000)	1.54 (.2041)
1983-1989	350	1.28 (0.2780)	9.23 (0.0001)	3.79 (0.0107)
1989-1993	319	0..32 (0.8662)	15.72 (0.000)	0.81 (0.4902)

Political variables: LCV averages for house and senate,
and average of dummies for above 25/below 75.

Scientific variables: 1993 NS score and average # of citations.

Charisma variables: dummies for birds & mammals, log(length).

Table 3. Relationship of Listing to Changes in Endangerment Status, '93-'04.

	1. No controls	2. Biological Controls	3. All Controls	4. Without '93 Score
listed	-0.204 (-1.68)	-0.101 (-0.73)	-0.30 (-2.39)	-0.092 (-1.32)
93 score	-0.103 (-2.47)	-0.071 (-1.70)	-0.090 (-2.11)	
interaction	-0.032 (-0.26)	0.026 (0.20)	-0.166 (-1.37)	
mammal		-0.355 (-2.88)	-0.380 (-3.20)	-0.447 (-3.85)
bird		-0.129 (-1.14)	-0.230 (-1.82)	-0.254 (-2.09)
reptile		-0.261 (-2.07)	-0.188 (-1.51)	-0.260 (-2.18)
fish		0.023 (0.29)	0.017 (0.21)	-0.001 (-0.01)
log(length)		0.033 (0.83)	-0.035 (-0.97)	-0.027 (-0.76)
small genus		-0.043 (-0.50)	-0.013 (-0.15)	-0.031 (-0.37)
carnivore		-0.289 (-1.68)	-0.353 (-2.29)	-0.412 (-2.67)
monotype		0.026 (0.33)	0.050 (0.61)	0.076 (0.91)
pro-environment			-0.001 (-0.13)	0.000 (-0.04)
anti-environment			0.009 (1.73)	0.007 (1.42)
LCV senate			-0.001 (-0.35)	-0.002 (-0.48)
LCV house			0.004 (0.74)	0.005 (1.03)
citations			0.112 (2.27)	0.090 (1.94)
R-squared	0.021	0.069	0.162	0.136

(numbers in parentheses are t-statistics)

Table 4. Relationship of Funding to Changes in Endangerment Status, '93-'04.

	1. No controls	2. Biological Controls	3. All Controls	4. Without '93 Score
funded	0.218 (1.53)	0.468 (3.30)	0.174 (1.10)	0.156 (1.49)
93 score	-0.078 (-2.24)	-0.045 (-1.24)	-0.062 (-1.65)	
interaction	0.259 (2.00)	0.369 (2.77)	0.117 (0.82)	
mammal		-0.459 (-3.91)	-0.454 (-3.91)	-0.473 (-4.13)
bird		-0.271 (-2.57)	-0.309 (-2.53)	-0.303 (-2.54)
reptile		-0.315 (-2.67)	-0.275 (-2.32)	-0.274 (-2.36)
fish		-0.012 (-0.16)	-0.018 (-0.22)	-0.023 (-0.29)
log(length)		0.023 (0.57)	-0.020 (-0.50)	-0.033 (-0.91)
small genus		-0.091 (-1.09)	-0.051 (-0.62)	-0.052 (-0.64)
carnivore		-0.407 (-2.61)	-0.410 (-2.74)	-0.420 (-2.81)
monotype		0.048 (0.59)	0.051 (0.61)	0.063 (0.75)
pro-environment			-0.001 (-0.10)	0.000 (-0.03)
anti-environment			0.008 (1.70)	0.007 (1.53)
LCV senate			-0.001 (-0.20)	-0.001 (-0.29)
LCV house			0.003 (0.70)	0.004 (0.86)
citations			0.052 (0.99)	0.066 (1.42)
R-squared	0.02	0.104	0.142	0.136

(numbers in parentheses are t-statistics)

Table 5. Comparison of Unfunded Listed Species to Controls.

(Dependant variable: changes in endangerment status)

	1. No controls	2. Biological Controls	3. All Controls	4. Without '93 Score
listed	-0.596 (-4.85)	-0.546 (-3.78)	-0.534 (-3.70)	-0.174 (-2.24)
93 score	-0.103 (-2.47)	-0.054 (-1.30)	-0.059 (-1.40)	
interaction	-0.408 (-3.27)	-0.421 (-2.85)	-0.412 (-2.79)	
mammal		-0.423 (-3.37)	-0.407 (-3.31)	-0.479 (-4.02)
bird		-0.314 (-2.68)	-0.301 (-2.30)	-0.268 (-2.14)
reptile		-0.220 (-1.71)	-0.234 (-1.83)	-0.274 (-2.21)
fish		0.047 (0.58)	0.040 (0.47)	0.018 (0.22)
log(length)		-0.005 (-0.11)	-0.003 (-0.06)	-0.018 (-0.41)
small genus		-0.022 (-0.24)	-0.023 (-0.25)	-0.027 (-0.29)
carnivore		-0.310 (-1.65)	-0.239 (-1.23)	-0.343 (-1.94)
monotype		0.090 (1.09)	0.085 (1.00)	0.106 (1.25)
pro-environment			-0.001 (-0.15)	0.001 (0.13)
anti-environment			0.006 (1.15)	0.004 (0.76)
LCV senate			-0.001 (-0.17)	-0.001 (-0.33)
LCV house			0.002 (0.49)	0.004 (0.85)
citations			-0.130 (-1.03)	-0.130 (-1.00)
R-squared	0.084	0.17	0.179	0.13

(numbers in parentheses are t-statistics)

Table 6. Effects of Funding Among Listed Species.

	2.			
	1. No controls	Biological Controls	3. All Controls	4. Without '93 Score
funding (continuous)	1.569 (5.09)	1.515 (4.01)	-0.248 (-0.54)	0.034 (0.07)
93 score	-0.225 (-1.79)	-0.230 (-1.75)	-0.392 (-3.22)	
interaction	1.089 (1.54)	1.615 (1.94)	-0.563 (-0.63)	
mammal		-0.026 (-0.11)	-0.214 (-1.08)	-0.453 (-2.42)
bird		-0.028 (-0.24)	-0.063 (-0.33)	-0.263 (-1.82)
reptile		0.091 (0.57)	0.218 (1.17)	-0.070 (-0.59)
fish		-0.082 (-1.07)	-0.084 (-0.75)	-0.128 (-1.43)
log(length)		0.078 (1.05)	-0.053 (-0.83)	0.018 (0.30)
small genus		0.040 (0.25)	0.145 (0.92)	0.086 (0.53)
carnivore		-0.437 (-1.59)	-0.234 (-0.94)	-0.457 (-1.86)
monotype		-0.034 (-0.36)	0.063 (0.68)	0.093 (0.86)
pro-environment			-0.012 (-1.04)	-0.007 (-0.65)
anti-environment			0.024 (1.83)	0.017 (1.36)
LCV senate			0.001 (0.15)	-0.003 (-0.44)
LCV house			0.002 (0.18)	0.009 (1.07)
citations			0.121 (2.09)	0.071 (1.04)
R-squared (numbers in parentheses are t-statistics)	0.111	0.149	0.38	0.245

Table 7. Treatment Effect Estimates

<u>Treatment</u>	<u>ATT</u>	<u>SE</u>	<u>Z</u>	<u>P> z </u>	<u>[95% CI]</u>
<i>Listed</i>	-0.0168	0.0721	-0.23	0.816	[-0.158, 0.125]
<i>Funded</i>	0.2429	0.1035	2.35	0.019	[0.040, 0.446]
<i>Listed w/o Funding</i>	-0.2189	0.0807	-2.71	0.007	[-0.377, -0.061]

Table 8. Comparison of Treated Outcomes to Matched Counterfactuals.

Treatment: Listing.	<u>Unlisted</u>	<u>Listed</u>	<u>Total</u>
<u>Species Outcome:</u>			
More endangered	22	24	46
Unchanged	97	100	197
Less Endangered	17	12	29
Total	136	136	272

Pearson chi(2) = 0.9947
Pr = 0.608

Treatment: High Funding	<u>No/Low Funding</u>	<u>High Funding</u>	<u>Total</u>
<u>Species Outcome:</u>			
More endangered	15	4	19
Unchanged	26	29	55
Less Endangered	1	9	10
Total	42	42	84

Pearson chi(2) = 12.9321
Pr = 0.002

Figure 1. Changes in Endangerment Status across Year Listed.

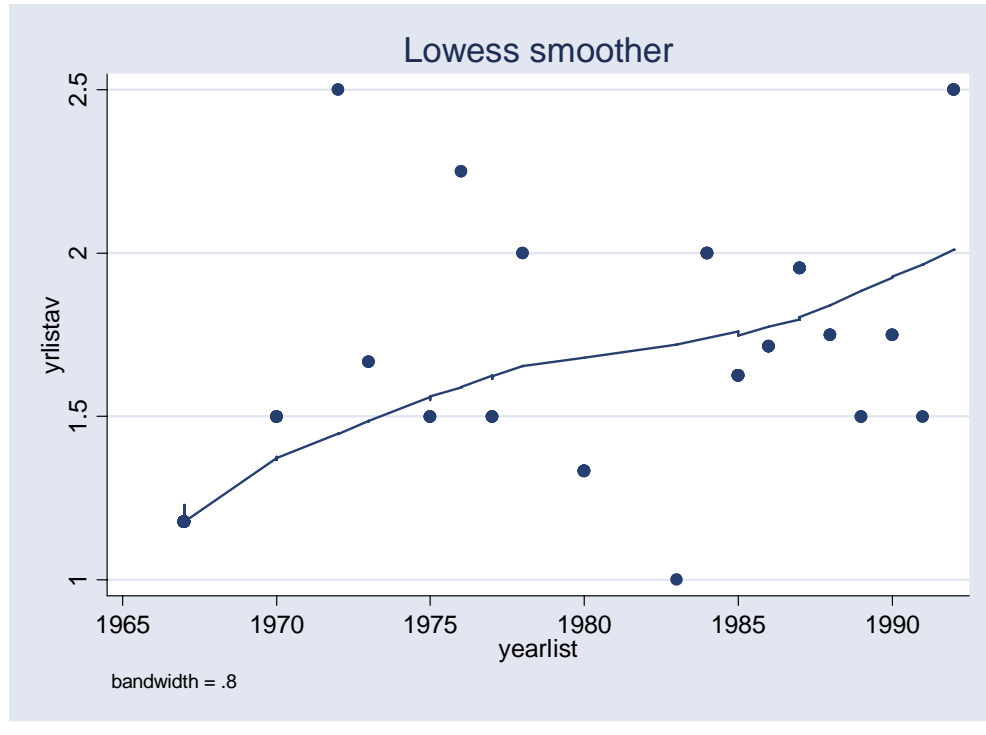


Figure 2.

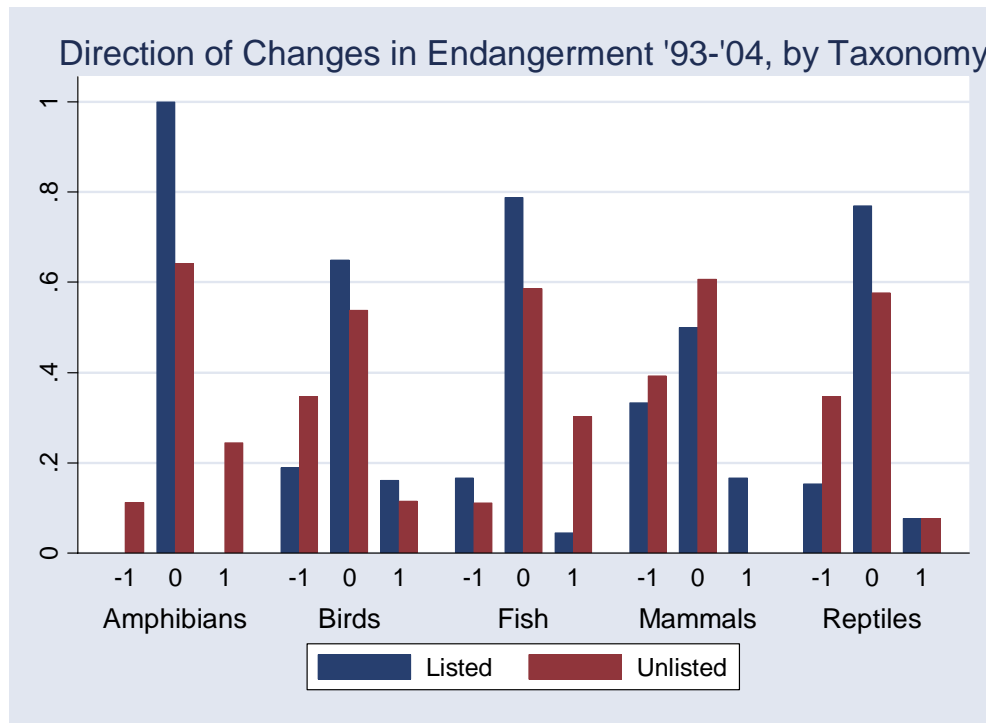


Figure 3.

