International trade and the survival of mammalian and reptilian species

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The Convention on International Trade in Endangered Species (CITES) bans international trade in species threatened with extinction. We investigate the effects of these bans on species’ endangerment, as assessed by the International Union for Conservation of Nature (IUCN). Our analysis exploits changes in CITES bans between 1979 and 2017. We find that CITES bans lead to subsequent improvements in mammalian species’ IUCN status, relative to species in which trade was not banned. These effects are primarily due to improvements in the status of commercially targeted species. On the other hand, CITES bans lead to deteriorations in reptilian species’ IUCN status. We find that major spikes in trade volume occurred in anticipation of the bans on reptilian species but not in anticipation of those on mammalian species.

INTRODUCTION

More than 30,000 known species are currently threatened with extinction, and the resulting loss in biological diversity can have severe effects on ecosystems and on the value humans and other species derive from them (1, 2). Many species are threatened because dead specimens (or their parts) have a high value when traded in markets (3). To counter this threat, the 1975 Convention on International Trade in Endangered Species (CITES) treaty regulates international trade in certain animal and plant species. The highest level protection under the CITES treaty applies to species listed in Appendix I of the agreement, which CITES defines as “species threatened with extinction.” With limited exceptions, states that are party to CITES can neither export nor import, for commercial purposes, live or dead specimens of any species listed in Appendix I.

As a theoretical matter, trade restrictions can have positive and negative effects on species survival. First, trade can protect a species if it encourages sound resource management practices: Commercial fisheries are the textbook example of how the profit motive can aid in sustaining species populations. However, this conclusion does not extend to environments with incomplete property rights, resulting in the well-known “tragedy of the commons” (4), in which case trade restrictions may be needed to prevent unsustainable overharvesting. Second, bans on international trade do not restrict subnational trade. From the perspective of the supplying economy, an international trading ban is an export restriction that reduces domestic prices. Producers in the domestic market may respond to this price reduction not only by shifting to other more profitable economic activities (a substitution effect) but also by increasing the volume of harvests to compensate for lower prices per specimen (an income effect). The overall effect of international trade restrictions depends on the balance of these supply responses (5). Third, illegal export markets may partially or entirely replace previously legal trade, reducing the effectiveness of the trade ban (6–9). Fourth, producers that anticipate an impending trade ban may increase harvests while trade is still legal (10), counteracting the intended effect of the ban.

Here, we investigate empirically whether CITES trading bans promote the survival of endangered animal species. To do so, we exploit changes in the set of species listed in CITES Appendix I over time. Key variation comes from 41 mammalian and 20 reptilian species that were added to Appendix I since 1979 and whose endangerment level has been assessed by the International Union on Conservation of Nature (IUCN) in one or more years between 1982 and 2018. We use difference-in-differences (DD), triple-difference (DDD), and ordered probit regression methods to examine whether, and how, being listed in CITES Appendix I has affected species’ subsequent endangerment. Thirty-one of the 41 newly listed mammalian species and 11 of the 20 newly listed reptilian species were previously in CITES Appendix II and, hence, already subject to certain international trade controls. CITES Appendix II includes species “in which trade must be controlled in order to avoid utilization incompatible with their survival.” Commercial trade in these species is subject to regulations intended to ensure that transactions are not detrimental to the survival of the species and minimize the risk of injury or cruel treatment of the specimen. Thus, estimated effects should be interpreted as changes relative to a counterfactual scenario in which the majority of affected species would have enjoyed CITES Appendix II protections had they not been listed in Appendix I.

For mammalian species, we find a statistically significant improving effect of CITES trading bans. When controlling for relevant economic factors (e.g., economic activity in the countries of the species’ distribution), inclusion in Appendix I is associated with an average improvement of species’ status of 0.15 to 0.52 points on IUCN’s seven-point threat assessment scale (depending on the regression specification), relative to the counterfactual. This improvement is generated by a shifting of probability mass from higher threat levels to lower threat levels on IUCN’s scale. Specifically, a CITES Appendix I listing reduces the probability that a mammalian species is assessed as “endangered” or worse and increases the probability that it is assessed as “vulnerable” or better. Consistent with the hypothesis that commercial trade threatens many species, we show that the improving effect of CITES trade bans is primarily driven by species that are the targets of commercial harvesting. Our estimates remain robust when the sample is restricted to species

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likely of particular concern in our context, such as species that received CITES trade bans and species that have been tracked by IUCN the longest. Furthermore, CITES trade bans appear particularly effective when they are not affected by Appendix I reservations, which are legal exceptions to the bans. We interpret the entirety of these results as constituting strong evidence that CITES international trade bans have been effective in promoting the survival of mammalian species.

For reptilian species, on the other hand, the results are reversed: We find a statistically significant worsening effect of CITES trade bans. When controlling for relevant economic factors, an Appendix I listing is associated with a deterioration of reptilian species’ IUCN status of 0.74 to 1.32 points (depending on the regression specification). This worsening effect is generated by a shifting of probability mass from lower threat levels to higher threat levels on IUCN’s scale and is primarily driven by the effect of the trade ban on commercially targeted species. Therefore, the CITES trading bans appear to have hurt reptilian species and have hurt those species most that are targets of commercial trade.

Why has CITES been effective in protecting mammalian species but has had the opposite effect on reptilian species? An earlier study of the effect of CITES trade bans on trade volume (10) found that imports of 46 animal species increased after the announcement of CITES trading bans but declined after the bans had become effective. Inspection of published CITES data on legal trade activity in Appendix I species confirms that a number of reptilian species in our dataset have seen sharp spikes in trade activity in the years leading up to a ban. Thus, one potential unintended effect of a CITES Appendix I listing is that it can spur a “last minute rush” in trade activity in anticipation of the trade ban. Rivalan et al. (10) conjectured that the harm caused by such rushes may outweigh the bans’ benefits. Our results support this conjecture for reptilian species. On the other hand, the CITES trade data contain no evidence of trade spikes before bans in the trade of mammalian species, which can explain why the CITES bans have been effective for mammals. (We will discuss possible reasons for the concentration of trade spikes in reptiles, as well as potential policy implications of our findings, after we present our empirical results.)

Several previous, small-sample studies concluded that CITES regulations had no measurable effect on endangerment [see (11) for a survey]. In a study of 12 animal species, the status of only two improved after being listed in Appendix I (12). Reviewing the available evidence in 2000, Martin (11) concluded that “[t]he direct evidence for whether CITES has been successful is, at best, inconclusive.” Two decades later, this sentiment does not appear to have changed. In a recent Science article, Frank and Wilcove (13) wrote that “[t]he overall effectiveness of CITES at protecting species from international trade remains an open empirical question.”

Our data, methods, and results improve over those in previous studies in three important ways. First, our panel spans a period of nearly four decades and includes several thousand mammalian and reptilian species for which IUCN threat level assessments are available. Although a much smaller subset of these species were included in CITES Appendix I listing during the study period, our dataset is the most comprehensive database that has, to our knowledge, been used to assess the effectiveness of CITES. Second, the appropriate statistical methods to study the question of whether CITES is effective must involve not only comparisons of the outcomes of Appendix I species from before and after the trade ban but also comparisons to species whose Appendix I listing did not change at the same time. The reason is simple. A species whose IUCN status fails to improve after a trade ban does not constitute evidence that the trade ban did not work. If the same species had seen its endangerment level worsen (or worsen more) without the trade ban, then the ban is clearly effective. The DD estimates presented here are based on this type of comparison. Third, we demonstrate that the effect of CITES international trade bans has not been uniform across animal classes. Our results show that the CITES bans have been effective in promoting the survival of mammalian species but have hurt reptilian species. These effects are consistent with the observed trade volume patterns in years leading up to the respective bans. We conclude by discussing the implications of our findings for future CITES trading bans.

RESULTS AND DISCUSSION

Our objective is to determine whether CITES international trade bans affect the endangerment of animal species, and, if so, how. We focus on the set of mammalian and reptilian species for which IUCN has reported an endangerment status in one or more years between 1982 and 2018. For each species, we coded the last available IUCN Red List status classification into a numerical variable as follows

\[
\text{Species status} = \begin{cases}
6 & \text{if species is extinct}, \\
5 & \text{if species is extinct in the wild}, \\
4 & \text{if species is critically endangered}, \\
3 & \text{if species is endangered}, \\
2 & \text{if species is vulnerable}, \\
1 & \text{if species is near threatened}, \\
0 & \text{if species is of least concern}
\end{cases}
\]

For each species, we then recorded whether a CITES trading ban was in effect in a given year. The resulting dataset contains 159,240 observations at the species-year level, covering a total of 5687 mammalian species and 6084 reptilian species. Summary statistics for this dataset are shown in Table 1.

Crucial for the identification of the effect of international trade bans on endangerment status is the presence of species subject to a trade ban in some years but not in others, as these species contribute the treatment variation necessary for our empirical framework. Since we consider time lags of up to 5 years in our models, a species contributes treatment variation if its CITES Appendix I listing changed in some year between 1978 and 2018 and if it has an IUCN status no later than 4 years after the change. A total of 41 mammalian species and 20 reptilian species fulfill these criteria, with the treatment being in the form of an addition to Appendix I in each case. These species are listed in Tables 2 and 3, respectively, and we refer to them as the set of treated species. (An additional mammalian species, *Pseudomys fumeus*, was delisted from Appendix I in 1987 before receiving an IUCN status of “near threatened” in 1990. As this is the only species with a “reverse treatment,” we removed it from the sample.)

We used two different estimation approaches, which are described in detail in Materials and Methods. The first approach computes the average effect of a CITES Appendix I listing on the IUCN status of listed species using ordinary least squares regression methods. The
Effect of CITES trade bans on mammalian species

**DD and DDD regression results**

The left column of graphs in Fig. 1 displays the estimates of the average effect of a CITES Appendix I listing on the IUCN status of listed mammalian species. [Formally, this is the parameter $\alpha$ in the linear DD regression model (Eq. 2), described in Materials and Methods.] Each panel shows point estimates and 95% confidence intervals for different model specifications and/or subsamples, for time lags ($\ell$) up to 5 years. Full regression results for the first panel in Figs. 1 and 2 are in the Supplementary Materials; others are available upon request.

The top panel reports estimates derived from the full mammalian sample and includes several economic and scientific control variables [e.g., average gross domestic product (GDP) per capita and average population density in the geographical distribution of a species]. The estimated effect of CITES trade bans on IUCN status is negative, i.e., indicating an improvement in endangerment, in all lag specifications and statistically significant at the 5% level or above for most lag specifications. Specifically, a CITES listing improves species status by up to 0.3 points on IUCN’s seven-point scale.

The middle panel in Fig. 1 shows results when the sample is restricted to early-tracked mammalian species, which we define as those species tracked in the IUCN Red List since before 1996. Note, from Table 1, that the average IUCN status across all observations is approximately 1 (near threatened), which is due primarily to the fact that the IUCN status is 0 (“least concern”) in more than one-half of observations. Moreover, in only 4.8% of observations for mammals is a CITES trading ban in effect. Thus, a majority of observations in our dataset pertain to species unlikely to be considered by CITES for conservation action in the form of trading bans. However, mammalian species tracked since before 1996 were substantially more threatened when they were first being tracked by IUCN than species that were added to the Red List later. To ensure that our estimates accurately represent the effect of trading bans on higher-concern species typically considered for CITES intervention, we estimated a CITES treatment effect using these early-tracked species only. Although almost 80% of observations are discarded, the restriction retains 31 of the 41 treated mammalian species. The bottom panel in Fig. 1 shows results when the sample is restricted to treated mammalian species only (i.e., those listed in Table 2). This restriction ensures that our results are not biased by unobservable characteristics that influence both CITES’ decision to list a species in Appendix I and trends in the species outcomes. For example, some species are likely to be endangered because of their high trade value, and a high trade value could create opposition to CITES

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**Table 1. Summary statistics.** The unit of observation is a species-year combination. The dataset covers 5687 mammalian species and 6084 reptilian species and the time period 1982–2018. The dataset is an unbalanced panel (i.e., observations of some variables and some species-year combinations are not available). BDL, Biodiversity Heritage Library. GDP, gross domestic product.

|                      | Mammals                           | Reptiles                          |
|----------------------|-----------------------------------|-----------------------------------|
|                      | $N$  | Mean  | SD   | Min. | Max. |
| IUCN status, as defined in (Eq. 1) | 113,621 | 1.011 | 1.421 | 0    | 6    |
| Species is listed in CITES Appendix I | 113,621 | 0.048 | 0.213 | 0    | 1    |
| Species is commercially targeted | 113,621 | 0.230 | 0.421 | 0    | 1    |
| GDP per capita* (in 2010 USD) | 110,573 | 9.410 | 13,470 | 95   | 68,150 |
| Trade as fraction of GDP* (in percent) | 103,723 | 62.7  | 26.9  | 0.17 | 513.8 |
| Population density* (in 1/km²) | 110,854 | 102.1 | 168.0 | 1.47 | 3,622 |
| Cumulative BDL citations | 113,621 | 44.1  | 110.1 | 0    | 2,670 |
|                      | $N$  | Mean  | SD   | Min. | Max. |
| IUCN status, as defined in (Eq. 1) | 45,619  | 1.060 | 1.421 | 0    | 6    |
| Species is listed in CITES Appendix I | 45,619 | 0.036 | 0.187 | 0    | 1    |
| Species is commercially targeted | 45,619 | 0.121 | 0.326 | 0    | 1    |
| GDP per capita* (in 2010 USD) | 44,125 | 13,711 | 16,272 | 137  | 93,606 |
| Trade as a fraction of GDP* (in percent) | 37,482 | 65.8  | 32.0  | 0.17 | 860.8 |
| Population density* (in 1/km²) | 45,171  | 136.8 | 278.0 | 1.98 | 7,096 |
| Cumulative BDL citations | 45,619  | 19.8  | 52.1  | 0    | 1,172 |

*Variable represents the average over all countries in a given species’ distribution.
| Species                                      | Scientific name            | Year of CITES Appendix I inclusion | IUCN status in year of inclusion (or first available status) |
|----------------------------------------------|----------------------------|-----------------------------------|------------------------------------------------------------|
| Guadalupe fur seal* ,†                      | Arctocephalus townsendi    | 1979                              | Vulnerable (1982)                                          |
| Gray wolf*                                   | Canis lupus                | 1979                              | Vulnerable (1982)                                          |
| Addax* ,†                                    | Addax nasomaculatus        | 1983                              | Endangered (1986)                                          |
| African wild ass* ,†                         | Equus africanus            | 1983                              | Endangered (1986)                                          |
| Northern bottlenose whale* ,†                | Hyperoodon ampullatus      | 1983                              | Vulnerable (1986)                                          |
| Dama gazelle* ,†                             | Nanger dama                | 1983                              | Vulnerable (1986)                                          |
| Scimitar-horned oryx* ,†                     | Oryx dama                  | 1983                              | Endangered (1986)                                          |
| Giant panda*                                 | Ailuropoda melanoleuca     | 1984                              | Near threatened (1986)                                     |
| Black muntjac* ,†                            | Muntiacus crinifrons       | 1985                              | Near threatened (1986)                                     |
| Golden snub-nosed monkey* ,†                 | Rhinopithecus roxelliana   | 1985                              | Near threatened (1986)                                     |
| Chacoan peccary* ,†                          | Catagonus wagneri          | 1987                              | Vulnerable                                                |
| Jentink’s duiker* ,†                         | Cephalophus jentinki       | 1990                              | Endangered                                                |
| Ocelot* ,†                                   | Leopardus pardalis        | 1990                              | Endangered                                                |
| Margay* ,†                                   | Leopardus wiedii           | 1990                              | Vulnerable                                                |
| African bush elephant* ,†                    | Loxodonta africana        | 1990                              | Vulnerable                                                |
| Iberian lynx*                                | Lynx pardinus             | 1990                              | Endangered                                                |
| Sloth bear* ,†                               | Melursus ursinus           | 1990                              | Endangered                                                |
| Okinawa flying fox*                          | Pteropus loochoensis       | 1990                              | Extinct in wild (1994)                                     |
| Marianas flying fox* ,†                      | Pteropus mariannus         | 1990                              | Endangered                                                |
| Pohnpei flying fox* ,†                       | Pteropus molossinus        | 1990                              | Endangered                                                |
| Large Palau flying fox* ,†                   | Pteropus pilosus           | 1990                              | Extinct in wild                                            |
| Samoan flying fox* ,†                        | Pteropus samoensis         | 1990                              | Endangered                                                |
| Pacific flying fox* ,†                       | Pteropus tonganus          | 1990                              | Endangered                                                |
| Pronghorn†                                   | Antilocapra americana      | 1992                              | Least concern (1996)                                       |
| Geoffroy's cat†                               | Leopardus geoffroyi       | 1992                              | Least concern (1996)                                       |
| Golden-capped fruit bat* ,†                  | Acerodon jubatus           | 1995                              | Endangered                                                |
| Red panda* ,†                                | Ailurus fulgens            | 1995                              | Vulnerable                                                |
| Saola* ,†                                    | Pseudoryx nghetinhensis    | 1995                              | Endangered                                                |
| Irrawaddy dolphin                            | Orcaella brevirostris      | 2005                              | Vulnerable (2008)                                          |
| Australian snubfin dolphin                   | Orcaella heinsohni         | 2005                              | Near threatened (2008)                                     |
| Cuvier’s gazelle* ,†                         | Gazella cuvieri           | 2007                              | Endangered                                                |
| Slender-horned gazelle* ,†                   | Gazella leptoceros        | 2007                              | Endangered                                                |
| Bengal slow loris†                           | Nycticebus bengalensis     | 2007                              | Vulnerable (2008)                                          |
| Greater slow loris†                          | Nycticebus coucang        | 2007                              | Least concern (1996)                                       |
| Pygmy slow loris†                            | Nycticebus pygmaeus        | 2007                              | Vulnerable                                                |
| African manatee* ,†                          | Trichechus senegalensis    | 2013                              | Vulnerable                                                |
| Barbary macaque* ,†                          | Macaca sylvanus            | 2017                              | Endangered                                                |
| Indian pangolin†                              | Macaca cynomolgus          | 2017                              | Endangered                                                |
| Philippine pangolin†                         | Macaca sylvanus            | 2017                              | Endangered                                                |
| Sunda pangolin†                               | Macaca fascicularis        | 2017                              | Critically endangered                                      |
| Chinese pangolin†                             | Macaca fascicularis        | 2017                              | Critically endangered                                      |

*Species has been tracked by IUCN since before 1996. †Species is commercially targeted.
methodology. The right column in Fig. 1 displays these DDD estimates, that is, our estimates of the effect of a CITES trade ban on commercial targets, relative to nontargets. All DDD estimates in Fig. 1 are negative. A statistically significant, negative DDD estimate indicates that trade bans are associated with larger improvements in commercial target species relative to nontargets. All DDD estimates in Fig. 1 are negative. Furthermore, all but three estimates are statistically significant at the 5% level or better.

**Ordered probit results**

We estimated the ordered probit model using the same time lags and the same set of explanatory variables contained in our linear DD and DDD specifications. Because the model is nonlinear, it can only be estimated on relatively small samples; accordingly, we estimated it using the sample of treated mammalian species only. The left part of Table 4 corresponds to our previous DD specification and reports the average marginal effects of a CITES Appendix I listing, which further contributes to their endangerment (and, in turn, drives up values over time). By restricting the sample only to species that received an Appendix I listing during the study period, we minimize the potential for selection effects or time-varying biases to influence our results. Ferraro and Simorangkir (14) used a similarly motivated sample restriction to study the effects of an economic policy on environmental outcomes.

Both restricted samples generate similar results. The estimates increase in magnitude compared to the full-sample results and are statistically significant for all lags. A CITES Appendix I listing is associated with a reduction in endangerment by up to 0.52 points on average ($P < 0.001$).

We now turn to the right column of graphs in Fig. 1. IUCN records a set of known threats for each species, and we used this information to classify species by whether they are intentional targets of commercial activity. With this classification, we further isolated the effect of trade bans on species status depending on whether they are commercial targets using the DDD regression approach. The right column in Fig. 1 displays these DDD estimates, that is, our estimates of the effect of a CITES trade ban on commercial targets, relative to nontargets. [Formally, this is the parameter $\beta$ in the linear regression model (Eq. 3) described in Materials and Methods.]

Note that the DDD estimator exploits variation in the sample over time and across four groups of species, determined by whether a species received a trade ban during the relevant period and whether a species is commercially targeted. As Table 2 shows, the group of mammalian species that received trade bans since 1978 and are not commercially targeted is relatively small, containing only four species. Therefore, we cannot expect the DDD model to be estimated with the same precision as the DD model. Moreover, the magnitude of the DDD estimate is heavily influenced by a small number of species that serve as the counterfactual for commercially targeted Appendix I species and must, therefore, be interpreted with caution. In particular, depending on subsample and time lag used, fewer than four treated nontarget mammalian species contribute treatment variation to a given regression. Nevertheless, a statistically significant, negative DDD estimate indicates that trade bans are associated with larger improvements in commercial target species relative to nontargets. All DDD estimates in Fig. 1 are negative. Furthermore, all but three estimates are statistically significant at the 5% level or better.

### Table 3. Reptilian species that contribute treatment variation to the analysis

Species listed contribute treatment variation to at least one regression. Depending on time lag considered and availability of IUCN status and control variables, not every species contributes treatment variation to every regression.

| Species                              | Scientific name       | Year of CITES Appendix I inclusion | IUCN status in year of inclusion |
|--------------------------------------|-----------------------|-----------------------------------|----------------------------------|
| American crocodile*†                 | Crocodylus acutus     | 1979                              | Endangered (1982)                |
| Saltwater crocodile*                 | Crocodylus porosus    | 1979                              | Endangered (1982)                |
| Bolson tortoise*†                    | Gopherus flavomarginatus | 1979                     | Endangered (1982)                |
| Simony’s lizard*†                    | Gallotia simonyi      | 1987                              | Vulnerable                       |
| Bog turtle*†                         | Glyptemys muhlengbii  | 1992                              | Near threatened                  |
| Kleinman’s tortoise*†                | Testudo kleinmann     | 1995                              | Vulnerable                       |
| Antsinyi leaf chameleon*†            | Brookesia perarmata   | 2003                              | Vulnerable                       |
| Flat-tailed tortoise*†               | Pyxis planicauda      | 2003                              | Endangered                       |
| Spider tortoise*†                    | Pyxis arachnoides     | 2005                              | Vulnerable                       |
| Asian narrow-headed softshell turtle* | Chitra chitra         | 2013                              | Critically endangered           |
| Burmese star tortoise*†              | Geochelone platynota  | 2013                              | Critically endangered           |
| Big-headed turtle*                   | Platysternon megacephalum | 2013                     | Endangered                       |
| Anzuetoi arboreal alligator lizard   | Abronia anzuetoi      | 2017                              | Vulnerable                       |
| Campbell’s alligator lizard†        | Abronia campbelli     | 2017                              | Critically endangered           |
| Abronia fimbriata                    | A. fimbriata          | 2017                              | Endangered                       |
| Frost’s arboreal alligator lizard    | Abronia frosti        | 2017                              | Critically endangered           |
| Abronia meledona                     | A. meledona           | 2017                              | Endangered                       |
| Psychedelic rock gecko†              | Cnemaspis psychedelica | 2017                     | Endangered                       |
| Turquoise dwarf gecko†               | Lygodactylus williamsi | 2017                    | Critically endangered           |
| Chinese crocodile lizard†            | Shinisaurus crocodilus | 2017                    | Endangered                       |

*Species has been tracked by IUCN since before 2007. †Species is commercially targeted.
listing on the probability that a species occupies any given rung on IUCN’s assessment scale. In all lag specifications, CITES Appendix I inclusion shifts probability mass from relatively unfavorable outcomes (“extinct,” “extinct in the wild,” “critically endangered,” and endangered) to relatively favorable outcomes (vulnerable, near threatened, and least concern). With the exception of the estimates on the extinct in the wild category, these effects are statistically significant at the 10% level of significance or above. The total mass shifted from the unfavorable to favorable outcomes is as large as 0.17 (for \( \ell = 2 \)). Thus, the trade ban reduced the probability that the listed species were assessed as endangered or worse by up to 17%.

The right part of Table 4 corresponds to our previous DDD specification and reports the average marginal effects of a CITES Appendix I listing on the probability that a commercially targeted species occupies any given rung on IUCN’s assessment scale, relative to nontarget species. Once again, the trade bans shift probability mass

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**Fig. 1. Effect of CITES trade bans on IUCN status—Linear models: Mammals.** (A to C) The y-axis represents the DD (\( \alpha \)) or DDD (\( \beta \)) estimate of the effects of the CITES trade bans (measured in points on IUCN’s 7-point scale), and the x-axis represents the time lag (\( \ell \)) in the CITES trade bans (measured in years). Bands shown represent 95% confidence intervals. All regressions were estimated using ordinary least squares; SEs are robust and adjusted for clustering on species. Full regression results for (A) are provided in table S1 (others are available upon request).
from less favorable to more favorable outcomes. The estimates are significant for $\ell \geq 2$, and the total shift is as large as 36% (for $\ell = 4$).

**Additional results**

Last, within our linear framework, we considered two additional sample restrictions that use variables of interest reported by IUCN and CITES. The results of these regressions are in fig. S3 (2a and 2b).

First, we restrict the set of early-tracked mammalian species to those at high risk of international trade. We include in this set all treated species, as well as those species not in CITES Appendix I that IUCN explicitly classifies as being traded internationally. This restriction removes observations for species not internationally traded, which might otherwise dilute worsening trends among traded species. Thus, if international trade is a threat to species survival, then we should expect the improving effect of an Appendix I listing to become stronger when excluding observations of nontraded species. When restricting the sample in this way, the estimated

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**Fig. 2. Effect of CITES trade bans on IUCN status—Linear models: Reptiles.** (A to C) The $y$ axis represents the DD ($\alpha$) or DDD ($\beta$) estimate of the effects of the CITES trade bans (measured in points on IUCN’s 7-point scale), and the $x$ axis represents the time lag ($\ell$) in the CITES trade bans (measured in years). Bands shown represent 95% confidence intervals. All regressions were estimated using ordinary least squares; SEs are robust and adjusted for clustering on species. Full regression results for (A) are provided in table S2 (others are available upon request).
Effect of CITES trade bans on reptilian species

Figure 2 is organized in the same way as Fig. 1 and contains linear regression estimates of the effect of CITES trade bans on the status

Table 4. Effects of CITES trade bans on IUCN status—Ordered probit models: Mammals (N = 1083). Average marginal effects of maximum likelihood estimates are reported. Models were estimated using the set of treated mammalian species. All regressions include year fixed effects; species fixed effects; and controls for GDP per capita, trade as fraction of GDP, population density, and cumulative Biodiversity Heritage Library (BDL) citations. Numbers in parentheses are P values associated with robust SEs (adjusted for clustering on species). LC, least concern; NT, near threatened; VU, vulnerable; EN, endangered; CR, critically endangered; EW, extinct in the wild; EX, extinct.

| 🃏 | 0 (LC) | 1 (NT) | 2 (VU) | 3 (EN) | 4 (CR) | 5 (EW) | 6 (EX) | Pseudo-R² |
|---|---|---|---|---|---|---|---|---|
| 𝜀 = 0 | 0.054 | 0.040 | 0.032 | -0.060 | -0.048 | -0.016 | -0.002 | 0.554 |
| | (0.057) | (0.051) | (0.055) | (0.042) | (0.043) | (0.058) | (0.037) | (0.056) |
| | 0.075 | 0.053 | 0.041 | -0.081 | -0.065 | -0.021 | -0.003 | 0.560 |
| | (0.014) | (0.010) | (0.011) | (0.012) | (0.007) | (0.007) | (0.009) | (0.006) |
| | 0.074 | 0.054 | 0.038 | -0.075 | -0.065 | -0.020 | -0.003 | 0.561 |
| | (0.009) | (0.008) | (0.009) | (0.007) | (0.008) | (0.008) | (0.007) | (0.006) |
| | 0.069 | 0.051 | 0.034 | -0.059 | -0.041 | -0.017 | -0.003 | 0.560 |
| | (0.015) | (0.012) | (0.011) | (0.008) | (0.012) | (0.014) | (0.011) | (0.007) |
| | 0.061 | 0.043 | 0.027 | -0.049 | -0.002 | -0.013 | -0.002 | 0.558 |
| | (0.005) | (0.004) | (0.005) | (0.007) | (0.006) | (0.012) | (0.005) | (0.005) |
| | 0.066 | 0.050 | 0.020 | -0.055 | -0.002 | -0.004 | -0.000 | 0.558 |
| | (0.015) | (0.012) | (0.014) | (0.008) | (0.012) | (0.014) | (0.011) | (0.007) |
| | 0.061 | 0.046 | 0.027 | -0.087 | -0.045 | -0.014 | -0.002 | 0.558 |
| | (0.005) | (0.004) | (0.005) | (0.007) | (0.006) | (0.012) | (0.005) | (0.005) |
| | 0.069 | 0.057 | 0.027 | -0.112 | -0.071 | -0.022 | -0.000 | 0.556 |
| | (0.014) | (0.010) | (0.014) | (0.012) | (0.014) | (0.014) | (0.011) | (0.007) |
| | 0.077 | 0.065 | 0.020 | -0.141 | -0.112 | -0.035 | -0.002 | 0.571 |
| | (0.005) | (0.004) | (0.005) | (0.006) | (0.006) | (0.014) | (0.006) | (0.006) |
| | 0.115 | 0.074 | 0.017 | -0.179 | -0.141 | -0.044 | -0.000 | 0.577 |
| | (0.005) | (0.004) | (0.005) | (0.006) | (0.006) | (0.014) | (0.006) | (0.006) |
| | 0.143 | 0.069 | 0.013 | -0.199 | -0.179 | -0.056 | -0.002 | 0.579 |
| | (0.005) | (0.005) | (0.005) | (0.006) | (0.006) | (0.014) | (0.006) | (0.006) |
| | 0.180 | 0.054 | 0.002 | -0.208 | -0.199 | -0.056 | -0.002 | 0.581 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.014) | (0.006) | (0.006) |

Effect of CITES trade bans on reptilian species

Figure 2 is organized in the same way as Fig. 1 and contains linear regression estimates of the effect of CITES trade bans on the status
We also estimated our ordered probit model for reptilian species. While the ordered probit model must be restricted to relatively small samples, the sample pertaining to treated reptiles only is too sparse to permit estimation of Appendix I effects in all seven of IUCN’s endangerment categories or estimation of Appendix I effects on commercially targeted species relative to nontarget species. To obtain an adequate sample size, we added observations pertaining to 10 reptilian species that were either included in Appendix I in 1977 or included after 1977 but did not have an IUCN status for 5 years or more following the initial listing. While these species do not themselves provide treatment variation to the analysis, they are suitable control species because they were added to CITES Appendix I during or shortly before our study period. Ordered probit results for this sample of reptiles are reported in Table 5.

In all lag specifications, CITES Appendix I inclusion shifts probability mass from relatively favorable outcomes (least concern, near threatened, and vulnerable) to relatively unfavorable outcomes (endangered, critically endangered, extinct in the wild, and extinct). With the exception of the marginal effects on the endangered and extinct in the wild categories, the estimates are statistically significant at the 5% level of significance or above, and the total probability shift from favorable to unfavorable categories is as large as 42.6% (ℓ = 3). Inclusion of the interaction with commercial target status confirms our finding that the estimated effects of CITES Appendix I inclusion are primarily driven by the effects on commercially targeted species.

### Explaining the difference

For mammals, the effect of CITES trading bans is in the intended direction: The bans resulted in improvements in the IUCN status of treated mammalian species relative to nontreated species. For the bans to have had the opposite effect on reptiles, some unintended consequence of these bans must have occurred that outweighed the intended effects. Rivalan et al. (10) hypothesized that CITES trade bans could spur 11th-hour sales before the bans going into effect. Consistent with this hypothesis, Rivalan et al. (10) found that trade in several animal species increased after the announcement of the trading bans but declined once the bans had gone into effect. For highly endangered species, these last-minute trading activities could reduce their populations irreparably. Perverse anticipatory effects of conservation action have also been documented in the context of the U.S. Endangered Species Act (ESA), where private landowners have deliberately destroyed populations or habitats of soon-to-be-listed species in an effort to escape subsequent development constraints under the ESA (15–17).

To examine whether this possibility explains why CITES trade bans hurt reptilian species but not mammalian species, we examined annual legal import quantities of Appendix I species published by CITES, where available. Drawing conclusions about entire classes of species from these data is complicated for several reasons, including the fact that import quantities were not available for all species and the fact that imports sometimes occur in animal parts or products that cannot always be suitably aggregated. In Materials and Methods, we describe our procedure to deal with these complications in more detail. We found noticeable volume spikes for several of the treated reptilian species in either the year of the CITES trade ban or in one of the two preceding years. These spikes are consistent with the 11th-hour trade hypotheses. Similar increases were not found for mammalian species.

To visualize our findings, Fig. 3 displays legal import quantities of whole specimens, where available, for both reptilian and mammalian treated species in the years before and after the CITES

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**Table 5. Effects of international trade bans on species status—Ordered probit: Reptiles (N = 697).** Average marginal effects of maximum likelihood estimates are reported. Models were estimated using the set of treated reptilian species plus 10 control species to obtain an adequate sample size. All regressions include year fixed effects; species fixed effects; and controls for GDP per capita, trade as fraction of GDP, population density, and cumulative BDL citations. Numbers in parentheses are P values associated with robust SEs (adjusted for clustering on species).

| ℓ = 0 | ℓ = 1 | ℓ = 2 | ℓ = 3 | ℓ = 4 | ℓ = 5 | Effect of CITES_{s, t−ℓ} on the probability that STATUS_{s, t−ℓ} = 0 is equal to | Effect of CITES_{s, t−ℓ} × TARGET_{s, t−ℓ} on the probability that STATUS_{s, t−ℓ} = 0 is equal to |
|-------|-------|-------|-------|-------|-------|---------------------------------|---------------------------------|
| 0 (LC) | -0.074 | -0.092 | -0.101 | -0.114 | -0.103 | -0.089 | -0.086 | -0.099 | -0.106 | -0.119 | -0.128 | -0.170 |
| (0.016) | (0.021) | (0.018) | (0.017) | (0.008) | (0.009) | (0.038) | (0.056) | (0.046) | (0.037) | (0.012) | (0.037) |
| 1 (NT) | -0.104 | -0.127 | -0.139 | -0.158 | -0.145 | -0.127 | -0.120 | -0.135 | -0.144 | -0.162 | -0.179 | -0.241 |
| (0.019) | (0.015) | (0.017) | (0.018) | (0.016) | (0.019) | (0.038) | (0.047) | (0.044) | (0.040) | (0.019) | (0.042) |
| 2 (VU) | -0.136 | -0.157 | -0.160 | -0.164 | -0.155 | -0.141 | -0.143 | -0.148 | -0.145 | -0.148 | -0.176 | -0.252 |
| (0.047) | (0.035) | (0.028) | (0.024) | (0.024) | (0.021) | (0.058) | (0.057) | (0.043) | (0.032) | (0.024) | (0.039) |
| 3 (EN) | 0.050 | 0.060 | 0.068 | 0.081 | 0.086 | 0.086 | 0.037 | 0.042 | 0.051 | 0.065 | 0.092 | 0.147 |
| (0.211) | (0.173) | (0.132) | (0.093) | (0.087) | (0.084) | (0.377) | (0.362) | (0.286) | (0.209) | (0.141) | (0.122) |
| 4 (CR) | 0.192 | 0.228 | 0.234 | 0.244 | 0.212 | 0.180 | 0.230 | 0.245 | 0.245 | 0.254 | 0.269 | 0.357 |
| (0.004) | (0.002) | (0.002) | (<0.001) | (<0.001) | (<0.001) | (0.001) | (0.010) | (0.005) | (0.002) | (0.001) | (0.012) |
| 5 (EW) | 0.005 | 0.006 | 0.007 | 0.009 | 0.008 | 0.007 | 0.006 | 0.008 | 0.009 | 0.010 | 0.011 | 0.015 |
| (0.138) | (0.182) | (0.177) | (0.167) | (0.114) | (0.088) | (0.176) | (0.208) | (0.202) | (0.196) | (0.138) | (0.144) |
| 6 (EX) | 0.066 | 0.081 | 0.090 | 0.103 | 0.096 | 0.084 | 0.075 | 0.088 | 0.092 | 0.101 | 0.110 | 0.145 |
| (0.008) | (0.014) | (0.014) | (0.017) | (0.011) | (0.009) | (0.028) | (0.050) | (0.042) | (0.034) | (0.011) | (0.009) |
| Pseudo-R² | 0.534 | 0.476 | 0.483 | 0.494 | 0.482 | 0.467 | 0.493 | 0.505 | 0.511 | 0.520 | 0.504 | 0.489 |
Fig. 3. Eleventh-hour trade spikes exist for reptiles but not mammals. Source: CITES import data. The figure is based on eight mammalian and seven reptilian treated species for which sufficient import data were available and for which these data could be aggregated to the whole specimen level. Year 0 represents the “11th hour” for each given species, defined as the year with the largest legal import quantity within the 3-year window leading up to (and including) the CITES trade ban for that species.

Since CITES does not operate in secrecy, increased trading activity in anticipation of impending trading bans is generally not preventable. There are, however, ways to minimize their adverse impacts. Note that the economic incentive to engage in 11th-hour sales may be especially strong when a species is highly endangered, as relative scarcity of specimens is expected to drive up prices. At the same time, highly endangered species are those to which these sales are likely to be most damaging. One implication, then, is that CITES trade bans should be implemented at endangerment levels at which anticipatory trade spikes, if they occur, are less likely to cause irreparable harm.

Interaction of CITES trade bans and other conservation efforts

A CITES Appendix I listing is not the only way to protect a species. Measures such as habitat restoration, education and awareness campaigns, the establishment of wildlife refuges, or the ex situ protection of threatened species are commonly used conservation tools. If an Appendix I listing was correlated with the introduction of such measures, as could be the case if states allocated more funding to the protection of listed species, then our DD estimates would reflect the combined effect of the bans and these complementary conservation strategies. One may then worry that the beneficial effect of an Appendix I listing for mammals that we found is due primarily to complementary conservation efforts. For the policy decision of whether to ban international trade, however, the relevant quantity is the separate effect of the ban, as the ban can be implemented independent of complementary conservation efforts.

On the other hand, some conservation tools are inherently incompatible with trade bans. Conservation in the form of game farming or harvest management strategies (e.g., regulated legal hunts) are most effective when trade in specimens or animal products remains legal and may be hampered when trade is banned. Thus, one may worry that our estimates did not properly account for the possibility that trade bans crowd out these market-based conservation efforts. For the decision of whether to adopt a trade ban, the relevant quantity is the net effect of the ban, including any reductions in the effectiveness of existing market-based strategies caused by the ban.

To examine the robustness of our estimates against the possibility of these interactions between trade bans and other conservation measures, we examined two conservation strategies. For all early-tracked species in our dataset, we collected from IUCN’s website information on (i) whether the species is presently subject to a harvest management plan (HMP) and (ii) whether it is presently subject to ex situ conservation (ESC). A HMP ensures that harvests of a species are regulated and that proceeds from the sale of harvested specimens or harvesting permits flow back to conservation authorities. ESC involves protecting and propagating a species outside its natural habitat, for example, in zoos, aquariums, or wildlife parks. Both HMPs and ESC are important tools in our context, as they can be used to protect species intentionally targeted by humans. Historical information on when these measures were first applied (or if they were once applied and then discontinued) would have been preferable; however, such time-varying information was not available in IUCN’s API.

Harvest management plans

Wildlife officials tasked with managing species populations have an incentive to set sustainable harvesting targets to maximize the
long-run revenue stream generated by the sale of harvested specimens (or the sale of harvesting permits), as these revenues can be used to fund further conservation efforts. As discussed above, a ban on international trade in the protected species could counteract this market-based conservation strategy.

Only 23 of the 630 early-tracked mammals, and only 8 of the 672 early-tracked reptiles, have such plans at present. Moreover, only 2 of all 61 treated species in the entire dataset have HMPs at present. [One of these species is the African elephant; see, e.g., (20). The other is the giant panda; this is likely a data error, as we are unaware of any legal harvests of this species.] It is possible that at least some of the treated species in our sample were previously protected by HMPs, but the trade bans "killed" these measures by shifting off international revenues. In this case, non-CITES species without HMPs would not provide an appropriate counterfactual for how the status of these treated species would have evolved but for the trade bans. Specifically, our previous DD estimates would likely be "too optimistic," in that they would not have accounted for counterfactual improvements the species would have experienced had their HMPs continued. On the other hand, if the treated species never had HMPs, the presence of non-CITES species with HMPs in the sample would bias our results: These species may have improved because of their HMPs, while similar counterfactual improvements would not have accrued to the treated species.

To examine whether these potential biases affect our conclusions, we split the group of early-tracked untreated species into two subsets: untreated species without HMPs and untreated species with HMPs. We then estimated our DD model using each of these subsets as separate control groups for the treated species without HMPs. Using the first control group minimizes the potential bias arising from the fact that some untreated species enjoy protections that do not presently apply to treated species and may never have applied to them. To understand why we use the second control group, suppose, hypothetically, that a treated species was subject to an HMP before it received its CITES listing but that the CITES trade ban killed the HMP. Had this species not received the CITES listing, it would have retained its HMP, that is, it would now look like an untreated species with an HMP. Thus, by using the second control group, we maximize the degree to which the DD estimates reflect the potential replacement of HMPs with trade bans, which, of course, is not directly observable in our data.

The results are reported in Fig. 4A. The red line represents the effect of CITES Appendix I listings on treated species without HMPs using species without HMPs as the control group. The blue line represents the corresponding estimate when the control group consists of untreated species with HMPs. The blue line is above the red line for both mammals and reptiles. This was expected, given that the blue line is based on a more favorable counterfactual, which pushes the estimated treatment effect in a less favorable direction (i.e., upward).

For mammals, the two sets of estimates are similar in magnitude to the estimates in panel 3 of Fig. 1 and to each other. This implies that our previous results for mammals are robust to the potential biases presented by HMPs. The estimates on the blue line are still negative and statistically significant. Therefore, our conclusion that the CITES trade bans are an effective conservation strategy for mammalian species remains robust, even if the bans may have weakened existing market-based conservation measures. For reptiles, the distance between the two sets of estimates is much more pronounced. This means that our previous estimates for reptiles could have been influenced to a larger degree by potential unobserved interactions with HMPs. However, even the lower estimates on the red line in the right panel of Fig. 4 are positive, statistically significant, and approximately of the same magnitude as those panel 3 of Fig. 1. Thus, our conclusion that banning international trade hurt reptilian species is robust as well.

**Ex situ conservation**

Unlike what is the case for HMPs, the effectiveness of ESC measures is not reduced when commercial trade in a species is banned. A CITES Appendix I listing also does not prohibit the international transfer of specimens for noncommercial conservation purposes. Therefore, trade bans and ESC measures can be implemented independently of each other. However, ESC measures generally require funding (unlike HMPs, which typically generate revenue). If public funds are more likely to be allocated to the protection of listed species than unlisted species, then the introduction of complementary conservation measures such as ESC can be temporally correlated with CITES trade bans. In the context of the ESA, Ferraro et al. (17) found that more public funding was allocated to conservation of species after they were listed as protected, although this direct consequence of a listing under the act is not fiscal (but involves the imposition of constraints on the use of land and water).

The possibility that ESC is correlated with CITES trade bans would not reverse our conclusion for reptiles: If the combined effect of trade bans and complementary conservation efforts is to reduce endangerment, then the effect of the ban alone must have been even more harmful. However, it could affect our conclusion for mammals, for which we found the bans to have been beneficial. This finding may be unwarranted if most or all of the improving effect is due to the concurrent introduction of other conservation efforts.

One hundred forty-four early-tracked mammalian species and 80 early-tracked reptilian species are currently subject to ESC, including 14 treated mammals and 8 treated reptiles. Because we have no data on when the ESC measures were introduced for each species, we cannot directly examine to what extent these measures correlate with CITES trade bans. However, we made the extreme assumption that, whenever a CITES species is also subject to ESC, the ESC treatment started in the same year of the CITES treatment. This would be the "worst case" scenario as far as the interpretation of our previous DD estimates is concerned, as the temporal correlation between both measures is now maximized. Because some treated species are not subject to ESC, it is possible to identify the effect of trade bans and ESC separately, under the worst case assumption of perfect temporal correlation of the two measures.

Figure 4B shows regression estimates from a DD model containing separate treatment effects for CITES trade bans and ESC measures using early-tracked untreated species not subject to ESC as the control group. [Formally, these are the parameters α and β in the regression model (Eq. 4) described in Materials and Methods.] Results for mammals are shown in the left panel. The estimated effect of the trade ban is negative (i.e., improving) and statistically significant except when \( \ell = 0 \). Its magnitude is comparable to that of our previous DD regressions, shown in panel 3 of Fig. 1. At the same time, the estimated effect of ESC is generally negative but not statistically different from zero except when \( \ell = 0 \). We conclude that, even if the ESC measures were introduced in exactly the same years as the trade bans, our DD results would still primarily reflect the effect of the trade bans and not the effect of concurrent ESC measures.
The right panel in Fig. 4B shows the corresponding results for reptiles. The estimated effect of ESC is positive, statistically significant, and multiple times larger than the estimated effect of the trade ban. This would be a highly implausible result if the ESC measures had been implemented concurrently with the trade bans: It would mean that the ESC measures were detrimental to species status and that they were many times more detrimental as the trade bans themselves. It is far more likely that reptilian species at risk of harm from 11th-hour trade spikes were also more likely to be subject to conservation measures in general, including ESC. As discussed above, this possibility does not threaten the conclusion that the trade bans harmed reptilian species. For example, reptilian species currently subject to ESC were already 1.7 points more endangered when they initially entered the IUCN Red List, compared to species not subject to ESC. Thus, some of these species may have received conservation efforts already before they received trade bans. It is also possible that some ESC measures were applied to species
whose status had deteriorated some time following the bans. In both scenarios, our DD estimates of the effect of trade bans on reptilian species would be biased in a conservative direction (i.e., but for the ESC efforts, the status of listed species may have worsened even more).

DISCUSSION
We analyzed the effects of CITES trade bans on the endangerment status of mammalian and reptilian species. We found that the bans have generally worked for mammals but backfired for reptiles. Potentially explaining this differential effect, we found large spikes in legal trade in the year of the bans and in the year immediately preceding the bans on reptilian species but not on mammalian species. Such anticipatory trade spikes may be especially detrimental when the bans are applied to critically endangered species: If market prices for remaining specimens are high, 11th-hour trading may be especially intense, making post-ban recovery less likely. This suggests that CITES trade bans should be implemented at lower endangerment levels (e.g., when a species is vulnerable or endangered, rather than critically endangered). While CITES international trade bans could conceivably have crowded out or crowded in alternative conservation measures, our results were robust to this possibility.

Our econometric analysis is necessarily limited by two main factors: the number of changes that CITES’ Conference of the Parties has made to Appendix I listings over time and by the time when IUCN first assessed each listed species. We could feasibly exploit only 41 changes for mammals and 20 changes for reptiles. Moreover, historical information on the use of alternative conservation measures was unavailable but would help further disentangle the effect of CITES bans from the effects of these alternative measures. Last, many threatened animal species are not traded across international borders but are still bought and sold in local and national markets, and it would be interesting to investigate whether restrictions on national or local trade in endangered species have effects similar to those of restriction on international trade.

MATERIALS AND METHODS
Data
We collected information for all mammalian and reptilian species for which IUCN has reported an endangerment status in one or more years between 1982 and 2018. The data sources that we used are described in the Supplementary Materials.

For each species s in this set, we coded the last available IUCN Red List status classification in year t into a numerical variable STATUSs,t, according to Eq. 1. We then constructed a binary variable CITESs,t,n which equals one if and only if species s was listed in CITES Appendix I at any time in year t. In our empirical framework, described below, CITESs,t,n serves as the main explanatory variable for IUCN,n,t.

For our purposes, it is important to note that IUCN’s classification criteria are based on assessments of species populations, population trends, and distribution patterns and “are to be applied to a [species] whatever the level of conservation action affecting it” (21). Thus, if a species is added to CITES Appendix I, any subsequent status upgrades by IUCN are the result of improvements in the species’ populations, population trends, or distribution patterns and not merely the result of the species being “better protected” because of its Appendix I listing. (Improvements in populations following CITES intervention could still be subject to measurement errors. In particular, if more public funding is allocated to the study of species listed in a CITES Appendix, then previously unknown populations might be found after a species is listed. Our data do not permit us to quantify this possible measurement error. However, the potential for it to influence our results is limited by the fact that many CITES Appendix I species were previously listed in Appendix II and, hence, already subject to heightened scrutiny. Furthermore, for reptilian species, this measurement error would bias our estimates in a conservative direction.)

We used IUCN’s species-specific threat variables to classify species according to whether they are commercially targeted. We classified an aquatic species as a commercial target if its set of threats included “Fishing and harvesting aquatic resources/Intentional use: Large scale/Species is the target.” The remaining aquatic species were classified as not being commercial targets; this group includes bycatch species and species targeted for small-scale, subsistence harvesting only. For terrestrial species, IUCN does not make a similar distinction between large-scale and subsistence harvesting. Lacking such information, we classified a terrestrial species as a commercial target if its set of threats included “Hunting and collecting terrestrial animals/Intentional use/Species is the target.”

We also constructed several control variables that can potentially explain changes in endangerment status but are unrelated to CITES trade restrictions. To control for economic development, general international trade levels, and overall exposure to human activity, we collected country-year level data on GDP per capita, international trade volume as a percentage of GDP, and population density, from the World Bank. For each species and year, we averaged each of these variables over all countries in the species’ geographical distribution, as reported by IUCN. To control for scientific interest in a species, we searched publications in the Biodiversity Heritage Library (BDL) for the scientific name of each species and recorded the cumulative number of pages (up to the given year) of articles on which this name appears at least once.

As discussed in Results and Discussion, some of our estimations were performed on restricted samples to remove a large number of low-concern species from the dataset. The first restriction is to focus on species tracked relatively early by IUCN. Most additions to IUCN’s Red List have occurred in what we refer to as “boost phases.” For mammalian species, the primary boost phase was the year 1996, when nearly two-thirds of all mammalian species in our dataset were tracked. This was followed by a second, smaller boost phase in 2008 that accounts for an additional 12% of species. For reptilian species, a single prolonged boost phase starts in 2007 and accounts for nearly 9 of 10 of all reptilian species in our dataset. Notably, for both mammals and reptiles, species added during boost phases are of lower concern, on average, than species that were already tracked earlier (see fig. S1). This pattern suggests that IUCN deliberately focused its early tracking efforts on species at higher risk of extinction. Our first restriction, therefore, is to limit the sample to mammalian species tracked since before 1996 and to reptilian species tracked since before 2007. This restriction markedly cuts the fraction of low-concern observations in the data with the modal IUCN status now being vulnerable for both mammals and reptiles (see fig. S2). Moreover, although almost 80% of observations are discarded, we retain 31 of 41 mammalian species and 12 of 20 reptilian species with treatment variation, and these species are indicated by an asterisk in Tables 2 and 3.
Our second restriction is to focus only on species that received a CITES international trading ban during the period under consideration, that is, the species listed in Tables 2 and 3. For mammals, this restriction results in approximately the same distribution of IUCN status as restricting the sample to early-tracked species. For reptiles, it further shifts status toward higher-threat categories, with the modal IUCN status now being critically endangered (see fig. S2).

Linear regression models

Our primary empirical framework is based on the DD estimator; that is, we estimate the effect of a CITES trade ban by (i) measuring the average change in the endangerment status of the treated species when moving from before to after the ban takes effect and (ii) subtracting from that the average change in status of the control species over the same time period. Changes in the status of control species serve as a counterfactual for how the status of treated species would have evolved but for the ban.

To be able to account for other observable characteristics, we implemented the DD approach via a linear multiple regression model. The endangerment level of species $s$ in year $t$ is governed by the following equation

$$
\text{STATUS}_{s,t} = \alpha \text{CITES}_{s,t-\ell} + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t}
$$

where $\text{CITES}_{s,t-\ell}$ is a dummy variable that equals one if and only if species $s$ is listed in CITES Appendix I in year $t - \ell$ (where $\ell \geq 0$ is a time lag), $X_{s,t}$ is a vector of species-year level controls, $\mu_s$ and $\theta_t$ are species fixed effects and year fixed effects, respectively, and $\epsilon_{s,t}$ is the error term. The effect of banning international trade on endangerment status is measured by the coefficient $\alpha$. To understand how the regression model implements the DD estimator, consider the simplified case in which there are only two species (1, treated; 0, untreated), only two time periods (1, posttreatment; 0, pretreatment), and no covariates. Then, the DD estimator of the treatment effect is

$$
(E[\text{STATUS}_{s,t}|s=1, t=1]-E[\text{STATUS}_{s,t}|s=1, t=0]) - (E[\text{STATUS}_{s,t}|s=0, t=1]-E[\text{STATUS}_{s,t}|s=0, t=0]) = \alpha
$$

$$
((\alpha + \mu_1 + \theta_1) - (\mu_0 + \theta_0)) - ((\mu_0 + \theta_0) - (\mu_0 + \theta_0)) = \alpha
$$

Our method is based on the same principle but allows for more than two units and more than two time periods, for different units to receive treatment in different years, as well as for outcomes to depend on observable covariates. The DD regression approach is further discussed in, e.g., (22, 23), pages 431–439 of (24), and pages 450–452 of (25). Equation 2 and all other regression models were estimated using ordinary least squares, with robust SEs adjusted for clustering on species.

To further isolate the effect of trade bans on species status, we extend our framework to the DDD approach (23, 26). The DDD estimator computes the difference between the DD estimates of two types of species: those that are commercial targets and those that are not commercial targets. Thus, the DDD estimate measures the effect of the ban on commercial targets relative to nontargets. To perform this comparison, we estimate the following DDD regression model

$$
\text{STATUS}_{s,t} = \alpha \text{CITES}_{s,t-\ell} + \beta \text{CITES}_{s,t-\ell} \times \text{TARGET}_t + \rho_i \text{TARGET}_t + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t}
$$

where $\text{TARGET}_t$ is a dummy variable that equals one if and only if we classified species $s$ as a commercial target and $\rho_i$ are additional year fixed effects applied to commercial target species only. The coefficient $\alpha$ represents the DD effect of banning trade on the status of nontarget species, and $\beta$ represents the incremental effect of trade bans on the status of target species.

Last, to estimate separate effects for CITES trade bans and ESC efforts, under the assumption that, when a listed species is subject to ESC, this measure started in the same year that the species was first listed, we use the following regression model

$$
\text{STATUS}_{s,t} = \alpha \text{CITES}_{s,t-\ell} + \beta \text{CITES}_{s,t-\ell} \times \text{ESC}_t + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t}
$$

where $\text{ESC}_t$ is an indicator variable that is 1 if and only if species $s$ is subject to ESC. Thus, the coefficient $\alpha$ measures the isolated effect of the trade ban on listed species, and $\beta$ measures the effect of concurrent ESC implementation on the listed species. Note that regression model (Eq. 4) appears similar to DDD model (Eq. 3), with the interaction term $\text{CITES}_{s,t-\ell} \times \text{TARGET}_t$ replaced by $\text{CITES}_{s,t-\ell} \times \text{ESC}_t$. However, Eq. 4 also drops the $\rho_i \times \text{TARGET}_t$ interaction term without replacing it with a corresponding $\rho_i \times \text{ESC}_t$ term. This makes Eq. 4 a DD model with heterogeneous treatment effects, rather than a DDD model, which is appropriate: Whether a species is subject to ESC or not is not a fixed condition but is itself a treatment, although we do not observe when the treatment started but assume here that it started in the same year as the trade ban.

In all three regression models (Eqs. 2 to 4), the time lag $\ell$ serves two purposes. First, the effects of CITES trading bans do not materialize immediately. Moreover, IUCN updates a species’ status in intervals that can span several years, resulting in a delay between the time a ban is instituted and the time the species’ status is reassessed following the ban. We allow for such time delays by considering lag values as large as $\ell = 5$ in the analysis. Second, the subset of species that contribute treatment variation in a specific regression depends on the value of $\ell$. For example, a species whose first IUCN status is recorded 3 years after the species first appears in CITES Appendix I exhibits treatment variation only when $\ell \geq 4$. Similarly, a species added to Appendix I in 2017 can only contribute treatment variation when $\ell \leq 1$. Using a range of time lags, therefore, enables us to use the full set of treated species listed in Tables 2 and 3 to identify the effects of CITES trade bans across all regressions (although not within any single regression).

Probit model

Our linear DD and DDD models assume that that species status is measured on an interval scale, given by Eq. 1. To treat species status as an ordinal variable, we also estimated an ordered probit model. The ordered probit method simultaneously estimates a “ladder” whose steps represent the various IUCN Red List threat levels and the effect of Appendix I inclusion on the likelihood that a species occupies any given rung on that ladder.

Formally, the ordered probit approach assumes that the (unobserved) latent endangerment level of species $s$ in year $t$ is a continuous variable

$$
Y_{s,t} = \alpha \text{CITES}_{s,t-\ell} + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t}
$$

$\text{Y}_{s,t}$ represents the incremental effect of trade bans on the status of target species.

$$
\text{STATUS}_{s,t} = \begin{cases} 
1 & \text{if } Y_{s,t} > 0 \\
0 & \text{otherwise}
\end{cases}
$$

$\text{STATUS}_{s,t}$ is the observed species status, and the estimated continuous latent variable $\text{Y}_{s,t}$ is used to separate species with a non-zero expected status change from those with an expected status change of zero. The probability that species $s$ is above a given latent status level $\beta$ in year $t$, that is, $\text{P}(\text{STATUS}_{s,t} = 1 | \alpha \text{CITES}_{s,t-\ell} + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t} > \beta)$, is estimated using the cumulative normal distribution.

$$
\text{P}(\text{STATUS}_{s,t} = 1 | \alpha \text{CITES}_{s,t-\ell} + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t} > \beta) = \Phi(\beta - \alpha \text{CITES}_{s,t-\ell} - \gamma X_{s,t} - \mu_s - \theta_t - \epsilon_{s,t})
$$

where $\Phi$ is the standard normal cumulative distribution function.
where the variables on the left-hand side are defined as in the linear DD model (Eq. 2). The (observed) IUCN status of species \( s \) in year \( t \) is now given by

\[
\text{STATUS}_{s,t} = \begin{cases} 
0 & \text{if } Y_{s,t} < \omega_1 \\
1 & \text{if } \omega_1 \leq Y_{s,t} < \omega_2 \\
\vdots \\
5 & \text{if } \omega_5 \leq Y_{s,t} < \omega_6 \\
6 & \text{if } \omega_6 \leq Y_{s,t} 
\end{cases} \quad (6)
\]

The parameters in Eq. 5 and the thresholds \( \omega_1 , \ldots , \omega_6 \) in Eq. 6 are estimated simultaneously, using maximum likelihood, under the assumption that the error terms \( \epsilon_{s,t} \) are normally distributed. We report the average marginal effects of the parameter \( \alpha \) on the probability that \( \text{STATUS}_{s,t} \) is equal to each of its seven possible values, that is, the average impact of including a species in CITES Appendix I on the probability that its observed IUCN status is least concern, vulnerable, and so on. By including an interaction terms as in Eq. 3, a “triple-difference version” of the same model can be obtained.

The ordered probit approach uses the full variation in status changes without imposing an interval scale assumption on ordinal information. However, it also has a number of disadvantages. First, it can only be implemented in samples with a relatively small number of species because of the computational burden introduced when a large number of fixed effects must be estimated in nonlinear models. Second, given that we observe a relatively small number of years for many species in our dataset, maximum likelihood estimates of nonlinear models can be biased because of the incidental parameter problem (27). Third, in nonlinear models, the species and year fixed effects (\( \mu_s \) and \( \theta_t \)) are conditional fixed effects, i.e., their associated marginal effects are not fixed but depend on the values of other variables. In particular, the marginal effect of the species fixed effect \( \mu_s \) on outcome probabilities now depends on time \((t)\); marginal effect of the year fixed effect \( \theta_t \) now depends on the species \((s)\), and both depend on covariates \((X_{s,t})\). If these dependencies are strong, then the marginal effects associated with the parameter of interest \((\alpha)\) may diverge from its true DD estimator (28).

**Causal identification**

If the estimated value of \( \alpha \) in the DD model (Eq. 2) is statistically significant and negative, trade bans are associated with an improvement in the status of the treated species. Conversely, if the estimated value of \( \alpha \) is statistically significant and positive, trade bans are associated with a deterioration in species status. For this association to have a causal interpretation, two conditions must be met.

The first condition is that the estimated effect should not be attributable to reverse causality or selection effects. In general, international trade bans are not exogenous to species endangerment status: Trade in certain species is restricted precisely because these species are threatened or are becoming more threatened. Treatment selection based on levels of outcome variables, i.e., more threatened species being more likely to be listed in CITES Appendix I, would not bias our DD estimates, as the species fixed effects (\( \mu_s \)) absorb all such differences. However, our DD estimates would be biased if selection were based on trends, that is, if trade bans were more likely to be imposed for species with worsening endangerment levels. If these trends were to continue after treatment, our estimate of \( \alpha \) would likely be positive. However, this would not mean that trade bans caused endangerment levels to change; rather, changes in endangerment status would have affected the likelihood of trade bans.

To ensure that our estimates are not explained by this mechanism, we performed a pretreatment trend analysis. If there were systematic differences in the outcome trends of control species and treated species before the treatment, then it is plausible that these differences would have persisted in the posttreatment period. Since not all species were included in CITES Appendix I in the same year, we estimated the following linear regression model

\[
\text{STATUS}_{s,t} = \sum_d \kappa_d I_{s,t}^d + \gamma X_{s,t} + \mu_s + \theta_t + \epsilon_{s,t} \quad (7)
\]

For \( d \in \{-8,8\}, I_{s,t}^d \) is a dummy variable that equals one if and only if an observation pertains to a treated species \( d \) years before and after the initial year the species was listed in CITES Appendix I. Thus, after controlling for species fixed effects (\( \mu_s \)), year fixed effects (\( \theta_t \)), and observables (\( X_{s,t} \)), the coefficient \( \kappa_d \) measures the effect of “being in the \( d \)th year preceding/following treatment” on species status. See, e.g., (29) for the use of this approach to estimating pretreatment trends. For \( d \in \{-8,8\}, I_{s,t}^d \) is a dummy variable that equals one if and only if an observation pertains to a treated species \( 8 \) or more years before and after the initial year the species was listed in CITES Appendix I. Figure 5 shows the point estimates and 95% confidence intervals for \( \kappa_d \) separately for mammals and reptiles. In the 7 years before being listed in CITES Appendix I, there is no discernible slope in \( \kappa_d \) for either mammals or reptiles, indicating that treated and control species experienced similar time trends in these years. Thus, our empirical approach is not likely to be susceptible to biases introduced by treatment selection based on trends during this 7-year window. On the other hand, there is a relatively pronounced downward slope in \( \kappa_d \) in the posttreatment period for mammals and a relatively pronounced upward slope for reptiles.

Note that Fig. 5 shows significant negative effect of being 8 or more years before treatment, for both mammals and reptiles. This suggests that there could have been early pretreatment trends that worsened the status of treated species, relative to untreated ones. For mammals, a worsening early trend would affect our DD estimates in a conservative direction (i.e., our DD results would underestimate the true causal effect of the treatment). For reptiles, early pretreatment differences may have increased the magnitude of the full-sample DD results; however, as discussed above, we obtained very similar results when using the set of treated reptiles only, which eliminates the potential bias from such trend differences.

The second condition is that the estimated effect should not be attributable to a common third factor correlated with both the independent and dependent variables. In particular, the introduction of international trading bans may be correlated with the introduction of other conservation measures, and it could be these measures (and not the trade bans) that cause improvements in the status of treated species. Our estimates are not likely to be explained by this mechanism either. First, most conservation measures are implemented at the local, regional, or national level, and governments that wanted to implement such policies would not require the consent of other nations to do so. Therefore, we would expect many of these measures to be introduced earlier than the international trading bans negotiated through the CITES framework. Second, if these local or national policies systematically affected the endangerment level of species that later on received CITES trade bans, then we would once...
of some other human activity (e.g., construction, mining, or harvesting a different species) or being targeted for noncommercial, subsistence purposes only. Thus, a finding of a significant estimate for \( \beta \) that is of the same sign as the estimate for \( \alpha \) in the DD model (Eq. 2) strengthens the causal interpretation of our results. (As discussed earlier, because of the way IUCN records threats for terrestrial species, our classification of commercial targets is over-inclusive, i.e., it likely contains species that are targeted by subsistence hunters and trappers who do not sell specimens internationally. To the extent that this overinclusion biases our estimates for \( \beta \), the bias is toward zero and, hence, in a conservative direction.)

Last, an implicit assumption of our empirical approach is that treatment of one species does not affect the status of others (“no interference among units”). In general, ecosystems are characterized by complex interactions, and conservation measures applied of one species could have indirect effects on others, which, in turn, could cause further, higher-order effects on yet other species. For example, if a trade ban on species \( A \) increases the population of \( A \) and \( A \) preys on species \( B \) (or \( A \) and \( B \) compete for the same food source), then the population of \( B \) might decline as a result. If \( B \) is not itself a treated species, then such interactions should have a negligible effect on our results given the large overall number of control species in our dataset. However, indirect effects could bias our estimates if both \( A \) and \( B \) are both treated and are treated at approximately the same time. This potential bias is unlikely to be significant, however, as species treated at the same time have limited geographical overlap. For example, although six flying foxes received trade bans in 1990, only two have overlapping geographical distributions.

**Analysis of CITES import data**

To examine whether anticipatory spikes in trade activity can explain why the trade bans hurt reptilian species, but not mammalian species, we analyzed annual legal import quantities of Appendix I species published by CITES (see the “Explaining the difference” section in Results and Discussion).

Drawing conclusions about entire classes of species from these data is complicated for a number of reasons. First, CITES publishes trade data only for species that it regulates. Thus, for species not in any CITES Appendix before being listed in Appendix I, no pre-ban trade data are available. Second, where available, the CITES trade data often show transactions in many different unit types (weight, volume, whole specimen counts, animal parts, etc.) that cannot always be sensibly aggregated. Third, when trade activity increases before CITES bans, the specific timing of these increases is not always the same. For some species, trading activity is noticeably higher in the year of the ban. For others, trade spikes can occur 1 or 2 years before the ban. Because commercial traders whose business is the export or import of an endangered species are likely aware of whether the species is being considered for a CITES trade ban in the near future, a trade spike 1 or 2 years before a ban becoming effective could be an anticipatory effect of impending CITES action. On the other hand, trade spikes that occur, say, 5 years before a ban are less likely to have the same explanation.

To deal with these complications, we adopted the following procedure. We focused on the set of seven reptilian and eight mammalian treated species for which CITES reported legal import quantities for a period spanning 5 years before the species’ Appendix I listing and for which these data could reasonably be aggregated to whole specimens (i.e., where imports where primarily in whole

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**Fig. 5. Trend analysis—Treated species versus nontreated species.** The figure shows regression estimates for the parameters \( \xi \) in our pretreatment/posttreatment trend model (Eq. 7). Year of treatment is the omitted category (i.e., all effects are estimated relative to year 0). Models were estimated including control variables. Bands are 95% confidence intervals computed from robust SEs adjusted for clustering on species.

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again expect to see these effects reflected in systematic differences in pretreatment trends of CITES species relative to other species. As already stated, no such trend differences exist in 7 years before treatment, indicating that other conservation measures were not widely used, were ineffective, or were applied equally to treated and untreated species. Third, our analysis of ESC measures showed that, even under the assumption that these measures were introduced at the same time as the trade bans, our conclusions would remain the same. The results from the DDD model (Eq. 2) further strengthen the causal identification of the effects of trade bans on species endangerment. International trade bans are most likely to affect species threatened as a result of being intentionally targeted by humans for commercial purposes, instead of either being the “collateral damage”
specimens or whole specimen parts such as skins). To identify 11th-hour trade spikes, we focused on local peaks in trading activity that occur either in the year of a respective ban or up to 2 years prior. For each treated species examined, we first identified the year with the largest recorded import quantity within that time window and then shifted the import data series for each species to align the species-specific local peaks. Last, we added the imports across species in each year. The result of this procedure is in Fig. 3, which was discussed in Results and Discussion already.

Supplementary Materials

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