# International Environmental Agreements and Imperfect Enforcement: Evidence from CITES[1]

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**Abstract**

International environmental agreements address global environmental problems such as the decline in biodiversity. The Convention on International Trade in Endangered Species of Wild Fauna and Flora (CITES) regulates international trade in wildlife to prevent its decline. Discussions about CITES' effectiveness abound, but evidence is lacking. We combine the largest available panel database on wildlife populations with the history of countries' membership and species' inclusion into CITES' protection. We find that wildlife populations increase by 20% after the species' inclusion. This effect is driven by populations in countries with thorough enforcement, irrespective of whether the species' trade is only restricted or completely banned under CITES. Our results suggest re-focusing discussions from whether CITES should partially restrict trade or impose a complete trade ban towards better enforcement. More generally, we find that enforcement is crucial for effective international environmental agreements.

**JEL Classification Codes:** F18, Q27, Q56

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# 1 Introduction

Global environmental problems such as the decline in wildlife and biodiversity have given rise to international environmental agreements (IEAs). To avoid extinction of endangered species, the Convention on International Trade in Endangered Species of Wild Fauna and Flora (CITES) regulates international wildlife trade. IEAs often remain vague in their implications for member countries. In contrast, CITES has established a system of export and import permits to implement its trade restrictions or bans in species under its protection. Whether CITES is effective in reaching its ultimate goal, i.e., preventing the decline of wildlife, is unclear.[1] One reason for scepticism is that enforcing CITES is difficult: It targets numerous species and its detailed regulations have to be implemented and enforced by national authorities across all member countries. CITES' regulations impose costs on wildlife trade or render it illegal. With imperfect enforcement, this may drive wildlife trade from regulated to unregulated countries and from legal to illegal sources.

In this paper, we document that CITES is effective in preserving wildlife if its regulations are properly enforced. We thus provide evidence that informs the ongoing debate on whether and under which circumstances restrictions on wildlife trade are effective in protecting wildlife. On the one hand, restricting trade seems an intuitive policy measure to prevent unsustainable resource use and subsequent wildlife decline. Outright trade bans can stigmatize consumption of wildlife products, reducing their demand. Economists, on the other hand, tend to be sceptical about trade bans, as banning wildlife trade renders the legal economic value of wildlife to zero, reducing the incentive for local communities to protect or harvest resources at sustainable levels. Without costly monitoring and enforcement, poaching and illegal trade may replace legal trade, rendering trade bans ineffective. Summarizing the literature on wildlife trade restrictions, Fischer (2010) calls for an empirical evaluation of their effectiveness in preventing wildlife decline.[2] Similarly, 't Sas-Rolfes et al. (2019) highlight the need for

[1] For different viewpoints concerning the effectiveness of CITES, see, e.g., Hutton and Dickson (2000); Ginsberg (2002); Bulte et al. (2004); Fischer (2010); Challender et al. (2015).

[2] So do Bulte and Barbier (2005) in an earlier survey of the effects of trade liberal-

evaluating the effectiveness of specific trade policy interventions in a recent survey on illegal wildlife trade. Our paper provides such an evaluation using the history of species' listings in CITES as well as of countries' CITES membership, and documents the importance of enforcement for effective IEAs.

A naive test of CITES' effectiveness would compare the size of wildlife populations of species listed in CITES with non-listed species. A challenge in interpreting this means comparison causally is the endogeneous selection of individual wildlife species into CITES. For example, CITES listed species may be more likely to become extinct. Then, comparing population sizes of CITES listed and non-listed species would deliver biased results. Other confounding factors may correlate with both wildlife decline and CITES' listing decisions: Commercially valuable species may be less likely to see their international trade restricted or banned (Stokstad, 2010). Moreover, CITES' listings may be more likely for well-known, charismatic species, reflecting a more general "taxonomic bias" in wildlife conservation efforts (Clark and May, 2002).

Our paper overcomes these challenges by using a geo-referenced unbalanced panel of from 1950 to 2015, the largest publicly available database on vertebrate population sizes over time. We combine these wildlife population data with the detailed history of species' inclusion into CITES and with data from the International Union for Conservation of Nature (IUCN) Red List of Threatened Species, the world's most comprehensive inventory of species' extinction risk. Our panel data allow us to control for unobserved species' characteristics that drive the non-random selection of species into CITES. This enables us to identify the causal effect of CITES' trade restrictions on wildlife population sizes once a species gets listed in CITES, and determine whether CITES effectively prevents the decline of wildlife.

We find that wildlife populations increase after their corresponding species is listed in CITES, but with considerable lag. This result is driven by populations located in CITES' member countries with strong enforcement of its rules. Populations increase by about 20% 11 to 15 years after the species is listed in CITES, and by about 50% for species that benefit from CITES' protection for more than 20 years. Frank and Wilcove (2019)

ization on welfare and wildlife stocks.

find that, on average, species are listed in CITES more than 10 years after they have been identified as threatened by international trade. Our results, combined with this finding, highlight the importance of reducing the time lag between new scientific evidence and species' inclusion into CITES to effectively prevent wildlife decline.

We explore whether populations of different types of species are affected differently after their CITES' listing. Species that are intentionally harvested, vulnerable species with high extinction risk, highly-studied species, well-known species, and large species, i.e., with a large body mass, do not seem to benefit differently from CITES protection in countries with strong enforcement. While CITES does not effectively protect populations of listed species in member countries with weak enforcement in general, it is effective for large species even when enforcement is weak. One reason for this may be that large species such as elephants, rhinos, or whales (and their derived products) are more readily recognizable by enforcing agencies such as customs, making the enforcement of CITES' regulations for these species easier. It may also reflect that these species are particularly salient for the international community and member countries with weak enforcement therefore focus their efforts on these species.

Our results are robust to controlling for time-varying country-specific confounding factors that may affect both CITES' listing decisions and wildlife decline such as habitat loss, corruption, or armed conflicts.

We explore the two main mechanisms through which species benefit from CITES' protection: 1.) outright trade bans or 2.) more nuanced trade restrictions that are meant to ensure the sustainable use of species. This allows us to quantify the relative effectiveness of these mechanisms, which are represented by CITES' two main appendices: Species listed in Appendix I are not allowed to be traded internationally for commercial purposes. Species listed in Appendix II may be traded internationally but their trade is restricted to ensure that trade is sustainable and does not threaten a species' survival. Our results show that wildlife populations increase by a similar magnitude for species in both Appendix I and Appendix II. Contrary to views that champion either international wildlife trade bans or sustainable use of wildlife, these results corroborate the importance of enforcement for effective wildlife protection (see, e.g., Smith et al., 2003b).

Our paper relates to various strands of the literature. A broad theoretical literature discusses the circumstances under which trade restrictions can alleviate or exacerbate the overharvesting of renewable resources. Whether trade bans are effective in protecting wildlife populations depends on specific modeling assumptions such as whether wildlife trade is imperfectly competitive, whether species can be illegally traded or stockpiled, whether legal trade enables the possibility of laundering of poached specimens, and how the behavior of the regulator is modeled, e.g., if it sells or destroys seized specimens of poached species. Much of this theoretical literature focuses on the ivory trade ban, see, e.g., Khanna and Harford (1996), Bulte and van Kooten (1999), Burton (1999), Fischer (2004), Heltberg (2001), and Kremer and Morcom (2000). Copeland and Taylor (2009) stress the importance of country-specific institutional factors for successful management of common property resources such as a government's ability to enforce wildlife trade regulations. Our results provide empirical evidence on the importance of countries' enforcement capability for CITES' effectiveness.

Several studies quantify the effectiveness of other IEAs. Aichele and Felbermayr (2012) study whether the Kyoto Protocol, which attempts to reduce carbon dioxide emissions of its member countries, has led to a reduction in countries' carbon footprint, i.e., the emissions embodied in domestic consumption and investment. They find that the Kyoto Protocol has been ineffective, as it has not reduced global emissions. Kellenberg and Levinson (2014) analyze the effectiveness of the Basel Convention, which intends to reduce the generation of hazardous waste by restricting its shipment to countries with inadequate environmental regulation. They find no evidence of a reduction in the overall level of international trade in waste. Contrary to the evidence in this literature, we find that CITES is effective in its goal of preventing wildlife decline. More generally, our paper highlights that international cooperation helps to prevent environmental degradation caused by global threats such as international wildlife trade.

Our paper also relates to the literature that analyzes the effects of domestic regulations concerning endangered species. A large part of this literature focuses on the U.S. Endangered Species Act (ESA). Similar to CITES, ESA's protection relies on listing endangered species. ESA implements CITES' regulations in domestic law, but has more far-reaching powers. Whereas CITES only regulates international wildlife trade, ESA protects species by effectively preventing any economic development of areas with populations of listed species, see the survey by Brown and Shogren (1998). Metrick and Weitzman (1996) document that species' characteristics determine the probability of receiving protection by ESA: Charismatic species, particularly large mammals, the so-called "charismatic megafauna", are more likely to be protected, highlighting the importance of the non-random selection of species. Ferraro et al. (2007) evaluate the effectiveness of ESA's listings by studying their impact on the change in a species' endangerment status between 1993 and 2004 using 430 species from the US. Similar to our results for CITES, they find that enforcement is crucial for ESA's effectiveness. Ando and Langpap (2018) provide a survey on empirical studies of ESA's effectiveness, as well as on similar regulations in Australia and Canada. The literature surveyed finds only little evidence for the effectiveness of domestic regulations that intend to protect endangered species. Our study identifies a positive effect of international wildlife trade regulation on populations using data from 185 countries over 66 years.

We also relate to a literature that empirically analyzes the consequences of international trade bans using case studies of individual species. Hsiang and Sekar (2016) study the effect of a temporary removal of the trade ban for ivory for a one-off international legal sale. Using an unbalanced panel of illegal elephant killings across 38 countries, they find that the temporary removal of the trade ban led to an increase in elephant poaching. Chimeli and Soares (2017) study the effects of the introduction of a ban on mahogany exports in Brazil in 2001. They find that illegal exports of mahogany increase after the introduction of the trade ban and decrease with improved monitoring and enforcement of trade bans. Taylor (2011) documents that international trade and the absence of trade restrictions in wildlife products explain the virtual extinction of the North American bison. Complementing these studies of individual species, we study the impact of CITES' effectiveness for more than 3000 species.

More broadly, our paper relates to the literature on environmental effects of international trade, see the review by Cherniwchan et al. (2017). This literature focuses mostly on local pollution and global emission effects of changes in trade policies for manufacturing goods, whereas we focus on the effects of wildlife trade policy on global wildlife.

The rest of the paper is organized as follows. Section 2 provides institutional background and describes the data. Section 3 describes our identification strategy. Section 4 presents results. Section 5 concludes.

# 2 Institutional background and data

## 2.1 Wildlife protection under CITES

CITES is the international environmental agreement that regulates wildlife trade in endangered species.[3] Essentially, it is a multilateral trade agreement, as it aims at ensuring species' survival by prohibiting or regulating international wildlife trade and the commercial use of wildlife and its products. Species covered by CITES are listed in two appendices, according to their degree of protection. International commercial trade in species listed in Appendix I is prohibited, but may be allowed for species listed in Appendix II if it does not endanger the survival of the species.[4]

With its entry into force in 1975, CITES protected a large number of species. In subsequent years, species were included into CITES at one of the Conferences of the Parties (CoPs), the meeting of representatives of CITES' member countries. Today, about 3664 vertebrate species are listed in any of CITES' appendices.[5] We present the distribution of entry years of the species in our data in Figure 1. The majority of species were first listed into CITES by the mid-80s, and since then the inclusion of species

[3]For an in-depth description of CITES, see Favre (1989), for more recent overviews see Hutton and Dickson (2000); Ginsberg (2002); Reeve (2006); Challender et al. (2015).

[4]In addition to the multilaterally agreed upon species listed in Appendices I and II, CITES also gives member countries the right to unilaterally list species in Appendix III. Countries can only list species in this appendix if they are not already included in Appendices I and II, if they are native to the respective country, and when the country has passed domestic regulation to prevent or restrict exploitation of the species and to control international trade. Populations of Appendix III species are only protected in their respective country, whereas Appendix I and II species are protected in all member countries. Our analysis therefore focuses on multilaterally protected species (i.e., Appendices I and II). We explore the robustness of our results to the effect of unilateral, domestic regulations reflected in Appendix III in Section 4.6.

[5]Own calculation based on data from `https://cites.org/eng/disc/species.php`, accessed 18/05/2022.

has slowed down.[6]

Once a species is listed, CITES monitors its international trade via a system of export permits (for Appendix I and II) and import permits (for Appendix I). CITES member countries are expected to control all international trade in species listed in CITES, even imports of species from non-member countries. We show the evolution of the number of member countries over time in Figure 2. Today, CITES' membership is almost universal, with 184 country members. This is more than the 164 signatories of the World Trade Organization agreements that are the basis of the multilateral trade system.

Implementing and enforcing CITES is crucial for its success. As part of the "National Legislation Project" (NLP), CITES oversees the implementation of CITES' regulations into domestic legislation in the member states and classifies countries accordingly. Using information and recommendations provided by the CITES Secretariat, countries are classified by CITES' Standing Committee.[7] The classification is based on four criteria that assess a country's legislation, as set out in Conf. 8.4 (Rev. CoP15) "National laws for implementation of the Convention": First, that the country has designated, at least, one management authority and one scientific authority. Second, that the country prohibits trade in specimens in violation of CITES. Third, that the country penalizes such trade; and, finally, that the country confiscates specimens illegally traded or possessed. Countries are then classified as Category 1 (those that have legislation that meet all four requirements for effective implementation of CITES), Category 2 (those countries that have legislation that is believed generally to meet one to three of the four requirements for effective implementation of CITES), and Category 3 (those that have legislation that is believed generally not to

[6] 242 species in our data were included since CITES' inception in 1975. Overall, 569 species in our data were included into CITES at some point in time.

[7] The Standing Committee consists of an elected group of member countries, representing the different world regions. The Standing Committee was created in 1979, reflecting the recognition by CITES member countries of the need of a coordinating body for CITES in between the biannual CoPs. For the current mandate of the Standing Committee as well as a description of how CITES member countries are elected to become Standing Committee members, see Conf. 18.2 "Establishment of Committees". For a detailed history of the Standing Committee as well as its pre-1979 predecessor, the Steering Committee, see Wijnstekers (2011), chapter 25.

Figure 1: Distribution of year of first entry into CITES (species)

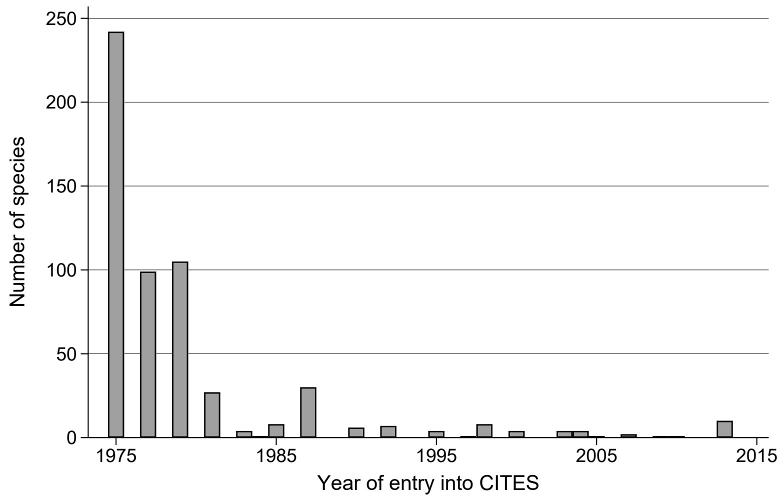



Figure depicts the distribution of the first year a species entered into one of CITES' appendices in our data.

meet any of the four requirements for effective implementation of CITES).[8] As implementing CITES into national legislation is a prerequisite for enforcing CITES, we take these categories as indicators of the quality of countries' enforcement and compliance procedures. In our analysis, we will make use of the variation across species, time, member countries, as well as their quality of enforcement, to identify the effect of CITES on wildlife.

## 2.2 Listing decisions

We provide details of the process of listing a species in one of CITES' appendices that are relevant for our identification strategy.

CITES' Article XV lays out the procedure for making amendments to Appendices I and II. Any member country can, at any time, propose a species to be included in any of the two appendices by sending a proposal to the CITES Secretariat in Geneva. The member country does not have to be a range country, i.e., there is no requirement that a species is native to the proposing country. In practice, proposals include a supporting statement which should provide both biological and trade data concerning

[8]This description is the language used by the Standing Committee to distinguish the country classifications, see, e.g., page 2 in CoP18 Doc. 26, Annex 3 (Rev. 1).

Figure 2: Year of entry into CITES (countries)

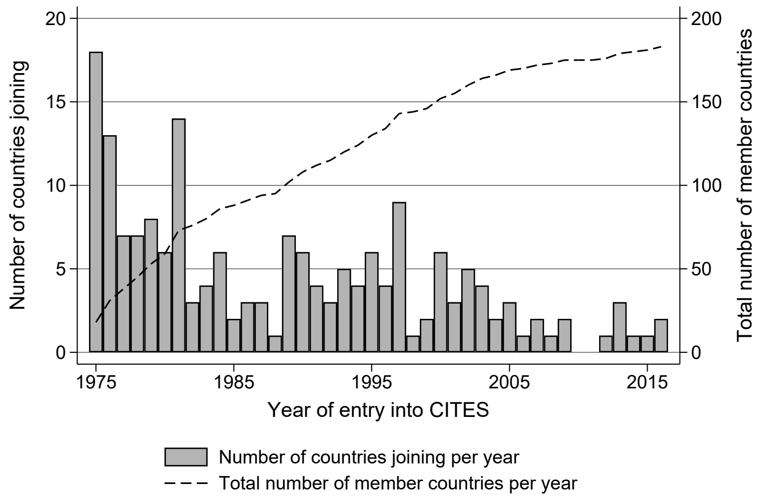



Figure depicts number of countries in which CITES entered into force per year.

the proposed taxon.

Member countries then vote at the next CoP whether the amendment should be adopted. To allow enough time for the Secretariat to communicate the proposal to all member countries, and to allow it and the member countries to gather relevant information, proposals have to be communicated to the Secretariat at least 150 days before the next CoP. While CITES stipulates CoPs to take place at least every two years, since 2004, three years have passed between two CoPs.[9] At a CoP, a proposal is accepted if at least a two-thirds majority of the members present and voting is reached, where every member country has one vote.

A particular feature of CITES' listing procedures helps with our identification strategy: Decisions separate the fact-finding ("Is a species endangered by international trade?") from the decision-making ("Should the species be included into Appendix I or II?"). Once the Secretariat receives a proposal, it has to gather information about the status of the species. According to Article XV(1)(a), the Secretariat has to formulate a recommendation as to whether the species should be included into Appendix I or II or not, and this recommendation has to be communicated to the mem-

[9]CITES also allows for a postal vote in between CoPs, but this procedure is hardly used in practice, see Favre (1989).

ber countries at least 30 days before the CoP. As described in Gehring and Ruffing (2008), this separation of the recommendation during the scientific assessment stage from the voting stage creates incentives for the Secretariat to provide a truthful evaluation about the facts concerning a proposed listing. Crucially, the recommendation has to be based on available evidence in line with the listing criteria *all* member countries have agreed upon and apply to *all* species.[10]

This restricts the possibility for individual member countries to make "deals", particularly as the Secretariat's recommendation and the evidence on which it is based is made available to all member countries. In addition, at any CoP, decisions are made on a large number of listing proposals.[11] The majority of member countries are not range countries for any given species considered in a listing decision, hence they are less likely to have an economic interest in a species. In practice, the majority of listing decisions are unanimous, see Favre (1989), and less than 10% of listing decisions are so contentious that voting is done by secret ballot, see Blundell and Rodan (2001). Consensus-based public voting likely reduces the possibility of "package deals" of trading favors between members, as any country that wishes to influence a particular listing decision would have to convince a large number of members that all would have to be willing to display their vote against the recommendation of the Secretariat in public. These aspects make it difficult for any one country to influence a particular listing decision, and in general, in the words of Gehring and Ruffing (2008), in CITES, "arguments prevail over power".

Still, if scientific evidence is scant, or for species where the decision is more contentious, the listing process may be more prone to be influenced by country-specific interests. For example, in the decision on listing the Great White Shark (*Carcharodon carcharias*), the available evidence prior to the listing decision was scant, and hence the recommendation of the Secretariat was formulated in a cautious way, see Gehring and Ruffing

[10] Listing criteria consider biological as well as trade-related factors of species, see for a detailed description Conf. 9.24 (Rev. CoP17) "Criteria for Amendment of Appendices I and II".

[11] For example, at the last CoP18 in 2019, 57 proposals were considered (see CoP18 Doc. 105.1 Annex 2). Favre (1989) reports between 50 to 100 proposals per CoP; Gehring and Ruffing (2008) report 30 to 50 adopted listing decisions per CoP.

(2008). We discuss this threat to our identification strategy in more detail in Section 3. Our data allow us to create a measure of the availability of scientific evidence at the time of inclusion, mitigating this threat. In addition, we exclude species for which listing decisions are known to be particularly contentious in a subsample analysis in Section 4.1.

Further, the CITES listing process has been criticized for over-representing certain types of species in its appendices, particularly charismatic species, see, e.g., Webb (2000). We document that this is the case in our data in Section 3. We therefore control for differences across species in their probability of being listed by inclusion of species-location (i.e., population) fixed effects in all our regressions.

Finally, the time lag between the emergence of scientific evidence and the listing of a species is considerable: Frank and Wilcove (2019) document that on average, more than ten years pass from an IUCN Red List assessment to a CITES inclusion. At the same time, Rivalan et al. (2007) show that anticipation effects in terms of higher trade volumes in soon-to-be-listed species only materialize in the immediate year before the listing of a species. Our event study specification controls for potential anticipation effects via the inclusion of respective leads.

## 2.3 Data

Our focus is measuring CITES' effectiveness in terms of its ultimate goal, the prevention of wildlife decline. We therefore use wildlife population size as our dependent variable.

*Wildlife population size data.*—We focus on the evolution of the population size of a (sub-)species $s$ at a geographic location $l$ at time $t$, i.e., population refers to the tuple $(s, l)$. We use the raw population data that are used to construct the Living Planet Index (LPI) by World Wildlife Fund (2016).[12] These data are the largest publicly-available database providing information on wildlife population sizes over time and are routinely used to monitor the progress of biodiversity conservation targets (see, e.g.,

[12] For a more detailed description of the raw data, see Loh et al. (2005) and Collen et al. (2009). The data can be downloaded from `http://www.livingplanetindex.org/projects?main\_page\_project=LivingPlanetReport\&home\_flag=1` (downloaded 10 January 2017).

Butchart et al., 2010 and Tittensor et al., 2014).[13]

The LPI project was created by the World Wildlife Fund (WWF) in 1997 as a global, comprehensive, and representative database that allows to measure the evolution of population sizes of vertebrate species over time. Today, the Zoological Society of London is managing the LPI data in collaboration with the WWF. Hence, the LPI data are collected independently of CITES, and CITES listings are determined independently from the data collection efforts of the LPI. To be included into the LPI database, a population has to be observed for at least two years using the same methodology. The population data have to be referenced and traceable, i.e., for every population in our sample, LPI reports the exact reference for the population data. The main source of population data are peer-reviewed academic journals (36% of all sources). Other sources, in declining importance, are secondary sources (33%), other sources (15%), government reports (10%), and unpublished reports (6%).[14]

Population size data are ideal for our purpose as they react more quickly to changes in wildlife protection than data on species' endangerment status and whether species are (close to becoming) extinct. Extinction is a long-run process and it can be difficult to determine whether a species is actually extinct.[15] Declines in population sizes are directly linked to reduced ecosystem services, e.g., for fisheries and agricultural production. Particularly, wildlife populations can become so small that they are functionally extinct, i.e., they cease to provide economic benefits, even when complete

[13] Note that wildlife trade data for species included in the LPI raw data are not available. Available databases such as the CITES Trade Database only contain trade in species listed in CITES, preventing before-and-after comparisons of species becoming listed in CITES. Furthermore, the CITES Trade Database is derived from the number of import and export permits CITES' members submit through annual reports. These data are measured with considerable error, and no clear rules exist on how to calculate trade volumes from the underlying records on permits, see, e.g., Berec et al. (2018). As pointed out by Chan et al. (2015), standard merchandise trade classifications such as the Harmonized System do not distinguish trade in individual species. Even if trade data were available, many species, including those that are regulated under CITES, are traded illegally and hence their trade is not documented, see, e.g., 't Sas-Rolfes et al. (2019).

[14] Information in this paragraph is based on the supporting documents provided on the LPI website, `www.livingplanetindex.org`, as well as Loh et al. (2005).

[15] For a discussion of these issues, see, e.g., Ceballos and Ehrlich (2002) and Butchart et al. (2006).

extinction can be avoided, see Sekercioglu et al. (2004).[16]

Population size data in the LPI are unbalanced and not necessarily available for consecutive years. For example, the population of Cape vultures (*Gyps coprotheres*) in Namibia is only observed for the years 1975, 1980, 1990, and 2000. Hence we cannot calculate annual (log) growth rates for all populations. In our empirical analysis, we therefore use a within estimator instead of a first difference transformation to control for unobserved population fixed effects. This allows us to incorporate the information from those populations whose size is not observed every year.

The LPI data report population size in a variety of units, depending on the study from which the raw data are collected: Population sizes may be simple counts of individuals in a given geographic location, or the number of breeding pairs; sometimes, population size is measured as the amount of biomass in a population, i.e., in kilograms, or as the number of individuals per a given area. In our empirical analysis, we use a log-linear regression with species-location (i.e., population) fixed effects that control for these differences in units of measurements across the different populations.[17] This strategy also corrects for potential population-specific mea-

[16]In our final dataset, 4003 observations report a population size of zero, about 3% of our final dataset. Note that population sizes of 0 do not necessarily imply that a population has gone extinct, as population size data are measured with considerable measurement error, see, e.g., Meir and Fagan (2000), and the recent upward revisions of population size data for *Gorilla gorilla gorilla* by Strindberg et al. (2018). In our dataset, of the 1226 populations which record a zero population size in one year, 87 percent report a non-zero population size afterwards. We therefore assume that zeros are due to random measurement error. If measurement errors are specific to certain populations, the fixed effects we include in our regressions will control for this.

[17]By way of illustration, imagine that there are just two different measurement units used, individuals and pairs. In this case, the difference in measurement units is a factor of 2. By multiplying our dependent variable by this factor for all observations measured in pairs, we can transform all observations measured in pairs to individuals. This factor is constant over time, as the measurement unit for a population does not change over time in our dataset, and is not affected by CITES. It is hence perfectly captured by a population fixed effect $\mu_{sl}$. More generally, all observations can be converted to the same unit of measurement by multiplying by $b_{sl}$, a population-specific, time-invariant scale factor ($N_{slt}^{same\ unit} = b_{sl} N_{slt}$). Taking the natural logarithm, this becomes ($\ln N_{slt}^{same\ unit} = \ln b_{sl} + \ln N_{slt}$). Hence, including a population-specific fixed effect in combination with using the dependent variable in logs controls for the different units of measurement problem and we can then interpret regression coefficients in the usual way, as a semi-elasticity that is independent of the unit of measurement. This also holds for measures of a species' population density in our sample, as the relationship between population density and biomass is stable over time and log-linear, so-called Damuth's rule, see Damuth (1981) and White et al. (2007).

surement errors.[18]

While the LPI data are arguably the largest available global database on vertebrate population data over time, one may be concerned that the LPI data overrepresent data of species with declining populations. Collen et al. (2009), who describe the LPI raw data in detail, investigate whether the LPI raw data overrepresent data with particular patterns of time trends. They argue that data sets with declining populations may be published more quickly because these data may be helpful to argue for more conservation efforts. Collen et al. (2009) test whether there is a difference in the publication date of data between declining and non-declining populations, but they do not find any significant difference. Similarly, studying whether endangered species are overrepresented in the LPI data, Loh et al. (2005) conclude that this "does not occur to any marked extent" (p. 295).

Also, the LPI data may overrepresent charismatic species, i.e., the LPI data itself may suffer from taxonomic bias. Clark and May (2002) document that scientific studies suffer from taxonomic bias. As the LPI data are based on scientific studies, this bias applies to the data as well. However, Clark and May (2002) also document that the taxonomic bias does not seem to change over time. Whether charisma of species changes over time is an under-researched topic, but in a pioneering study, Monsarrat

[18]Classical measurement error in our dependent variable, i.e., population size, does not lead to inconsistently estimated regression coefficients. However, a form of non-classical measurement error in our dependent variable may occur if endangered species' populations are harder to measure. For these species, the variance of the measurement error is larger, i.e., the measurement fluctuates more, potentially in combination with systematic over- or undercounting, i.e., a non-zero average measurement error. Including a constant (or population fixed effects) in the regression allows for a non-zero mean in the dependent variable, and, conditional on the regressors, a non-zero error term. Hence, a non-zero average measurement error for such species is accounted for by the population fixed effects. The differences in variance imply heteroskedasticity. We will cluster standard errors at the species-level, allowing for arbitrary correlation of the error terms across populations of the same species and thus account for this heteroskedasticity. Another form of non-classical measurement error may occur if more endangered populations are measured with a larger measurement error, i.e., there is a negative correlation between the size of the population and the size of the measurement error. A simple way to model this is to assume that instead of the true dependent variable $Y_p$, we measure $\tilde{Y}_p = Y_p + v_p$, and the measurement error is given by $v_p = \delta Y_p + v_p^*$, where $v_p^*$ is uncorrelated with the other variables and $\delta < 0$. Bound et al. (1994) show that this leads to a downward bias in the regression coefficient. If this were the case, we would underestimate the effectiveness of CITES and our estimates would be a conservative lower bound.

and Kerley (2018) find that species considered to be of high charisma in the 16th to 19th century are generally considered to be of high charisma today too, so in our view, charisma and the according taxonomic bias can be treated as time-invariant drivers of population sizes and CITES listings, such that they are controlled by the population fixed effects we include in all our regressions.

*CITES data.*—We combine the wildlife population size data with information about which species are listed in CITES' Appendices I and II from the *Checklist of CITES Species* by UNEP-WCMC (2017). We get the year in which countries became CITES members from the CITES Secretariat website.[19] We use the CITES classification of Category 1 members to identify countries with high levels of enforcement and compliance procedures from the CITES official document "Status of Legislative Progress for Implementing CITES", CoP17 Doc. 22 Annex 3 (Rev. 1). We create an indicator variable that identifies populations in Category 1 CITES member countries.

*CITES' sanctions data.*—CITES allows to impose sanctions on countries that are not compliant with CITES' regulations. If a country is sanctioned, all commercial trade in CITES-listed species is suspended. Sanctions are indicative of a lack of enforcement of CITES. It is likely that CITES is not effective in sanctioned countries. In a subsample analysis, we therefore exclude all populations in sanctioned countries for those years where the sanctions are applied. To do so, we rely on the historical data on suspensions of all commercial trade in CITES-listed species that is available from Sand (2013). Sand lists sanctions for the period 1985-2013. We update sanctions data until 2016 by using the information provided on the CITES webpage regarding "Countries currently subject to a recommendation to suspend trade".[20]

*Corruption data.*—CITES being an international trade agreement, its rules have to be implemented by national governments and enforced by cus-

[19] The "List of Contracting Parties" is available at `https://www.cites.org/eng/disc/parties/chronolo.php`.

[20] This information is available at `https://www.cites.org/eng/resources/ref/suspend.php`, but is updated with frequency, hence countries that are no longer subject to a recommendation to suspend trade, are removed from the list. We update the sanctions data with the help of the *Wayback Machine - Internet Archive*.

toms officials. In countries with high levels of customs corruption, CITES may therefore be less effective. As a proxy for corruption at the border, we use the share of a country's population that answered "yes" to the question "in the last 12 months anyone living in a household paid a bribe in any form to customs" (variable "Paid Bribe: Customs") in the Global Corruption Barometer by Transparency International. As an alternative, more general measure of corruption in a country, we use the World Bank's control of corruption indicator by Kaufmann et al. (2010). This indicator is standardized to have a mean of zero and a standard deviation of one, with higher values indicating more control of corruption (i.e., less corruption). We create an indicator variable that is one if a country has a below average level of corruption, and zero otherwise. We use both corruption measures as reported in the Quality of Government Basic Dataset (version Jan17) by Dahlberg et al. (2017).

*GDP per capita data.*—We estimate separate treatment effects for populations in high-income countries versus populations in other countries, as the effectiveness of CITES may be conditioned by a country's income level. We use GDP per capita data in purchasing power parities in 2011 US$ from the updated Maddison project by Bolt and Luiten Van Zanden (2020). We define a population to be located in a high-income country when its GDP per capita in that year is within the top 25% of countries by GDP per capita in the panel of countries from 1950 to 2015.

*IUCN Red List data.*—We estimate separate species-type specific treatment effects because CITES may be more or less effective for different types of species. For example, the protection offered by CITES may be more effective for species with commercial value (or species with "intentional use"), as it may prevent overharvesting. We use data on intentional use from the IUCN-CMP Unified Classification of Direct Threats (version 3.2), which is a refined version of the classification introduced by Salafsky et al. (2008).[21] We also estimate a separate effect for species that are vulnerable. We use the IUCN Red List of Threatened Species classification on extinction risk and consider as vulnerable all species that are classified as "critically endangered", "endangered", and "vulnerable", i.e., species facing extremely or

[21] We downloaded these data from the IUCN Red List API-v3 (`http://apiv3.iucnredlist.org/api/v3/docs`) on 20 February 2019.

very high risk of extinction in the wild.[22]

*Citizen science data (iNaturalist).*—We estimate a separate treatment effect for well-known species. To identify these species, we use citizen science data on users' identifications for species from *iNaturalist*.[23] This database contains information about exemplars of species identified by the community, mostly by photos, going back to 1970. Today, users can upload their photos via a smartphone app.

*Species traits data.*—We estimate a separate treatment effect for large species, i.e., species with larger than average body mass. We use data from the EltonTraits 1.0 dataset, a species-level compilation from various sources of species' attributes of birds and mammals by Wilman et al. (2014).[24]

# 3 Research design and identification

Our goal is to estimate the causal effect of CITES' listings on the size of species' populations. To inform our identification strategy, we start by visualizing the average trends in our population data. Figure 3 shows the evolution of the average population size for species that are listed in CITES at some point of time within our sample period and those which are not, respectively, i.e., the graph does *not* take into account that different species enter in different years. The figure shows predicted log population size per year for these two groups. To calculate predicted population sizes, we run a regression of log population sizes on a population fixed effect to control for the difference in measurement units, and different year effects for CITES listed and non-listed (never listed) species. Figure 3 shows the average of the predicted values from this regression excluding the population fixed effect to ensure that we use the same measurement unit for all observations. Species protected by CITES have smaller populations before CITES entered into force in 1975, i.e., there are pre-existing differences in listed and non-listed species which we will control by the inclusion of popula-

[22] We downloaded these data from the IUCN Red List API-v3 on 8 November 2017.

[23] Data downloaded from the *iNaturalist* webpage `https://www.inaturalist.org/home`. Data downloaded are for taxa on amphibians, birds, fishes, mammals, and reptiles. Data downloaded on 12 and 13 November 2019.

[24] The main sources for the body mass data are Smith et al. (2003a) for mammals and Dunning (2007) for birds.

tion fixed effects in our regressions below. The figure also shows a dashed line with the evolution of the difference in population size between listed and non-listed species. This line allows a coarse comparison of the relative pre-existing trends for the period before any species are treated, i.e, before 1975. We see that population size seems to move on parallel trends before CITES entered into force.[25] After 1975, species start to get listed in CITES' appendices and trends start to diverge.

The intuition derived from eyeballing Figure 3 is also borne out when using a formal statistical test for equality of trends following the approach by Antwi et al. (2013). As species start to be included into CITES with its entry into force in 1975, we only use data of populations observed before 1975 for this test ($N = 14540$). We then regress population size on population (species-location) and year fixed effects, and an interaction term of the linear time trend with $EVERCITES_s$, $EVERCITES_s t$, where $EVERCITES_s$ indicates whether species $s$ is part of the treatment group, i.e., a species included into CITES' appendices at some point in time in our dataset. There is no difference in the trends of treatment and control groups prior to the entry into force of CITES.[26]

One of the reasons for pre-existing differences between listed and non-listed species is that the probability of being listed in CITES is different across species. Metrick and Weitzman (1996) document that different types of species have different probabilities of getting listed in the U.S. Endangered Species Act. We confirm their result for CITES. In Appendix A, we show that large mammals (the so-called "charismatic megafauna"), vulnerable species, and species used intentionally have a higher probability of being listed in CITES. Similarly, we find higher probabilities of getting listed for mammals, birds, and reptiles than for fishes. This provides evidence of a selection bias driven by species' time-invariant characteristics.

The probability of a species getting listed may change over time because new scientific evidence on the status of a species becomes known. For every population in our data, the LPI data reference the source of the population

[25] Note that the number of observations included in the "treated" group is considerably lower than the number of observations included in the "control" group, which explains the larger variance in the average population size for listed species.

[26] The estimated coefficient for $EVERCITES_s t$ is $-0.001$ ($s.e. = 0.013$, $p$-value $= 0.951$), the 95% confidence interval is [-0.026; 0.025].

Figure 3: Average population size: Listed and never listed species by year

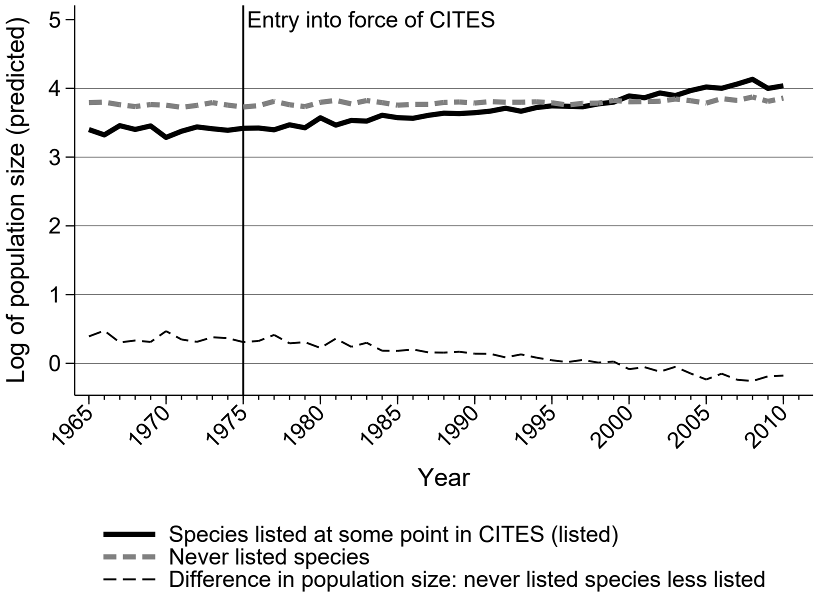



Figure depicts the (predicted) population size per year for species that have ever been listed in CITES versus species that have never been listed in CITES.

size data in the scientific literature. This allows us to construct a balanced panel dataset for all species for the years 1950 to 2015 where we count the accumulated number of studies available in a given year for a specific species, $ACCUMULATEDSTUDIES_{st}$. We run the following regression:

$$CITES_{st} = \alpha_s + \beta ACCUMULATEDSTUDIES_{st} + \delta t + \varepsilon_{st}, \qquad (1)$$

where $CITES_{st}$ is an indicator variable which is one if species $s$ is listed in CITES in year $t$ and zero otherwise, $\delta t$ is a time trend, and $\alpha_s$ is a species fixed effect that controls for all time-invariant species characteristics influencing the selection into CITES as documented in Appendix A.[27] We cluster standard errors at the species level. We present results in Table 1.

In column (1), we start with estimating a restricted version of Equation (1) by dropping $ACCUMULATEDSTUDIES_{st}$ and the species fixed effect. We find a significant time trend, with the probability of being listed

[27]Changes in $ACCUMULATEDSTUDIES_{st}$ may also be interpreted as a proxy for changes in the unobserved endangerment status of a particular species. Then, the coefficient $\beta$ should be biased towards zero when assuming a classical measurement error.

Table 1: Determinants of CITES listings (panel)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| trend | 0.003 | 0.003 | 0.003 | 0.003 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| accumulated number of studies available | | 0.005 | | −0.000 |
| | | (0.003) | | (0.001) |
| $R^2$ | 0.05 | 0.05 | 0.58 | 0.58 |
| $N$ | 228162 | 228162 | 228162 | 228162 |

*Notes*: Table 1 reports estimated coefficients from a panel linear probability model of variants of Equation (1). The dependent variable is a dummy variable that equals one when the species is listed in CITES in year $t$ and zero otherwise. As regressors we use a variable that measures the accumulated number of published studies in our dataset in $t$ on a specific species and a time trend. Column (1) estimates Equation (1) but drops $ACCUMULATEDSTUDIES_{st}$ using pooled OLS, i.e., without a species fixed effect $\alpha_s$. Column (2) re-estimates Column (1) but adds $ACCUMULATEDSTUDIES_{st}$. Columns (3) and (4) re-estimate Columns (1) and (2) but add a species fixed effect. Column (4) estimates Equation (1) as presented in the main text. Standard errors are in parentheses and are clustered at the species level.

in CITES increasing by 0.3 percentage points per year. Its explanatory power is quite low as it only explains about five percent of the variation in the dependent variable. In column (2), we add the number of accumulated studies. The regressor is not significant. Accordingly, the explanatory power of the regressor is close to zero, which together with the time trend contributes still only five percent to the overall $R^2$ of the pooled regression. Columns (3) and (4) re-estimate columns (1) and (2) but add the species fixed effect. They reveal that the majority of the variation in $CITES_{st}$ is explained by the species fixed effect.[28]

These results inform our identification strategy to estimate the effect of CITES' listings on wildlife populations. Including population-specific fixed effects remedies the documented time-invariant selection bias by focusing on within-population variation for a given species. In addition, our population fixed effect eliminates the time-invariant taxonomic bias. It also controls for systematic time-invariant differences between species that have been listed early on in CITES and those that have been listed later. We control for the trend in the probability of species becoming listed by including year fixed effects. Our baseline regression is given by:

$$\ln N_{slt} \quad = \quad \mu_{sl} + \eta_t + \beta(\text{in CITES})_{st} + \varepsilon_{slt}, \qquad (2)$$

where $N_{slt}$ is the size of the population of a species $s$ in location $l$ in year

[28]In unreported regressions, we also experimented with non-linear time trends as well as including only studies published after the entry into force of CITES in 1975. Conclusions do not change.

$t$.[29] (in CITES)$_{st}$ is a dummy variable that is one when a species is listed in one of CITES' appendices in a given year, and zero otherwise. $\mu_{sl}$ is a time-invariant species-location (i.e., population) fixed effect that controls for factors such as, e.g., habitat suitability, which determine population size of a species in a given location. Even in a world without any loss of wildlife caused by human activity, species are unevenly distributed across space according to their habitat. For example, red fox (*Vulpes vulpes*) populations vary considerably across their geographical range, which is the largest within the order Carnivora, see Hoffmann and Silero-Zubiri (2016). This highlights the importance of allowing for different base levels of a species' population size in different locations. In addition, species differ in terms of both their abundance and their extinction risk due to factors such as body weight, size, attractiveness to humans, economic value and reproductive rates, see Hutton and Dickson (2000), Cardillo et al. (2005), and McClenachan et al. (2016).

$\eta_t$ is a year-specific fixed effect that controls for time-varying factors that influence treatment and control species in a similar way. Finally, $\varepsilon_{slt}$ is an error term that measures random fluctuations in population size. This regression is equivalent to a generalized differences-in-differences approach where species protected by CITES are the treatment group and the control group comprises species that are not included in the CITES appendices.[30] Our final sample includes 11054 populations of 3457 species in 185 countries over 66 years (from 1950 to 2015). We follow the suggestion of Bertrand et al. (2004) and cluster standard errors at the species level to allow for correlation within species as our treatment variable is defined at the species level.

We later relax the assumption of constant treatment effects over time by estimating the following event study specification:

$$\ln N_{slt} = \sum_{\substack{\tau\in\{-10,-5,0,5\\10,15,20,>20\}}} \beta_\tau \mathbf{1}(t = t_s^{CITES} + \tau)_{st} + \mu_{sl} + \eta_t + \varepsilon_{slt}. \quad (3)$$

[29]Note that population refers to a given species $s$ in location $l$. Hence, for a given year, there may be several species in the same location, and the same species may occur in several locations.

[30]For our estimation, we use the STATA package `reghdfe` by Correia (2016).

We regress the log of population size on a set of relative time dummies that indicate the number of years before or after a species' listing in either CITES Appendix I or II. Our interest lies in estimating the treatment effect $\beta_\tau$ on population size $\tau$ years after a species is included into CITES' appendices.[31] The set of time dummies allows the treatment effect to vary with time $\tau$ since the year a species was listed into CITES' appendices, $t_s^{CITES}$. We consider different effects for the year of inclusion ($\tau = 0$), the first five years after listing, and then, in five year intervals up to 20 years. We also consider a separate treatment effect for species listed for more than 20 years.

We estimate leading values of the treatment to test the reliability of our identification strategy. A statistical significant effect for $\tau = -10$ or $\tau = -5$ indicates pre-existing differences in the trends between listed and non-listed species, which may cast doubt on the common trend assumption underlying our approach.

As an additional test of the parallel trend assumption, we follow the recommendation by Bilinski and Hatfield (2019) and estimate a model that uses the same treatment variables as in Equation (3) but we include $EVERCITES_st$ instead of placebo treatments in the years before inclusion into CITES. Note that in this specification with a full set of treatment dummies for all years after treatment starts, $\theta EVERCITES_st$ directly measures a violation of the parallel trend assumption prior to treatment, i.e., inclusion into CITES. We estimate $\hat{\theta} = -0.002$ ($s.e. = 0.007$, $p$-value $=$ 0.818). Hence we can rule out a violation of the parallel trend assumption of trend differences larger than the bounds of the 95% confidence interval of $\hat{\theta}$, $[-0.014, 0.011]$, validating our identification strategy.[32]

[31] We write $\tau = -10$ for years 6 to 10 years before a species' CITES listing, $\tau = -5$ for years 1 to 5 before a species' CITES listing, $\tau = 0$ for the year of a species' CITES listing, $\tau = 5$ for years 1 to 5 after a species' listing into CITES, $\tau = 10$ for years 6 to 10, $\tau = 15$ for years 11 to 15, $\tau = 20$ for years 16 to 20, and $\tau > 20$ for more than 20 years after a species' CITES listing.

[32] Note that neither Figure 3 nor this test are ultimately informative about whether treated and non-treated species follow the same pre-treatment trends in our setting with variation in treatment timing. Instead, pre-treatment dummies in an event study specification are used to test this assumption, and we include these in all our event study specifications, see Equation (3). For a lucid discussion of these issues, see chapter 9 in Cunningham (2021).

# 4 Results

## 4.1 Effect of CITES listings on wildlife population sizes

*Species listed in CITES.*—We present results of Equation (2) in column (1) of Table 2. After a species is listed in CITES, the populations of this species increase by 20%.[33] Most of the species in our sample were included into CITES in 1975 (see Figure 1). There may be a difference in CITES' effectiveness between the species listed in 1975 and those that were listed later. We therefore estimate separate treatment effects for these two groups of species.[34] In column (2), we define treatment only for those species that were included into CITES in 1975. We estimate a similarly sized effect as in column (1) but with low precision. In column (3), we define treatment only for those species included after 1975. The effect is again of similar size, and is now precisely estimated. In column (4), we include both dummies simultaneously, with similar results. We cannot reject the null hypothesis that the effect for species listed in 1975 and after 1975 is the same ($p$-value = 0.754), but their effect is jointly significant ($p$-value = 0.031).

*Sanctions.*—Under Article XIV.1(a) of CITES, member countries can sanction other countries if they do not comply with CITES regulations, e.g., by not passing local legislation to implement CITES.[35] We create an indicator variable that is one if a country is not subject to sanctions in a given year (*NONSANCTIONED*). By interacting this variable with our baseline CITES treatment dummy from column (1), we can test whether CITES is less effective for populations located in sanctioned countries. We present results in column (5). We do not find a significant difference of the effectiveness of CITES between populations in sanctioned and non-sanctioned countries. However, we should interpret this result with caution: The correlation between our baseline treatment dummy and the interaction term with sanctioned countries is 0.99, therefore, our data probably do not allow us to identify a meaningful difference between populations in

[33]We calculate marginal effects of variable $k$ as $(e^{\beta_k} - 1) \times 100$.

[34]This also checks for the similarity of treatment effects for early and late treated species, in the spirit of Goodman-Bacon (2021). Note that the decomposition of difference-in-differences estimates in balanced panels proposed by Goodman-Bacon (2021) does not apply in our unbalanced panel, see Baker et al. (2022).

[35]For an overview of CITES' sanction regime, see Sand (2013).

Table 2: Effect of CITES on population size (species listed in CITES)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| in CITES | 0.184 | | | | 0.242 | −1.281 | −0.146 | 0.008 | 0.082 | 0.061 | −0.139 |
| | (0.071) | | | | (0.132) | (0.325) | (0.105) | (0.091) | (0.104) | (0.106) | (0.172) |
| in CITES in 1975 | | 0.207 | | 0.217 | | | | | | | |
| | | (0.122) | | (0.122) | | | | | | | |
| in CITES after 1975 | | | 0.164 | 0.170 | | | | | | | |
| | | | (0.081) | (0.082) | | | | | | | |
| in CITES × *NONSANCTIONED* | | | | | −0.058 | | | | | | −0.051 |
| | | | | | (0.114) | | | | | | (0.117) |
| in CITES $\times(1-P(BRIBE{=}1))$ | | | | | | 1.700 | | | | | |
| | | | | | | (0.393) | | | | | |
| in CITES $\times(1-CORRUPT)$ | | | | | | | 0.420 | | | | 0.183 |
| | | | | | | | (0.131) | | | | (0.138) |
| in CITES × *HIGH-INCOME* | | | | | | | | 0.263 | | | 0.148 |
| | | | | | | | | (0.084) | | | (0.090) |
| in CITES × *MEMBER* | | | | | | | | | 0.122 | −0.199 | −0.068 |
| | | | | | | | | | (0.093) | (0.133) | (0.139) |
| in CITES × *MEMBER* × *CATEGORY 1* | | | | | | | | | | 0.399 | 0.229 |
| | | | | | | | | | | (0.168) | (0.161) |
| $N$ | 119538 | 119538 | 119538 | 119538 | 119538 | 107566 | 113818 | 119538 | 119538 | 119538 | 113818 |

*Notes*: Table 2 reports estimated regression coefficients from a panel regression of log of population size on various treatment dummies along a set of population and year fixed effects. Standard errors are in parentheses and are clustered at the species level. Column (1) estimates Equation (2), i.e., a regression in which the treatment dummy equals one for populations of species listed in CITES. Column (2) includes a variation of the treatment dummy that equals one for populations of species listed in CITES in 1975. Column (3) includes a variation of the treatment dummy that equals one for species listed in CITES after 1975. Column (4) includes both treatment dummies for species listed in CITES in 1975 and after 1975. Column (5) interacts the treatment dummy from column (1) with a dummy variable that is one for populations located in non-sanctioned countries. Column (6) interacts the treatment dummy with $1-P(BRIBE{=}1)$, where $P(\cdot)$ is the probability of corruption at the border. Column (7) interacts the treatment dummy with a dummy variable that is one when a country has a low level of corruption. Column (8) interacts the treatment dummy with a dummy variable that is one for populations located in high-income countries. Column (9) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES member country. Column (10) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES Category 1 member country, i.e., a country with high levels of enforcement. Column (11) includes several regressors simultaneously.

sanctioned and non-sanctioned countries.

*Corruption.*—Countries with high levels of corruption may limit the success of conservation projects by reducing effective funding levels and distorting priorities. Empirically, there seems to be a relationship between (bad) governance and wildlife (decline) (see Smith et al., 2003b). We interact our baseline treatment dummy with a variable defined as one minus a particular measure of corruption. As our measure of corruption, we use the share of population in country $c$ answering "yes" to the question "in the last 12 months anyone living in a household paid a bribe in any form to customs" (variable "Paid Bribe: Customs", see Dahlberg et al., 2017). Hence, we interact our baseline treatment dummy with $1 - P(BRIBE{=}1)$, which allows corruption at the border to vary the "dosage" of the effectiveness of CITES. Results obtained in column (6) imply that less corruption at the border increases the effectiveness of CITES. At the mean value of $1 - P(BRIBE{=}1)$ (0.88 in our sample), we find that inclusion into CITES increases populations by (-1.281+1.700*0.88)*100=22%.

CITES not only has to be enforced by customs officials but also by other government agencies. Hence our corruption measure may be too specific and not capture the overall level of corruption in a country. Also, individuals may under-report that they have paid a bribe to avoid self-incrimination. More generally, differences in definitions of corruption may lead to important differences across corruption indicators, see Malito (2014). We therefore use the World Bank's control of corruption index to construct an indicator variable that is one when a country is characterized by low (below average) levels of corruption. We interact this indicator with our baseline treatment dummy and present results in column (7). We find a significant effect of CITES only for populations of species located in countries with low corruption, confirming the importance of good governance for CITES' effectiveness.

*Countries' income level.*—Countries with a higher income per capita may be more effective at implementing CITES due to higher public funding. In addition, with rising income levels, consumers have a higher willingness to pay for conservation, see the meta-analysis by Jacobsen and Hanley (2009). We therefore interact our baseline treatment indicator with a dummy variable that is 1 for populations located in high-income countries in a given year.[36] We present results in column (8). We find that CITES' effectiveness is concentrated on populations in high-income countries.

*CITES' member countries.*—Until now, our treatment dummy takes the same value for all populations of a given species. Some populations of protected species in our data are located in a country which was not a CITES member country at the time of observation. CITES members are expected to apply CITES regulations also to wildlife trade originating from non-member countries (see Article X of CITES). Therefore, CITES in principle should also protect species in non-member countries. If the protection of a species is only imperfectly enforced, our estimates in the previous columns can be interpreted as the intention-to-treat effect of the inclusion in CITES. Still, CITES' listings may be more effective to protect wildlife populations in member countries. We therefore map each population to the country in which it is located. This allows us to include, in addition to our baseline species-specific treatment dummy an interaction term between the treatment dummy and a dummy variable indicating whether the country in which the population is located is a member of CITES that year. We present results in column (9). Both variables are not significant individually, but are jointly significantly different from 0 ($p$-value = 0.02), and we cannot reject the hypothesis that the effect of CITES is the same for populations in and outside CITES member countries ($p$-value = 0.83). This is not surprising. Article X of CITES stipulates that trade with non-member countries is only allowed when essentially equivalent documentation, particularly export permits, are provided by any potential trader, and countries explicitly are allowed to even apply stricter standards to non-member country trade.[37]

*Member countries' implementation and enforcement.*—Column (9) may seem to imply that there are no differences between member and non-member countries of CITES. This may be due to heterogeneity within the group of CITES member countries, as some member countries may implement and enforce CITES more stringently than others. Following the previous result that CITES' listings are effectively protecting species in countries

[36]Note that not all populations are located in either high-income or non-high-income countries: In our data, populations are also found in international waters or Antarctica.

[37]For a detailed discussion of the provisions of Article X and their implementation, see Sand (2013) and Wijnstekers (2011), pp. 339-342.

that are less corrupt, we analyze whether the effectiveness of CITES might differ by a member country's implementation and enforcement level. If a country is not implementing or enforcing CITES regulations properly, we should not expect CITES to have a positive impact on the populations located in these countries. To check this, we create a dummy variable that identifies populations in Category 1 CITES member countries, i.e., countries that are CITES members and whose national legislation fully complies with the requirements of CITES, and interact it with our baseline treatment dummy. We present results in column (10). We find that CITES is only effective in Category 1 CITES member countries, i.e., countries that enforce CITES well, stressing the importance of proper enforcement.

In column (11), we include all variables simultaneously from columns (5) to (10), except that we only use the corruption measure from column (7), as it is available for more countries.[38] The precision of the estimates becomes low, and none of the regressors is individually significant. For example, we cannot distinguish meaningfully whether it is the high level of income of countries or their high level of enforcement that is driving CITES' effectiveness, as there is not enough variation in our data to separate the influence of the different variables. This is likely due to their high pairwise correlation, which ranges between 0.74 and 0.99.

*Contentious listing decisions.*—While Gehring and Ruffing (2008) argue that for CITES listing decisions, "arguments prevail over power", this may not always be the case, particular for species for which listing decisions are contentious. For these species, listing decisions may be driven by country-specific interests. We check whether our results are driven by such species. According to Blundell and Rodan (2001), listing decisions of elephants, sharks, turtles, and whales are particularly contentious. In a subsample analysis, we drop all populations of these species.[39] We present results in

[38]The correlation between in CITES $\times(1-P(BRIBE{=}1))$ and in CITES $\times(1-CORRUPT)$ is 0.82.

[39]Particularly, we drop all populations of the family Elephantidae (i.e., elephants) and of the order Testudines (i.e., turtles). To drop shark populations, we drop populations of the orders Carcharhiniformes, Hexanchiformes, Lamniformes, Orectolobiformes, Squaliformes, and Squatiniformes. Our data set does not contain shark populations of the orders Heterodontiformes, Pristiophoriformes, and Echinorhiniformes. As whales are an informal taxonomic group, we drop all populations of species that include the word "whale" in their common name. This also excludes orcas (killer whales, *Orcinus orca*) that are part of the family Delphinidae, i.e., oceanic dolphins. In total, we drop

Appendix Table 2. Compared with Table 2, we find similar results, albeit estimated coefficients are slightly larger in absolute magnitude. In sum, we do not find evidence that our results are driven by contentious listing decisions.

*Event study specifications.*—We now allow the treatment effect of CITES listings to vary over time in our event study specification given in Equation (3). We present estimates in Figure 4. Results confirm that CITES listings have a positive effect on species' population sizes, however, the effect of CITES does not occur immediately, as it takes about 16 to 20 years until populations of CITES-listed species increase in size as a consequence of the species' listings in CITES. In addition, the pre-trend variables are not significant, validating the common trend assumption.

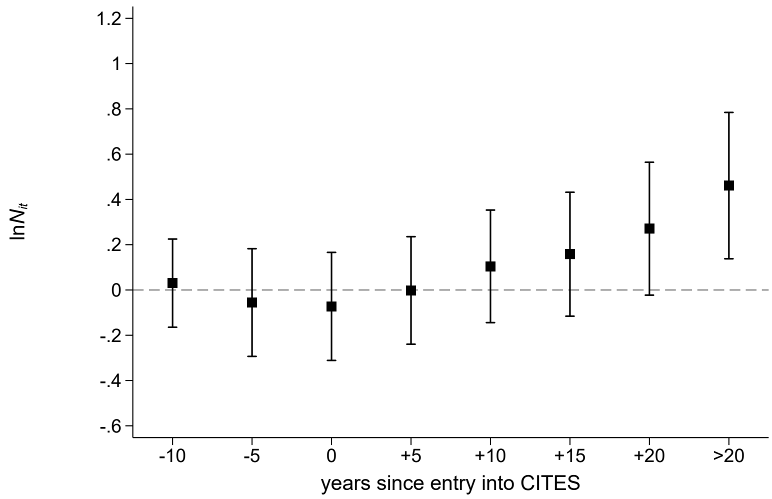
In the following, we present variations of the baseline event study specification where we allow for separate effects across different types of countries, similar to the variables presented in Table 2. As in columns (2) and (3) of Table 2, we allow for separate effects of species listed in 1975 and after 1975. We present results in Appendix Figure 1. We find a similar pattern of the estimated coefficients for both species listed in 1975 and those listed after 1975. It takes about 16 to 20 years until populations of CITES-listed species increase. Again, precision of the estimates is considerably lower when singling out 1975, probably because we observe the majority of populations after 1975. The effect of CITES on population sizes increases over time, as wildlife populations slowly recover.

We then allow for separate dynamic effects of CITES listings for countries with high and low corruption, using the corruption measure from column (7) in Table 2. We present results in Figure 5. For high-corruption countries (left panel), we find negative point estimates but with low precision overall. Importantly, we find that CITES' effectiveness is driven by populations in countries with low corruption (right panel). Compared to Figure 4, we now find significantly larger populations 11 to 15 years after inclusion into CITES, i.e., earlier than in the baseline event study specification.

In Figure 6, we alternatively allow for separate dynamic effect for population in low and high income countries, similar to column (8) in Table

5820 observations of 126 species.

Figure 4: Effect of CITES on population size (species listed in CITES)



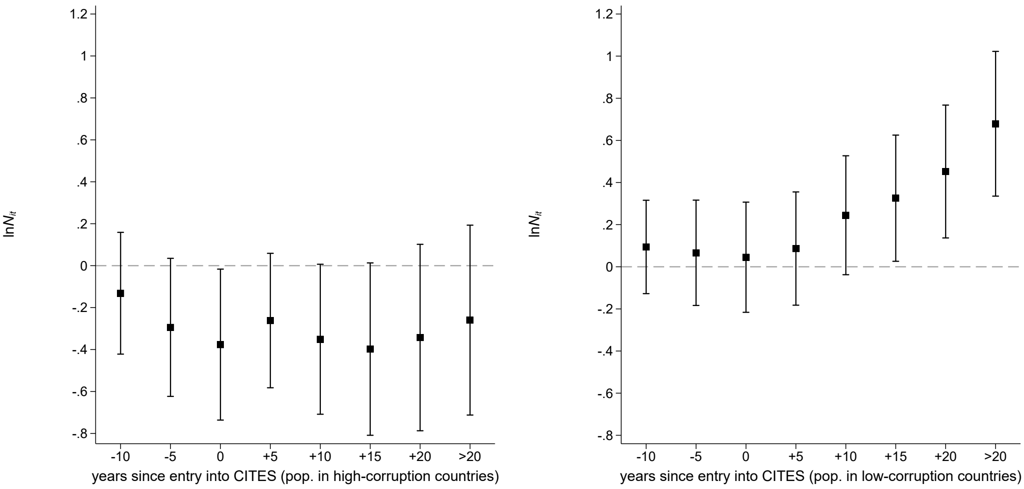

This figure shows coefficient estimates from Equation (3), i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population and year fixed effects. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 119538.

2. While we find no significant effect of CITES's listings in low-income countries (left panel), we find that populations are significantly larger in high-income countries beginning 16 years after their inclusion into CITES (right panel).

Finally, we allow for separate dynamic effects of CITES' listings depending on whether a population is located in a CITES member country with a high level of CITES enforcement, i.e., a Category 1 member country, similar to column (10) in Table 2. We present results in Figure 7. We find that CITES has a significant lagged effect on population sizes in Category 1 countries only, stressing the importance of proper enforcement.

The results in this section show evidence of heterogeneous treatment effects at the country level, as CITES is effective in countries with low corruption, high income, and strong enforcement. They also show that CITES' effectiveness increases over time.

Figure 5: Effect of CITES on population size by corruption level (species listed in CITES)



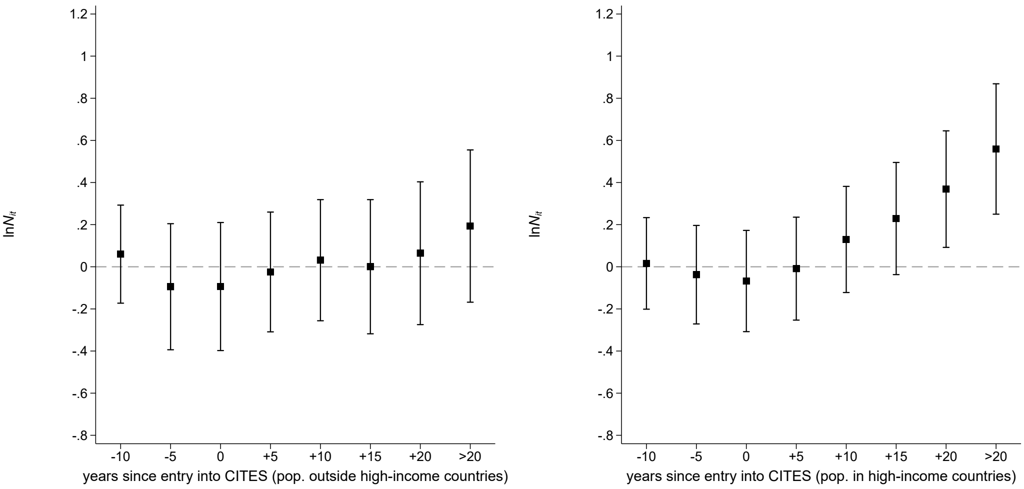

This figure shows results from a panel regression of log of population size on a set of treatment dummies, along with a set of population and year fixed effects. The left panel shows the coefficient estimates of dummy variables indicating the years since a species' entry into CITES interacted with a variable indicating whether the population is located in high-corruption countries. The right panel shows the coefficient estimates of dummy variables indicating the years since entry into CITES interacted with a variable indicating whether the population is located in a low-corruption country. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 113818.

## 4.2 Reverse causality and selection on vulnerability

While our specification controls for differences in the probability of inclusion of different species into CITES via the population fixed effects, it may be that a species' vulnerability or endangerment status change over time, i.e., species whose populations experience a recent decline are more likely to be included into CITES. This would be a case of reverse causality: Because a species is in decline, it gets listed. Similarly, species whose populations have increased recently may have a lower probability of inclusion into CITES. What is the implication of this *selection on vulnerability* on the size of our estimated CITES treatment effect?

Let us denote the influence of such a time-varying omitted variable by $\alpha x_{st}$. If this omitted variable is correlated with $(\text{in CITES})_{st}$, our estimated effect of CITES, $\beta$, is biased. With *selection on vulnerability*, the bias is

Figure 6: Effect of CITES on population size by country income level (species listed in CITES)



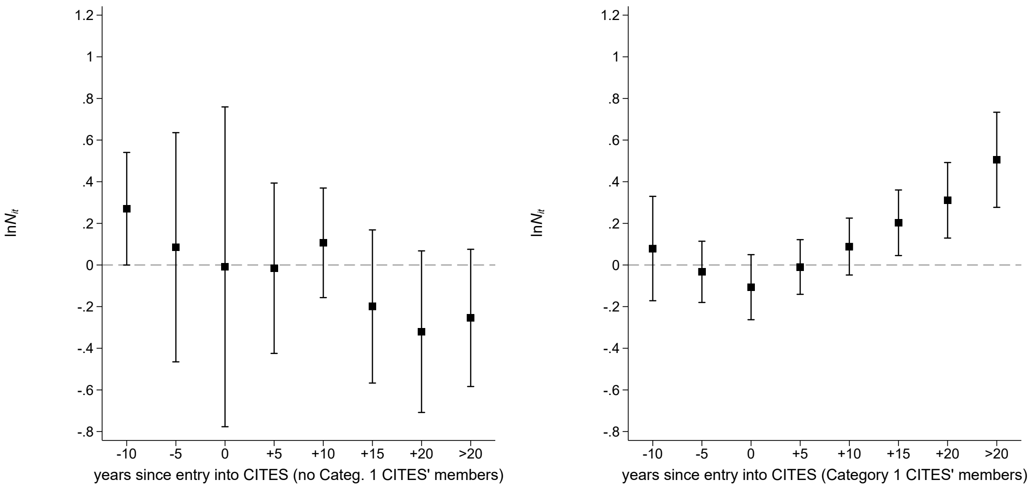

This figure shows results from a panel regression of log of population size on a set of treatment dummies, along with a set of population and year fixed effects. The left panel shows the coefficient estimates of dummy variables indicating the years since a species' entry into CITES interacted with a variable indicating whether the population is located outside high-income countries. The right panel shows the coefficient estimates of dummy variables indicating the years since entry into CITES interacted with a variable indicating whether the population is located in a high-income country. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 119538.

downwards, as vulnerability is negatively correlated with population sizes ($\alpha < 0$) and as discussed above, the correlation between vulnerability and CITES is likely positive, i.e., $Corr[(\text{in CITES})_{st}, x_{st}] > 0$.[40]

While the inclusion of population fixed effects goes some way towards controlling for this vulnerability bias, i.e., for its species and population specific time-invariant determinants, it does not control for time-varying vulnerability bias. In an ideal world, we would control for an independent, time-varying risk vulnerability measure at the population-level for all populations across the globe but such a measure does not exist to the best of our knowledge. The best available proxy at the global level is the IUCN Red List. The IUCN Red List is the largest available database that pub-

[40]In our species-level data, the correlation between a species being ever listed in CITES' Appendix I or II and it being classified as either "critically endangered", "endangered", or "vulnerable" by the time-invariant IUCN Red List data is 0.327.

Figure 7: Effect of CITES on population size depending on enforcement status (species listed in CITES, in CITES' member countries)




This figure shows results from a panel regression of log of population size on a set of treatment dummies, along with a set of population and year fixed effects. The left panel shows the coefficient estimates of dummy variables indicating the years since a species' entry into CITES interacted with a variable indicating whether the country is a CITES member in year $t$ for non-"Category 1" countries. The right panel shows the coefficient estimates of dummy variables indicating the years since entry into CITES interacted with a variable indicating whether the country is a CITES member in year $t$ for "Category 1" countries. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 119538.

lishes classifications for the endangerment status of individual species, see Rodrigues et al. (2006), but not at the level of populations. However, there is hardly any time variation in the IUCN Red List except for few individual species. Rondinini et al. (2014) estimate that with current funding levels, 83% of Red List assessments will be outdated by 2025 and the average age of assessments will be more than 30 years. Therefore, available measures of endangerment status are slow moving indicators at best.

Another way of looking at this problem is through counterfactual thinking, see Ferraro (2009). To estimate the effectiveness of CITES's listings, we would like to know what would have happened to a species had it not been listed. For a credible identification, we need to identify counterfactual species, i.e., those that are similar to listed species (in terms of their endangerment status and other characteristics) except their CITES listing status.

As two robustness checks, we use two separate approaches to identify such counterfactual species: 1.) As a crude approximation, we restrict our sample to species that have similar endangerment status, and 2.) we use a matching approach in the spirit of Ferraro et al. (2007) to identify counterfactual species that are similar in terms of observable characteristics. We will explain both approaches in the following.

*Subsample analysis for vulnerable species.*—We restrict our sample to species that have similar IUCN Red List threat evaluations, irrespective of whether they are listed in CITES or not. We use the IUCN Red List of Threatened Species classification on extinction risk and create an indicator variable that identifies vulnerable species, which we define as species that are classified as "critically endangered", "endangered", and "vulnerable", i.e., species that face a high risk of extinction in the wild. Once we do this, our sample reduces to 16391 observations, about 14% of our baseline sample. We present results in Appendix Table 3. The table replicates Table 2 from the main text but uses the restricted sample. Point estimates are mostly of the same sign and larger than those in our baseline results presented in Table 2, in line with our bias prediction. Likely due to the small sample size, most of the estimated effects are not significant with the exception of the effect of CITES in high-income countries (columns (8) and (11)). Hence, the subsample analysis confirms CITES' effectiveness for populations of species located in high-income countries.

*Matching.*—Ferraro et al. (2007) use propensity score and covariate matching estimators to control for the endogenous selection of species listed into ESA. This allows them to identify counterfactual species, i.e., comparable species that are not listed into ESA to answer the question what would have happened to a species' endangerment status if it had not been listed in ESA. While Ferraro et al. (2007) use a cross-sectional outcome regression, Ferraro and Miranda (2017) go one step further in terms of methodology and combine covariate matching with a two way fixed effects panel estimator to identify the counterfactual observations. This is closer to our application, as we also use panel data and a two way fixed effects panel estimator. We therefore combine the insights by Ferraro et al. (2007) and Ferraro and Miranda (2017): Following Ferraro and Miranda (2017), we use nearest neighbor covariate matching with replacement using a Mahalanobis distance metric to create a sample of matched counterfactual species, i.e., species that are similar in terms of observable characteristics except their CITES listing status, for all species that are listed into CITES in our data set. As Ferraro and Miranda (2017), we apply a caliper of one standard deviation of the propensity score, i.e., the probability that a species gets listed into CITES' Appendix I or II, and we trim our sample by keeping only those species for which the estimated propensity score lies within [0.1;0.9] using Crump et al. (2009)'s rule of thumb. We follow Ferraro et al. (2007) and use a Metrick and Weitzman (1996) style regression to estimate the propensity score, using the same covariates as we use for the covariate matching. In particular, we estimate a logit model using the same specification as we use in the log-linear probability model in column (4) of Appendix Table 1, i.e., the most exhaustive specification of our Metrick and Weitzman (1996) style regressions. After our matching procedure, the sample of matched species is well balanced in terms of covariates and the propensity score, see Appendix Table 4 and Appendix Figure 2.

Following the methodology of Ferraro and Miranda (2017), we use the matched species to create a matched panel dataset of populations. This panel contains all populations listed into CITES as well as all populations of species identified as nearest neighbors in the matching process, i.e., the non-treated species that serve as counterfactuals. This ensures that we estimate the treatment effect of CITES on population sizes on a sample of populations of species that are similar in terms of their observed covariates. In this panel dataset, we re-estimate our baseline panel fixed effects model from Table 2. We present results in Appendix Table 5. The matched sample consists of 13645 observations. A major difference is that in column (1), we find a smaller effect of CITES' listing on population sizes that is no longer significant, and we do not find significant effects of CITES after 1975 in columns (3) and (4). As in the baseline results, we find non-significant effects when we distinguish between species listed in 1975 in columns (2) and (4), and no difference between sanctioned and non-sanctioned countries in column (5). We do find significant effects and confirm the detrimental effects of corruption for the effectiveness of CITES in columns (6) and (7). We also confirm the effectiveness of CITES in high-income countries in column (8). As in the baseline results, we do not find a differential effect for

CITES member countries (column (9)), but we confirm the importance of enforcement in column (10). When controlling for all regressors simultaneously, we obtain similar signs and magnitudes as in our baseline results, but with low precision.

To sum up, when identifying counterfactual species, we confirm the importance of low corruption, high income, and the quality of enforcement for the effectiveness of CITES.

## 4.3 Effects of CITES: Robustness to cohort-specific treatment effects

A recent literature studies the estimation of treatment effects and event studies using two way fixed effects (TWFE) models with staggered treatments.[41] We explore its implications in our setting. This literature proposes alternative estimators than TWFE in the case of staggered treatments when treatment effects are heterogeneous across cohorts (units that receive treatment in the same time period form a cohort, i.e., in our setting species listed in CITES in the same year). Prominent examples for such estimators are Callaway and Sant'Anna (2021) and Sun and Abraham (2021). A common feature of this literature is that it focuses on balanced panels only, and the respective estimators can only be applied in balanced settings, whereas our panel is unbalanced.[42] This literature highlights that estimated treatment effects with staggered treatments may suffer from bias. This bias arises if the standard estimator forms wrong comparisons, i.e., comparisons between treated units (species) with those that will be treated at a later date. These comparisons contaminate the estimated treatment effects for one cohort by effects of later cohorts.

This bias can be remedied by estimating separate, cohort-specific event studies using TWFE using clean control groups.[43] This delivers consistent

[41] de Chaisemartin and D'Haultfœuille (2022) and Wooldridge (2021) provide technical surveys of this literature. For a more intuitive survey from an applied perspective, see Baker et al. (2022), but note that this paper does not include the insights by Wooldridge (2021).

[42] Similarly, Goodman-Bacon (2021)'s diagnostic decomposition often used in this context does not apply in unbalanced panels, see Baker et al. (2022), page 391.

[43] As Wooldridge (2021) points out, two further advantages of TWFE we use in comparison with the proposed estimators by Callaway and Sant'Anna (2021) and Sun and

treatment effects for each cohort, see Sun and Abraham (2021), Wooldridge (2021), and Baker et al. (2022). Baker et al. (2022) explain that this problem is particularly relevant when the data set contains only few units that are never treated (never included into CITES). In our analysis, the percentage of species in the sample that are never listed into CITES is 85.5%. Still, as a robustness check, we report estimates for single treatment events, i.e., cohorts, using as control groups only species that were never treated (never listed in CITES) to avoid comparison of treated species with species that will be treated at a later date. We estimate our model on a sample that only includes species that where included into CITES at its inception in 1975, and species that have never been included into CITES. Hence, this regression allows us to consistently estimate the treatment effects for those species that were included in 1975. We present these results in Appendix Figure 3. Similar to the results presented in the left panel of Appendix Figure 1, while point estimates indicate an increasing effect of CITES over time, the precision of the estimates is low.

Similarly, we estimate an event study for all species listed into CITES in 1977, the next CoP of CITES, see Appendix Figure 4. There, we find similar effects as in the right panel of Appendix Figure 1, both significant and increasing over time.

Finally, we estimate an event study for all species listed into CITES in 1979, see Appendix Figure 5. Estimates are imprecise. It seems that we identify significant and positive effects of CITES primarily from the variation offered by the species included in 1977.[44] In unreported regressions, we estimated cohort-specific treatment effects for species added at later CoPs. Most of these effects could not be identified in our sample, as only few species are included in any given year after 1979, see Figure 1. Also, not enough time has passed for species included at more recent CoPs to identify longer time lags. There are simply not enough species listed in CITES in our sample to allow identification of cohort-specific treatment effects in

Abraham (2021) are that it can be applied in unbalanced panels and it allows for the inclusion of heterogeneous trends, both features that are highly relevant in our application.

[44] There are no listed species in 1976 nor 1978, see 1. Listing mostly takes place at the biannual Conferences of the Parties (CoP) of CITES, so with few exceptions cohorts are formed by the species included at a particular CoP.

later years. Note that for consistent estimates of cohort-specific effects, the number of species per cohort has to be sufficiently large. Intuitively, in the extreme case of only one species listed per cohort (year), estimates would suffer from an incidental parameter problem and could not be estimated consistently.

To sum up, allowing for cohort-specific effects of CITES is pushing the variation in our unbalanced panel to such an extent that precise identification becomes difficult. Still, at least for some cohorts, CITES has significant and positive effects on population sizes.

## 4.4 Species-type specific treatment effects

We have seen that CITES is effective in countries that properly enforce it, but its effect occurs mostly with a 16 to 20 year lag. We now explore whether treatment effects differ across different types of species. For ease of exposition, we only include the last treatment dummy ($\tau > 20$) in these regressions instead of the full set of lags. We estimate separate effects for non-"Category 1" member countries (i.e., where CITES is poorly implemented or enforced) and for "Category 1" countries (i.e., where CITES is effectively implemented or enforced). We present results in Appendix Table 6. We consider the following different groups of species:

*Intentionally-used species.*—Some species have a direct economic value as they are used for human consumption, and as a consequence they are intentionally harvested with potential negative effects on their population sizes. We create an indicator variable for species with intentional use, i.e., where the species is the target of economic activity. Population size increases for species listed in CITES for "Category 1" countries only. We estimate a negative coefficient for the interaction term, but due to the lack of precision, it is not statistically significant, see column (1).

*Vulnerable species.*—The IUCN Red List provides an evaluation of the extinction risk of species using different categories. We identify species that are classified as either "critically endangered", "endangered", or "vulnerable" by the IUCN Red List as vulnerable species and create the according interaction term, see column (2). We find that CITES only increases population size in "Category 1" member countries. The interaction term for vulnerable species is not precisely estimated.

*Highly-studied species.*—Some species receive more funding for their conservation and hence are more studied by researchers (see, e.g., Brambilla et al., 2013; Colléony et al., 2017). This may be because some species are more well-known and have particularly desirable features in the view of the general public, e.g., the "cuteness" of koalas (*Phascolarctos cinereus*).[45] CITES' effectiveness may be different for these highly-studied species. Our population data contain information about the scientific study that is the data source for a specific population's size over time. We therefore count the number of studies per species and year and create a dummy variable for those species for which the number of studies is larger than the sample average. We present results in column (3). We do not find evidence that CITES effectiveness changes for highly-studied species.

*Well-known species.*—The number of scientific studies may not fully reflect how well-known a species is in the general public. We therefore use a more direct proxy by using data from the citizen science project *iNaturalist*. Contributors to *iNaturalist* can identify the species of the animal they have seen using their smartphone and have the possibility to corroborate the data by confirming the species identified by other users in the *iNaturalist* app. Different users should agree more often on a species the more well-known it is. We calculate the average number of identification agreements by species and year. We interpret a higher than average number of agreements as an indication that a species is well-known. Column (4) shows that CITES' effectiveness does not depend on how well-known a species is.

*Large species.*—We consider species-type specific treatment effects for "charismatic megafauna" as a species' charisma may be a function of its physical size (see Metrick and Weitzman, 1996). We create a variable for large species as a dummy that equals one for those species with a higher than average body mass in our sample, and zero otherwise.[46] According to our results in column (5), CITES increases population size in "Category 1" member countries for both large and other species. In non-

[45]More generally, the less similar a particular taxonomic order is to humans (i.e., the larger its phylogenetic distance), the lower the number of scientific studies on this species (Martín-López et al., 2009).

[46]In this group, our sample includes species that are well-known under their common names buffalo, elephant, giraffe, hippopotamus, manatee, rhino, walrus, and whale.

"Category 1" member countries, CITES increases the population size only of large species. As these species are more readily recognizable by enforcing agencies such as customs, the enforcement of CITES' regulations for these species may be easier. It may also reflect that these species are particularly salient for the international community and member countries with weak enforcement therefore focus their efforts on these species.

While our results show that species with different characteristics do not seem to benefit differently from CITES,[47] we find clear evidence of heterogeneous treatment effects across countries as CITES is only effective in countries with strong enforcement. We therefore explore the impact of unobserved time-varying country-level confounding factors in the next section.

## 4.5 Controlling for country-specific time-varying confounding factors

Our results have shown the importance of country characteristics for CITES' effectiveness. Countries' enforcement efforts, their attitudes towards protecting wildlife, the size of their wildlife populations, and voting in favor of listing further species at one of the CoPs are likely correlated. Over time, these attitudes may change due to changes in countries' governments as well as changes in societal attitudes and awareness concerning environmental issues. These and other time-varying country-specific factors that affect both the probability of a species' listing in CITES and its population size may bias our results. For example, the occurrence of (civil) wars correlates with wildlife decline (see Daskin and Pringle, 2018). The extent of agricultural production also varies across countries, and increases in agricultural production are a key driver of habitat loss and subsequent wildlife decline (see Green et al., 2005).

To control for these and other unobserved country-specific time-varying factors, in a first step, we include country-specific trends by augmenting Equation (3) with $\delta_c t$. To check robustness, we consider several specifications that account for different functional forms of the country-specific

[47] Excluding large species, as CITES is effective for large species in non-"Category 1" member countries.

time trends. Following Neumark et al. (2014), we consider polynomials of orders 2 to 5 for country-specific trends, and we compare results with our main specification (i.e., without country-specific trends). Results in Appendix Table 7 show the robustness of our baseline results. Across all specifications, pre-trends are not significant. For all specifications, we obtain a positive and significant effect of CITES on population sizes 16 to 20 years after species' listing. We also obtain a positive and significant effect of CITES after 11 to 15 years of listing the species in CITES at a 10% of significance level for all specifications, except for our main specification (i.e., without country-specific trends).

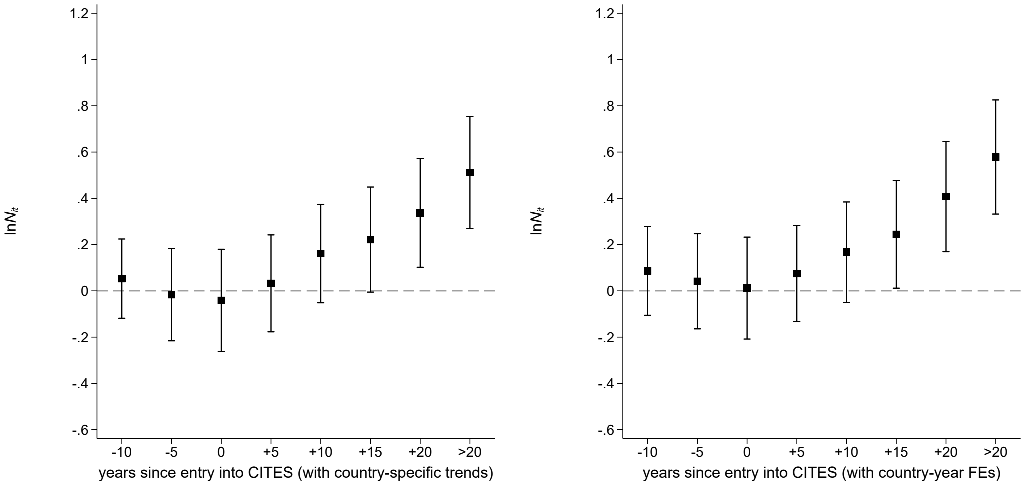
In a second step, instead of country-specific time trends, we include country-year fixed effects into our model to control for arbitrary shocks across countries and time, e.g., time-varying enforcement and compliance patterns over CITES signatory and non-signatory parties.

Figure 8 displays results for our event study specification augmented with the country-specific linear trends (in the left panel) and country-year fixed effects (in the right panel). Obtained results are consistent with our main findings and confirm that listing species in CITES has a positive effect on species population size. Again, this effect is lagged and starts to be significant from 16 to 20 years after listing.

## 4.6 Are estimates of CITES' effectiveness driven by domestic regulation?

In our analysis, we have considered species listed in Appendices I and II, i.e., those species which have been listed after approval by the CITES member countries. The previous section controls for country-year specific drivers of CITES listings, e.g., country-specific lobbying efforts in favor or against CITES listings. At the same time, particularly range countries may lobby for the protection of specific species. More generally, some countries may protect their wildlife populations by domestic regulations, independent of whether a species is included in CITES. One may think that our estimates for the effectiveness of CITES simply pick up the effectiveness of such domestic regulations, and wrongly attribute their effect to CITES. The multilateral listing decision occurs in the same year for all populations of

Figure 8: Effect of CITES on population size, including country-specific trends, or country-year FEs (species listed in CITES)




This figure shows coefficient estimates from an event study specification, i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population fixed effects. The left panel shows the coefficient estimates of a specification that includes country-specific time trends. The right panel shows the coefficient estimates of a specification that includes country-year fixed effects. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 119538 (left panel) and 118106 (right panel).

a species across all countries, whereas domestic regulations that protect particular species that are unrelated to CITES can be implemented at any point in time. Our identification strategy uses this timing of listing decisions that is the same for all populations of a species but that varies across species and correlates it with population sizes over time.

It is difficult to identify whether there exists domestic regulation that protects a particular species in a country in a given year for the large list of species and populations in our dataset. However, a particular feature of CITES' appendices allows us to proxy for such efforts at the species-country-year level.

Appendix III is different from Appendices I and II. Whereas inclusion into Appendices I and II occurs only after a positive vote by a two-thirds majority of the member countries, members unilaterally can list into Appendix III native species not included in Appendices I and II that they protect through domestic regulation. These inclusions have to be submitted to the Secretariat. To qualify, species have to be native to the country, have to be protected by domestic policies, and require control of their international trade.[48]

To investigate whether our estimates of the effectiveness of CITES are driven by species-specific domestic policies, we create a dummy variable $(\text{domestic regulation})_{sct}$ that is 1 if country $c$ has in place domestic regulation that protects species $s$ in year $t$, and 0 otherwise. We proxy this by a listing of the species in Appendix III by the respective country.[49] We then estimate the following model:

$$\begin{aligned} \ln N_{slt} &= \mu_{sl} + \eta_{ct} + \beta(\text{in CITES})_{st} \\ &\quad + \delta(\text{domestic regulation})_{sct} + \varepsilon_{slt}. \end{aligned} \tag{4}$$

We present results in Table 3. In column (1), we estimate a restricted version of Equation (4) by including the dummy for domestic regulation, but not the CITES listing dummy. The estimate is negative and not significant. In column (2), we only include our regressor of interest, $(\text{in CITES})_{st}$. Note that column (2) in Table 3 is similar to column (1) in Table 2 but now includes country-year fixed effects instead of only year fixed effects to control for unobserved country-specific factors that may vary over time as in Section 4.5. The estimate is positive, significant, and similar to its counterpart in Table 2. In column (3), we add our control for domestic regulation. Whereas the effect of CITES remains significant and barely changes, the effect of domestic regulation remains not significant. This is not surprising, given the low correlation between the two regressors of 0.07.

We then reestimate the event specification from the right panel of Figure 8 but add the control variable "domestic regulation". We present results in Figure 9. While domestic regulation is again not significant, the point estimates of the effects of CITES are virtually unchanged.[50]

[48]See CITES Article II(3) in combination with Conf. 9.25 (Rev. CoP18) "Implementation of the Convention for species in Appendix III" and its predecessor Conf. 5.22, and Favre (1989).

[49]A caveat is in order, though: Countries may have domestic regulation that protects a particular species but not list it in Appendix III. In this case, our proxy does not measure domestic protection accurately.

[50]We present point estimates of the event study with and without controls in Appendix Table 8.

Table 3: Effect of CITES on population size controlling for domestic regulation

| | (1) | (2) | (3) |
|---|---|---|---|
| domestic regulation | −0.112 | | −0.215 |
| | (0.226) | | (0.218) |
| in CITES | | 0.211 | 0.212 |
| | | (0.061) | (0.061) |
| $N$ | 118106 | 118106 | 118106 |

*Notes*: Table 3 reports estimated regression coefficients from a panel regression of log of population size on a set of regressors along with a set of population and country-year fixed effects. Standard errors are in parentheses and are clustered at the species level. Column (1) estimates a regression in which the treatment dummy equals one for populations of species located in countries with domestic regulation protecting the respective species (i.e., for countries listing the species in Appendix III). Column (2) includes a dummy equal to one for species listed in Appendix I or II, i.e., the same regressor as in column (1) in Table 2. Column (3) includes both treatment dummies simultaneously.

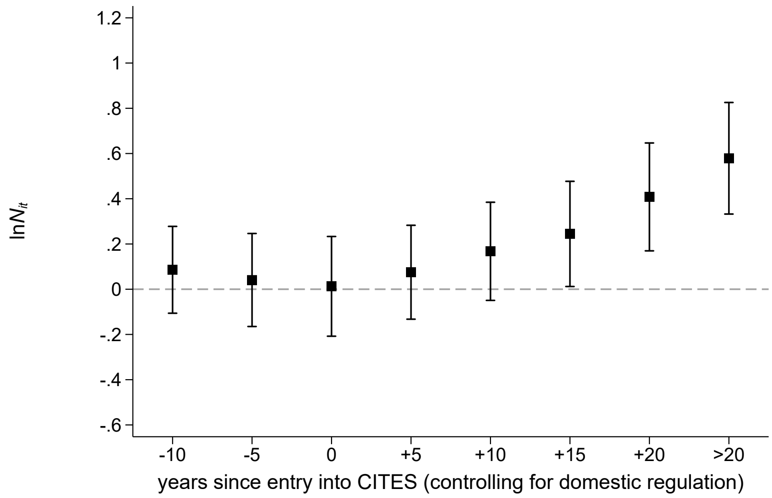
In sum, we find that domestic regulation is not driving our results that CITES has a positive effect on wildlife populations.

## 4.7 Is CITES effective because it bans wildlife trade or because it enables sustainable wildlife trade?

We have established that inclusion into CITES leads to an increase in the size of wildlife populations. Until now, our analysis has remained silent on a key debate concerning CITES. The main way how CITES offers protection for species is via inclusion into either its Appendix I or Appendix II. These two appendices represent two different approaches to wildlife conservation. Species listed in Appendix I cannot be traded internationally for commercial purposes, i.e., it imposes an international trade ban. Species listed in Appendix II can be traded internationally as long as this trade is sustainable and does not endanger the survival of the species ("sustainable use"). Which of these two approaches is more effective in protecting wildlife is debated among conservationists, policy makers, and the wider community. Economic theory as well as case studies provide conflicting arguments.

On the one hand, prohibiting trade may have negative effects as it reduces the (international) legal value of wildlife to zero, reducing economic incentives to protect wildlife. Also, enforcing wildlife trade bans is difficult. Bans reduce the legal supply of goods from wildlife, but do not directly affect demand. At the same time, bans may stigmatize the purchase and

Figure 9: Effect of CITES on population size controlling for domestic regulation




This figure shows coefficient estimates from an event study specification, i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into Appendix I or II of CITES, along with a set of population and country-year fixed effects. It also includes a dummy that equals one for populations of species located in countries with domestic regulation protecting the respective species (i.e., for countries listing the species in Appendix III). 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 118106.

possession of goods derived from endangered species, and hence may reduce demand as well, see Fischer (2004).

On the other hand, sustainable use of species listed in Appendix II allows local communities to generate income from their legal use in the long run (Rivalan et al., 2007; Challender et al., 2015). However, it may increase demand by legitimizing the consumption of wildlife goods. Consumers may interpret labels that assure goods are produced in accordance with CITES as a go-ahead without any negative environmental consequences. It may also allow poachers to launder illegally harvested specimens in the legal market, see Fischer (2004).

Trade bans create incentives for poaching and diversion (or "leakage") to illegal channels, rendering bans ineffective. While these leakage concerns are likely more important for trade bans as their cost of compliance is higher, sustainable use restrictions impose costs on traders and therefore

Figure 10: Distribution of year of first entry into CITES' Appendix I and Appendix II

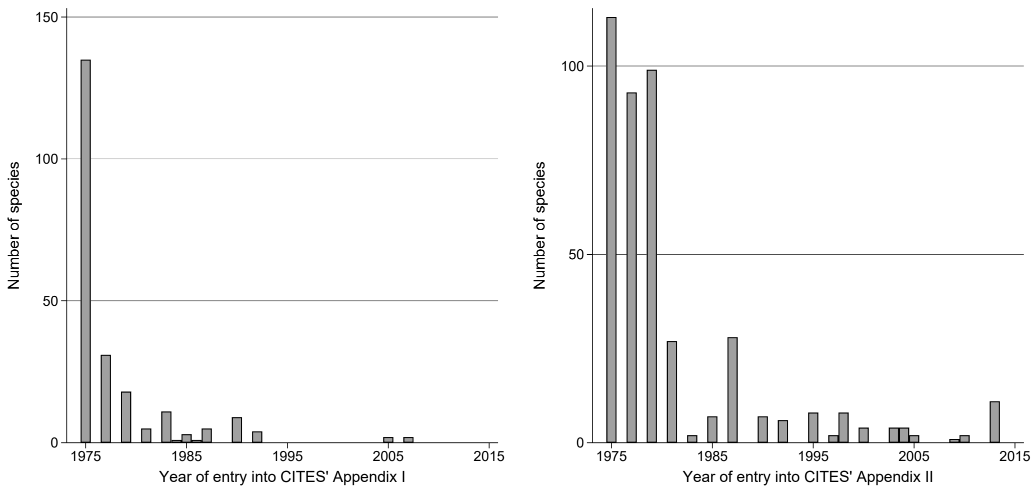


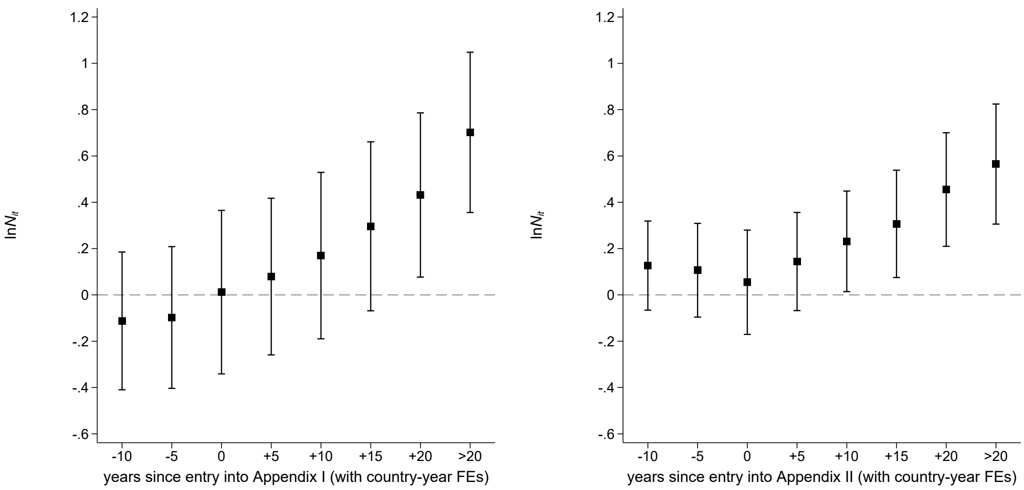

Figure depicts the distribution of the year a species entered into Appendix I (left panel) and Appendix II (right panel) in our data.

also can lead to illegal wildlife trade. As we use full wildlife population data, not trade or production data, we circumvent these issues.

To disentangle which mechanism, trade bans or sustainable use, dominates the positive effect of CITES on wildlife population sizes, we distinguish species listed in Appendix I and species listed in Appendix II. In Equation (3) we have defined $t_s^{CITES}$ as the year when a species $s$ is included in either Appendix I or II for the first time, whichever year comes first. We now distinguish whether a species has ever been listed in Appendix I or Appendix II. We show the distribution of years when a species in our sample is first listed in one of the two appendices in Figure 10. There has been a movement away from Appendix I in recent years and species now enter CITES via its Appendix II.[51]

[51] Note that the number of species entering into any of CITES' appendices depicted in Figure 1 is not the sum of species entering in Appendix I and Appendix II as species can have been uplisted or downlisted over time. For example, the Mauritius kestrel (*Falco punctatus*), a falcon from Mauritius, was included into Appendix II in 1975 and uplisted to Appendix I in 1977, hence it appears in both the left and right panel of Figure 10, but only once in Figure 1. In addition, in a given year, a (sub)species may be listed in one appendix, but a subspecies of the same species may be listed in another appendix, so that the species appears in both appendices in one year. For example, different subspecies of the brown bear (*Ursus arctos*) were listed in either Appendix I or II in 1975.

Figure 11: Effect of CITES on population size (App. I vs. App. II), including country-year FEs (species listed in CITES)




The left panel shows coefficient estimates from an event study specification, i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES' Appendix I. The right panel shows coefficients from a separate estimation where dummy variables indicate the years since entry into CITES' Appendix II. Both regressions include population and country-year fixed effects. 95% percent confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 118106.

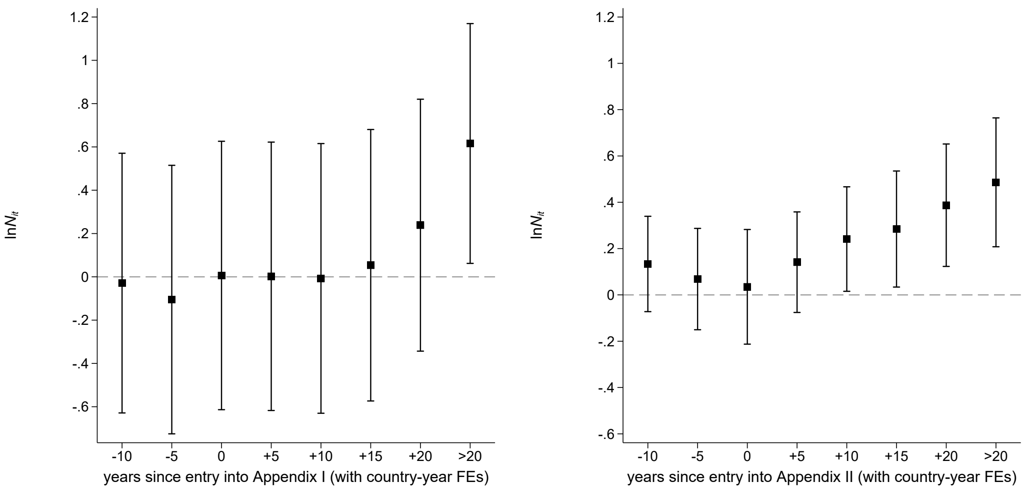
We show the results of two regressions in Figure 11. The left panel shows coefficients of an event study where we define treatment as the year when a species is listed in Appendix I. The right panel shows the results for a regression where we define treatment as the year when a species is listed in Appendix II. We find that population sizes of species included into either Appendix I or II increase by similar amounts. However, the positive effect on wildlife population sizes is significant after 6 to 10 years of inclusion into Appendix II, whereas we find a positive and significant effect for species included into Appendix I after 16 to 20 years.

Some species may move from Appendix II to Appendix I (they get "uplisted") or from Appendix I to Appendix II (they get "downlisted"). The previous regressions ignored these dynamics. We therefore take into account a species' history of being uplisted or downlisted as a robustness check. For example, the bald eagle (*Haliaeetus leucocephalus*) was downlisted from Appendix I to Appendix II in 2005, while the African elephant (*Loxodonta africana*) was first listed in CITES' Appendix II in 1977, and uplisted to Appendix I in 1990. Listings in CITES appendices can occur at any taxonomic level, i.e., either individual (sub-)species are included in an appendix or a whole taxonomic group (genus, family, order) is included, i.e., groups of related species. The sturgeon, the source of sought-after caviar, is a good example. The common name sturgeon refers to 27 species which are part of the family Acipenseridae, which itself is part of the larger order Acipenseriformes. In 1975, the species *Acipenser oxyrinchus* was included in Appendix I. In 1979, this species was downlisted to Appendix II. In 1998, the whole order Acipenseriformes was included in Appendix II. The family Lemuridae, a group of primates found primarily in Madagascar, was included in Appendix I in 1975, except the probably best known lemur species, the ring-tailed lemur (*Lemur catta*), which was included in Appendix II. *Lemur catta* was then uplisted into Appendix I in 1977. We take into account changes like these in Figure 12, where we drop populations of species that have ever been listed in *both* Appendix I *and* Appendix II during the period available for those populations in the sample ("switchers").[52]

Figure 12 confirms that trade bans and sustainable use are both effective in the long-run. We find significant positive effects on wildlife populations of incentivizing sustainable use six to ten years after inclusion into Appendix II. However, it seems that identification of positive effects for trade bans, i.e., Appendix I, in Figure 11 stems mostly from "switchers". The low precision of the effect of Appendix I listings implies that we cannot rule

[52]Specifically, we drop from our regressions 4707 observations that correspond to populations of 46 (sub-)species. Their common names are: Addax, African elephant, American alligator, American crocodile, Bald eagle, Black caiman, Black rhinoceros, Bonobo, Brown bear (Grizzly and Kodiak bear), Chimpanzee, Common spider tortoise, Dalmatian pelican, Dugong, Fin whale, Flatback turtle, Forest elephant, Green turtle, Grey wolf, Guadalupe fur seal, Gyrfalcon, Iberian lynx, Indus blind dolphin, Insular flying-fox, Irrawaddy dolphin, Leatherback turtle, Loggerhead sea turtle, Markhor, Mauritius kestrel, Mongolian saiga, Morelet's crocodile, Nile crocodile, Northern elephant seal, Olive ridley, Peregrine falcon, Red-necked amazon, Ring-tailed lemur, Saltwater crocodile, Samoa flying fox, Sei whale, Southern white rhinoceros, Tiger, Vicuna, and Yellow-shouldered amazon. Of these, 9 were downlisted from Appendix I to Appendix II of CITES: American alligator, Bald eagle, Black caiman, Mongolian saiga, Morelet's crocodile, Nile crocodile, Northern elephant seal, Southern white rhinoceros, and Vicuna; different populations of 7 (sub-) species were listed in different CITES' appendices (I and II) the same year: Dugong, Fin whale, Grizzly bear, Markhor, Red-necked amazon, Sei whale, and Yellow-shouldered amazon. For example, all populations of Dugong were listed in Appendix I in 1975, except those of Australia that were listed in Appendix II. The remaining (sub-)species correspond to species that were uplisted from Appendix II to Appendix I of CITES.

Figure 12: Effect of CITES on population size (App. I vs. App. II), including country-year FEs (species listed in CITES), excluding "switchers"




The left panel shows coefficient estimates from an event study specification, i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES' Appendix I. The right panel shows coefficients from a separate estimation where dummy variables indicate the years since entry into CITES' Appendix II. Both regressions include population and country-year fixed effects. 95% percent confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 113556.

out positive effects of trade bans in the early years after their imposition. One of the reasons for this may be the relatively low number of affected species who are not "switchers" in Appendix I.

# 5 Conclusion

Wildlife is in decline. One driver of this decline is international wildlife trade. CITES is the international environmental agreement whose goal is to protect endangered species from extinction either by restricting their international trade to sustainable levels or by banning their international trade altogether. We provide the first global assessment of CITES' effectiveness. We combine geo-referenced panel data on wildlife population sizes for 11054 vertebrate populations across 185 countries with the history of their species' inclusion into CITES and of countries' CITES membership. We find that CITES is effective: Wildlife populations increase by 20% after their species' inclusion into CITES. This effect accrues slowly over time.

We find significant and positive effects 16 to 20 years after species are listed in CITES.

While our results show that CITES does prevent wildlife decline, our approach remains silent on whether other approaches would be more efficient to protect wildlife than regulating wildlife trade via CITES. Neither does our approach determine whether the effect of CITES is strong enough to prevent the eventual extinction of a population or of the whole species, or whether CITES merely postpones its extinction. Wildlife decline is not only caused by the harvesting and consumption of endangered species, the focus of CITES. The production of merchandise goods for foreign consumption in biodiversity hotspots has been shown to have large detrimental effects on wildlife due to its impact on habitat loss, see Lenzen et al. (2012). Identifying and monitoring the effects of international merchandise trade on wildlife may well be needed to effectively prevent the extinction of endangered species. Irrespective of the specific drivers of wildlife decline, our research highlights that efforts to create detailed, time-varying indicators of the vulnerability of wildlife at the population level should be increased, as our knowledge of the state of wildlife populations is still inadequate.

Our results reveal that CITES is effective at protecting wildlife, but only in member countries that properly enforce its rules. We also document that CITES mainly protects populations in high-income countries, which may indicate lack of funding for proper enforcement in low-income countries. Focusing on mechanisms, we find that both wildlife trade bans and restrictions that incentivize sustainable use of endangered species increase wildlife.

In light of these results, it seems that much of the ongoing focus on the relative merits of trade bans versus sustainable use of wildlife is at least partly misguided. International wildlife trade regulation can be a tool for effective wildlife protection—if it is properly enforced, and properly funded. More generally, CITES demonstrates that international environmental agreements can be effective. However, mere membership in an agreement is not enough; instead, an agreement is effective if its members commit to its rules and enforce them. Therefore, empirical evaluations of international environmental agreements should take into account not only whether countries sign up to them but also the level of de facto enforcement of their rules.

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# For Online Publication Appendix for "International Environmental Agreements and Imperfect Enforcement: Evidence from CITES"

## A Determinants of CITES listings

We provide evidence that different types of species are not selected randomly to be listed in CITES, creating a selection bias. We investigate a number of factors that may affect the probability of a particular species being listed into CITES. For example, more charismatic species that are well-known and studied more often, or species with a higher extinction risk may have a higher probability of being listed.

We use the LPI data in combination with the CITES listing data, data on the average body mass of a species from the EltonTraits 1.0 dataset, as well as IUCN Red List extinction risk data, data on whether a species is used intentionally, and whether the species is threatened by fishing, both intentionally (the species is the target of the fishing activity) or unintentionally (e.g., by-catch). Data sources are described in detail in the main text in Section 2.3.

As regressors, in addition to including dummies for the taxonomic class (mammal, bird, reptile, and amphibian; fishes are the baseline category), we consider whether the species is vulnerable (*vulnerable*) and whether there is intentional biological resource use of the species (the species is the target), i.e., including hunting and collection of terrestrial animals, fishing and harvesting aquatic resources (*intentional use*). We also include separately any direct threat of fishing, which includes unintentional effects, i.e., the species is not the target (*fishing*). The last regressor is the log of the average of the body mass of the species (*log of body mass*).

We present results of an OLS regression in which the dependent variable is a dummy variable that equals one when the species has ever been listed in CITES and zero when the species has never been listed in CITES in Appendix Table 1. As regressors, we use the variables described above. Columns (1) to (4) show that mammals, birds, and reptiles are more likely

Appendix Table 1: Determinants of CITES listings (cross-section)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| mammal | 0.416 | 0.353 | 0.346 | 0.308 | 0.053 |
| | (0.025) | (0.024) | (0.023) | (0.024) | (0.023) |
| bird | 0.123 | 0.116 | 0.167 | 0.136 | |
| | (0.011) | (0.012) | (0.013) | (0.014) | |
| reptile | 0.246 | 0.228 | 0.276 | 0.244 | |
| | (0.034) | (0.038) | (0.037) | (0.038) | |
| amphibian | −0.014 | −0.039 | 0.019 | −0.012 | |
| | (0.009) | (0.014) | (0.015) | (0.016) | |
| vulnerable | | 0.280 | 0.231 | 0.227 | 0.195 |
| | | (0.023) | (0.023) | (0.023) | (0.033) |
| intentional use | | | 0.162 | 0.184 | 0.131 |
| | | | (0.016) | (0.019) | (0.034) |
| fishing | | | | −0.097 | |
| | | | | (0.020) | |
| log of body mass | | | | | 0.038 |
| | | | | | (0.004) |
| $R^2$ | 0.16 | 0.22 | 0.25 | 0.25 | 0.27 |
| $N$ | 3622 | 2838 | 2838 | 2838 | 1647 |

*Notes*: Appendix Table 1 reports estimated regression coefficients from an OLS regression of a dummy variable that equals one when the (sub-)species has ever been listed in CITES (and zero when it has never been listed in CITES) on a number of variables affecting the probability of being listed. Standard errors are in parentheses and are clustered at the species level. Data are for a cross-section of the subsample of species from the LPI data for which the IUCN Red List reports information on threats. Regressions in columns (1)-(4) include dummies for the taxonomic class of the species: mammal, bird, reptile, and amphibian; fishes are the baseline category. Columns (2)-(5) include variables that measure whether the species is vulnerable and whether the species is used intentionally. Column (4) also includes a variable that measures whether there is any threat of fishing. Column (5) includes a variable for body mass of the species. Data on body mass is only available for mammals and birds, therefore column (5) includes a dummy for the taxonomic class mammals only; birds are the baseline category.

to be listed in CITES than fishes. This is consistent with Metrick and Weitzman (1996) who analyze listing decisions for the Endangered Species Act in the United States. A species is more likely to be listed if it is more vulnerable, i.e., it has a higher extinction risk, see columns (2) to (5); if a species is used intentionally, see columns (3) to (5); and it is less likely to be listed if there is any direct threat of fishing, see column (4). Finally, column (5) shows that large species (i.e., with a higher body mass) are more likely to be listed.[53]

# B Baseline results for non-contentious species

[53] Because data on body mass is only available for mammals and birds, this regression only includes a dummy for the taxonomic class mammals; birds are the baseline category.

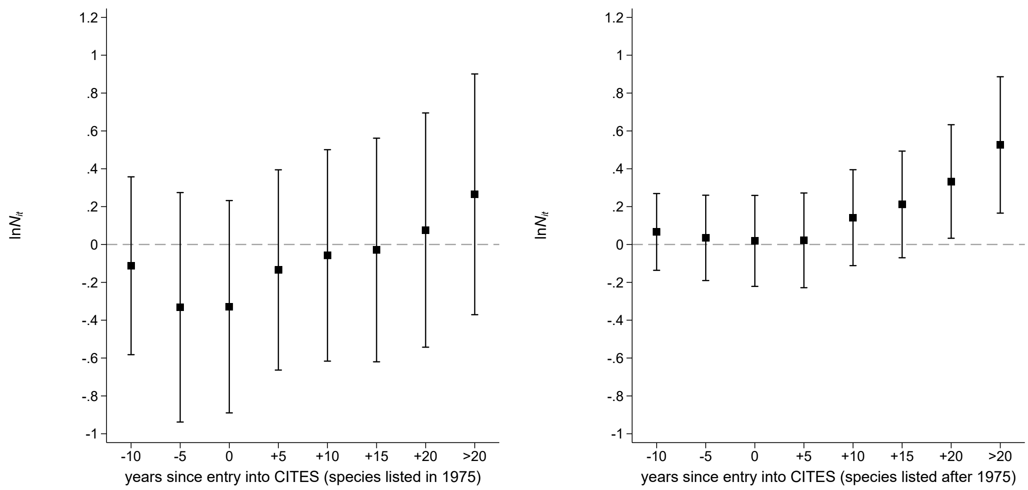
## Appendix Table 2: Effect of CITES on population size (species listed in CITES) dropping contentious species

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| in CITES | 0.225 | | | | 0.218 | −1.645 | −0.199 | −0.005 | 0.113 | 0.074 | −0.182 |
| | (0.078) | | | | (0.145) | (0.325) | (0.121) | (0.107) | (0.116) | (0.116) | (0.198) |
| in CITES in 1975 | | 0.299 | | 0.309 | | | | | | | |
| | | (0.151) | | (0.151) | | | | | | | |
| in CITES after 1975 | | | 0.188 | 0.194 | | | | | | | |
| | | | (0.086) | (0.087) | | | | | | | |
| in CITES × *NONSANCTIONED* | | | | | 0.007 | | | | | | 0.000 |
| | | | | | (0.128) | | | | | | (0.129) |
| in CITES $\times(1-P(BRIBE{=}1))$ | | | | | | 2.203 | | | | | |
| | | | | | | (0.374) | | | | | |
| in CITES $\times(1-CORRUPT)$ | | | | | | | 0.543 | | | | 0.265 |
| | | | | | | | (0.135) | | | | (0.152) |
| in CITES × *HIGH-INCOME* | | | | | | | | 0.318 | | | 0.107 |
| | | | | | | | | (0.095) | | | (0.088) |
| in CITES × *MEMBER* | | | | | | | | | 0.133 | −0.388 | −0.247 |
| | | | | | | | | | (0.106) | (0.148) | (0.147) |
| in CITES × *MEMBER* × *CATEGORY 1* | | | | | | | | | | 0.635 | 0.423 |
| | | | | | | | | | | (0.150) | (0.160) |
| $N$ | 113930 | 113930 | 113930 | 113930 | 113930 | 102867 | 108513 | 113930 | 113930 | 113930 | 108513 |

*Notes*: Appendix Table 2 reports estimated regression coefficients from a panel regression of log of population size on various treatment dummies along a set of population and year fixed effects for a subsample that drops all contentious species. Standard errors are in parentheses and are clustered at the species level. Column (1) estimates Equation (2), i.e., a regression in which the treatment dummy equals one for populations of species listed in CITES. Column (2) includes a variation of the treatment dummy that equals one for populations of species listed in CITES in 1975. Column (3) includes a variation of the treatment dummy that equals one for species listed in CITES after 1975. Column (4) includes both treatment dummies for species listed in CITES in 1975 and after 1975. Column (5) interacts the treatment dummy from column (1) with a dummy variable that is one for populations located in non-sanctioned countries. Column (6) interacts the treatment dummy with $1-P(BRIBE{=}1)$, where $P(\cdot)$ is the probability of corruption at the border. Column (7) interacts the treatment dummy with a dummy variable that is one when a country has a low level of corruption. Column (8) interacts the treatment dummy with a dummy variable that is one for populations located in high-income countries. Column (9) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES member country. Column (10) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES Category 1 member country, i.e., a country with high levels of enforcement. Column (11) includes several regressors simultaneously.

# C Separate effects for species listed in 1975 and after

Appendix Figure 1: Effect of CITES on population size (species listed in CITES). Species listed in 1975 vs. species listed after 1975




This figure shows coefficient estimates from Equation (3), i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population and year fixed effects. The left panel shows the coefficient estimates of dummy variables indicating the years since entry into CITES interacted with a variable indicating whether the species was listed in CITES in 1975. The right panel shows the coefficient estimates of dummy variables indicating the years since entry into CITES interacted with a variable indicating whether the species was listed in CITES after 1975. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 119538.

# D Effect of CITES: subsample analysis and matched sample

Appendix Table 3: Effect of CITES on population size (species listed in CITES) only for vulnerable species

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| in CITES | 0.191 | | | | 0.303 | −0.319 | 0.023 | 0.091 | 0.015 | 0.026 | −0.105 |
| | (0.132) | | | | (0.167) | (0.555) | (0.135) | (0.125) | (0.147) | (0.141) | (0.156) |
| in CITES in 1975 | | 0.329 | | 0.334 | | | | | | | |
| | | (0.175) | | (0.178) | | | | | | | |
| in CITES after 1975 | | | −0.001 | 0.039 | | | | | | | |
| | | | (0.143) | (0.149) | | | | | | | |
| in CITES × *NONSANCTIONED* | | | | | −0.113 | | | | | | −0.069 |
| | | | | | (0.098) | | | | | | (0.098) |
| in CITES $\times(1 - P(BRIBE{=}1))$ | | | | | | 0.607 | | | | | |
| | | | | | | (0.753) | | | | | |
| in CITES $\times(1 - CORRUPT)$ | | | | | | | 0.281 | | | | −0.065 |
| | | | | | | | (0.296) | | | | (0.230) |
| in CITES × *HIGH-INCOME* | | | | | | | | 0.324 | | | 0.316 |
| | | | | | | | | (0.153) | | | (0.136) |
| in CITES × *MEMBER* | | | | | | | | | 0.232 | 0.031 | 0.176 |
| | | | | | | | | | (0.140) | (0.146) | (0.162) |
| in CITES × *MEMBER* × *CATEGORY 1* | | | | | | | | | | 0.301 | 0.192 |
| | | | | | | | | | | (0.243) | (0.201) |
| $N$ | 16391 | 16391 | 16391 | 16391 | 16391 | 13127 | 14943 | 16391 | 16391 | 16391 | 14943 |

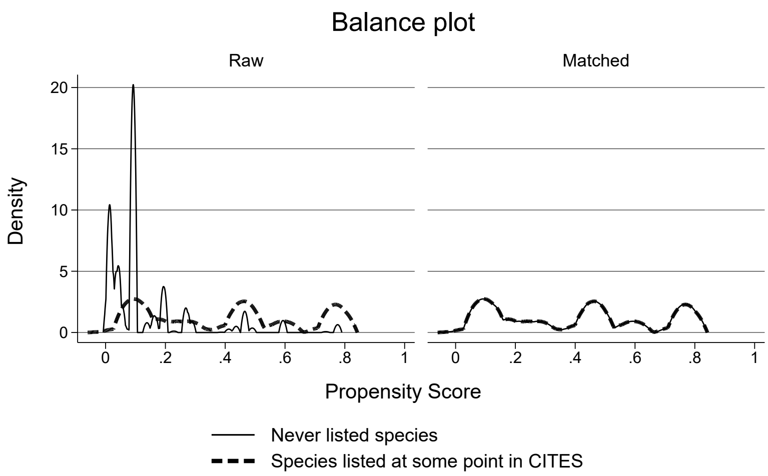
*Notes*: Appendix Table 3 reports estimated regression coefficients from a panel regression of log of population size on various treatment dummies along a set of population and year fixed effects for a subsample of vulnerable species. Standard errors are in parentheses and are clustered at the species level. Column (1) estimates Equation (2), i.e., a regression in which the treatment dummy equals one for populations of species listed in CITES. Column (2) includes a variation of the treatment dummy that equals one for populations of species listed in CITES in 1975. Column (3) includes a variation of the treatment dummy that equals one for species listed in CITES after 1975. Column (4) includes both treatment dummies for species listed in CITES in 1975 and after 1975. Column (5) interacts the treatment dummy from column (1) with a dummy variable that is one for populations located in non-sanctioned countries. Column (6) interacts the treatment dummy with $1 - P(BRIBE{=}1)$, where $P(\cdot)$ is the probability of corruption at the border. Column (7) interacts the treatment dummy with a dummy variable that is one when a country has a low level of corruption. Column (8) interacts the treatment dummy with a dummy variable that is one for populations located in high-income countries. Column (9) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES member country. Column (10) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES Category 1 member country, i.e., a country with high levels of enforcement. Column (11) includes several regressors simultaneously.

Appendix Table 4: Covariate balance summary statistics for the matched sample of species

| | Standardized differences | | Variance ratio | |
|---|---|---|---|---|
| | raw | matched | raw | matched |
| mammal | 0.777 | -0.000 | 2.320 | 1.000 |
| bird | -0.145 | 0.000 | 0.957 | 1.000 |
| reptile | 0.223 | -0.000 | 2.231 | 1.000 |
| amphibian | -0.352 | -0.000 | 0.063 | 1.000 |
| vulnerable | 0.777 | -0.000 | 2.457 | 1.000 |
| intentional use | 0.724 | -0.000 | 1.257 | 1.000 |
| fishing | -0.334 | -0.000 | 0.338 | 1.000 |

*Notes*: Appendix Table 4 shows the standardized differences and variance ratios for the covariates used to identify the matched species for the nearest neighbor covariate matching used to preprocess the data before calculating the regressions reported in Appendix Table 5.

Appendix Figure 2: Balance plot for the matched sample of species




This figure shows a balance plot for the raw data and the matched sample of species where the propensity score, i.e., the probability that a species is listed in Appendix I or II of CITES at some point in time, is estimated using a logit model and using the same covariates as in column (4) of Appendix Table 1. We use this propensity score for the caliper to preprocess the data before calculating the regressions reported in Appendix Table 5. Density plots use an optimal Epanechnikov kernel. Note that the estimated propensity score lies strictly within [0;1] for all species (minimum value is 0.006). The apparent occurrence of values $< 0$ is only due to the smoothing behavior of the kernel density estimator and does not affect the matching parameter estimates.

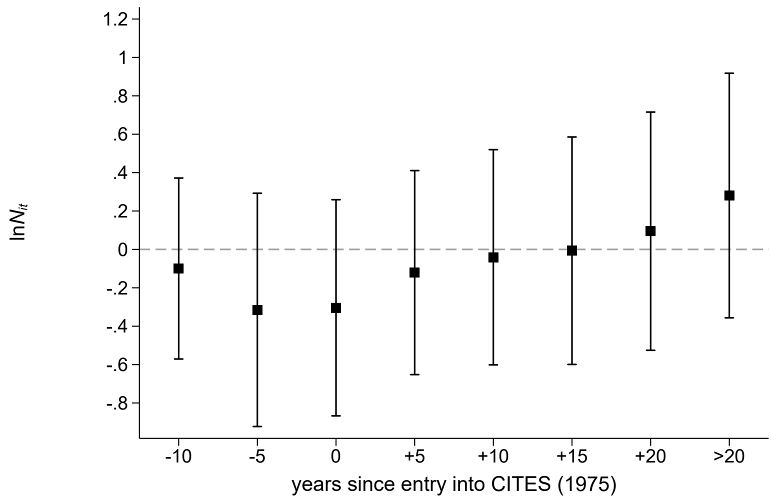
Appendix Table 5: Effect of CITES on population size (species listed in CITES) for the matched sample of species

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| in CITES | 0.048 | | | | 0.192 | −1.130 | −0.196 | −0.080 | 0.032 | 0.034 | −0.047 |
| | (0.113) | | | | (0.168) | (0.383) | (0.134) | (0.114) | (0.125) | (0.120) | (0.176) |
| in CITES in 1975 | | 0.228 | | 0.222 | | | | | | | |
| | | (0.144) | | (0.147) | | | | | | | |
| in CITES after 1975 | | | −0.090 | −0.010 | | | | | | | |
| | | | (0.119) | (0.125) | | | | | | | |
| in CITES × *NONSANCTIONED* | | | | | −0.144 | | | | | | −0.137 |
| | | | | | (0.123) | | | | | | (0.123) |
| in CITES ×$(1 - P(BRIBE{=}1))$ | | | | | | 1.532 | | | | | |
| | | | | | | (0.476) | | | | | |
| in CITES ×$(1{-}CORRUPT)$ | | | | | | | 0.446 | | | | 0.171 |
| | | | | | | | (0.181) | | | | (0.169) |
| in CITES × *HIGH-INCOME* | | | | | | | | 0.290 | | | 0.196 |
| | | | | | | | | (0.106) | | | (0.104) |
| in CITES × *MEMBER* | | | | | | | | | 0.024 | −0.304 | −0.139 |
| | | | | | | | | | (0.103) | (0.129) | (0.143) |
| in CITES × *MEMBER* × *CATEGORY 1* | | | | | | | | | | 0.440 | 0.270 |
| | | | | | | | | | | (0.184) | (0.158) |
| $N$ | 13645 | 13645 | 13645 | 13645 | 13645 | 11579 | 13076 | 13645 | 13645 | 13645 | 13076 |

*Notes*: Appendix Table 5 reports estimated regression coefficients from a panel regression of log of population size on various treatment dummies along a set of population and year fixed effects for a subsample of populations of matched species. We identify matched species for all species included into CITES using nearest neighbor covariate matching with replacement using a Mahalanobis distance metric and a caliper of one standard deviation of the estimated propensity score using column (4) of Appendix Table 1. For the covariate matching, we also use the regressors from column (4) of Table Appendix Table 1. We trim the sample by dropping species with a propensity score outside [0.1;0.9]. Standard errors are in parentheses and are clustered at the species level. Column (1) estimates Equation (2), i.e., a regression in which the treatment dummy equals one for populations of species listed in CITES. Column (2) includes a variation of the treatment dummy that equals one for populations of species listed in CITES in 1975. Column (3) includes a variation of the treatment dummy that equals one for species listed in CITES after 1975. Column (4) includes both treatment dummies for species listed in CITES in 1975 and after 1975. Column (5) interacts the treatment dummy from column (1) with a dummy variable that is one for populations located in non-sanctioned countries. Column (6) interacts the treatment dummy with $1 - P(BRIBE{=}1)$, where $P(\cdot)$ is the probability of corruption at the border. Column (7) interacts the treatment dummy with a dummy variable that is one when a country has a low level of corruption. Column (8) interacts the treatment dummy with a dummy variable that is one for populations located in high-income countries. Column (9) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES member country. Column (10) interacts the treatment dummy with a dummy variable that is one when a population is located in a CITES Category 1 member country, i.e., a country with high levels of enforcement. Column (11) includes several regressors simultaneously.

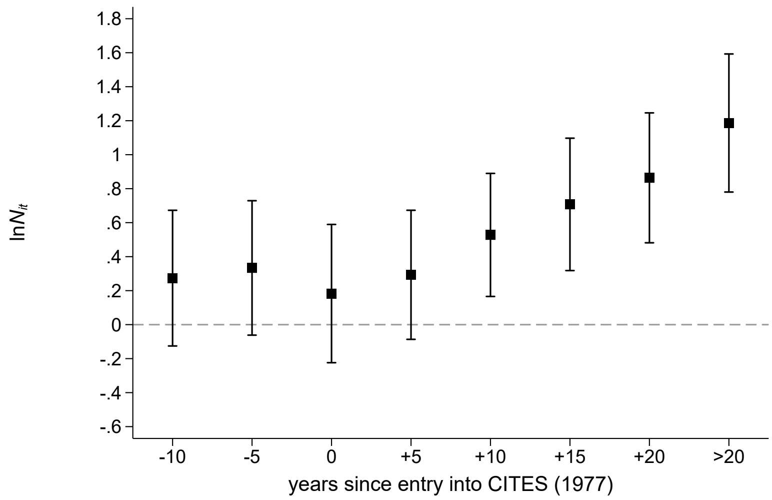
# E Separate effects for species listed in the first three cohorts (1975, 1977 or 1979), excluding species listed in CITES in other years

Appendix Figure 3: Effect of CITES on population size (species listed in CITES in 1975). Excluding species listed in CITES in other years



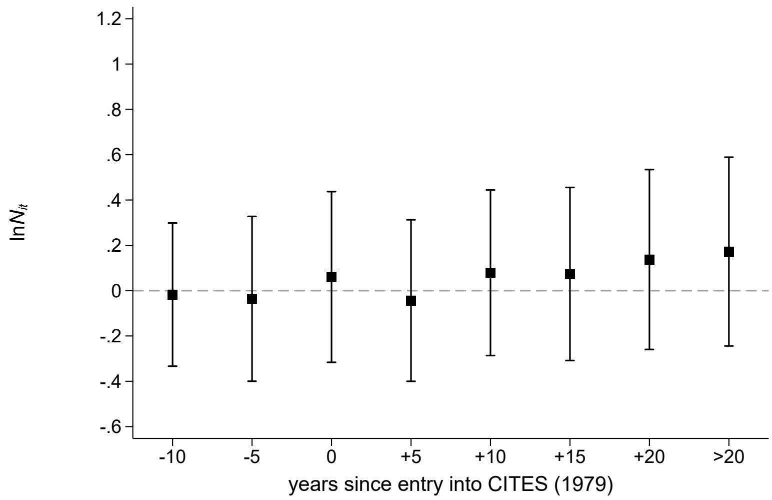

This figure shows coefficient estimates from Equation (3), i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population and year fixed effects. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 109923.

Appendix Figure 4: Effect of CITES on population size (species listed in CITES in 1977). Excluding species listed in CITES in other years




This figure shows coefficient estimates from Equation (3), i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population and year fixed effects. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 100092.

Appendix Figure 5: Effect of CITES on population size (species listed in CITES in 1979). Excluding species listed in CITES in other years




This figure shows coefficient estimates from Equation (3), i.e., a panel regression of log of population size on a set of dummy variables indicating the years since a species' entry into CITES, along with a set of population and year fixed effects. 95% confidence intervals are displayed around each point estimate. Standard errors are clustered at the species level. Number of observations: 107194.

# F Species-type specific treatment effects

Appendix Table 6: Effect of CITES on population size. Species-type specific treatment effects for species listed in CITES in CITES' member countries

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *for non-"Category 1" member countries* | | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| $>$ 20 years later | −0.006 | −0.067 | −0.170 | −0.154 | −0.203 |
| | (0.150) | (0.183) | (0.093) | (0.082) | (0.102) |
| ...for species with intentional use | −0.126 | | | | |
| | (0.178) | | | | |
| ...for vulnerable species | | −0.049 | | | |
| | | (0.208) | | | |
| ...for highly-studied species | | | 0.178 | | |
| | | | (0.105) | | |
| ...for well-known species | | | | 0.124 | |
| | | | | (0.074) | |
| ...for large species | | | | | 0.323 |
| | | | | | (0.154) |
| $N$ | 103429 | 100712 | 119538 | 119538 | 119538 |
| *for "Category 1" member countries* | | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| $>$ 20 years later | 0.389 | 0.377 | 0.268 | 0.309 | 0.333 |
| | (0.106) | (0.088) | (0.060) | (0.069) | (0.077) |
| ...for species with intentional use | −0.157 | | | | |
| | (0.130) | | | | |
| ...for vulnerable species | | −0.213 | | | |
| | | (0.118) | | | |
| ...for highly-studied species | | | 0.140 | | |
| | | | (0.095) | | |
| ...for well-known species | | | | 0.061 | |
| | | | | (0.045) | |
| ...for large species | | | | | −0.039 |
| | | | | | (0.116) |
| $N$ | 103429 | 100712 | 119538 | 119538 | 119538 |

*Notes*: Appendix Table 6 reports coefficient estimates of a regression which uses $\ln N_{slt}$ as the dependent variable and includes separate treatment dummies that indicate populations of species listed at least 21 years in CITES' appendices and located in member countries classified as non-"Category 1" countries and "Category 1" countries, as well as interaction terms of these dummies with species-level dummies which identify different types of species. Column (1) estimates the regression including an interaction term of the separate treatment dummies with a dummy variable that equals one for species with intentional use. Column (2) includes an interaction for vulnerable species, i.e., when they are classified as either "critically endangered", "endangered", or "vulnerable" by the IUCN Red List. Column (3) includes an interaction with highly-studied species, i.e., with a higher than average number of studies per species and year. Column (4) includes an interaction with well-known species, i.e., with a higher than average number of identification agreements by the users of ***iNaturalist*** per species and year. Column (5) includes an interaction with large species, i.e., with a higher than average body size. All regressions contain population and year fixed effects. Standard errors are in parentheses and are clustered at the species level.

# G Alternative specifications for country-specific trends

Appendix Table 7: Effect of CITES on population size

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| 6-10 years before | 0.030 | 0.053 | 0.053 | 0.079 | 0.084 | 0.079 |
| | (0.099) | (0.087) | (0.087) | (0.088) | (0.089) | (0.089) |
| 1-5 years before | −0.056 | −0.016 | −0.016 | 0.019 | 0.029 | 0.023 |
| | (0.121) | (0.102) | (0.102) | (0.102) | (0.102) | (0.102) |
| year of listing in CITES | −0.073 | −0.041 | −0.041 | −0.002 | 0.003 | −0.003 |
| | (0.122) | (0.113) | (0.113) | (0.113) | (0.113) | (0.113) |
| 2-5 years later | −0.002 | 0.032 | 0.033 | 0.057 | 0.063 | 0.056 |
| | (0.121) | (0.107) | (0.107) | (0.108) | (0.108) | (0.108) |
| 6-10 years later | 0.104 | 0.161 | 0.162 | 0.167 | 0.174 | 0.165 |
| | (0.127) | (0.108) | (0.109) | (0.109) | (0.109) | (0.110) |
| 11-15 years later | 0.158 | 0.222 | 0.222 | 0.236 | 0.241 | 0.234 |
| | (0.140) | (0.116) | (0.116) | (0.115) | (0.116) | (0.116) |
| 16-20 years later | 0.271 | 0.337 | 0.337 | 0.364 | 0.368 | 0.362 |
| | (0.150) | (0.120) | (0.120) | (0.121) | (0.121) | (0.120) |
| $\geq$ 20 years later | 0.461 | 0.512 | 0.512 | 0.541 | 0.544 | 0.539 |
| | (0.165) | (0.123) | (0.123) | (0.123) | (0.123) | (0.123) |
| $N$ | 119538 | 119538 | 119538 | 119538 | 119538 | 119538 |
| order of polynomial of time | 0 | 1 | 2 | 3 | 4 | 5 |

*Notes*: Appendix Table 7 reports estimated regression coefficients and standard errors in parentheses. Standard errors are clustered at the species level. Dependent variable $\ln N_{slt}$. All regressions contain population and year fixed effects. For comparison, column (1) presents results of Equation (3). Columns (2)-(6) include country-specific (non-)linear trends modelled as polynomials of order 1 to 5 of time, respectively.

# H Effect of CITES controlling for domestic regulation

Appendix Table 8: Effect of CITES on population size controlling for domestic regulation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| domestic regulation | -0.112 | | −0.215 | | −0.199 |
| | (0.226) | | (0.218) | | (0.194) |
| in CITES | | 0.211 | 0.212 | | |
| | | (0.061) | (0.061) | | |
| 6-10 years before | | | | 0.086 | 0.086 |
| | | | | (0.098) | (0.098) |
| 1-5 years before | | | | 0.041 | 0.041 |
| | | | | (0.105) | (0.105) |
| year of listing in CITES | | | | 0.012 | 0.013 |
| | | | | (0.112) | (0.112) |
| 2-5 years later | | | | 0.075 | 0.075 |
| | | | | (0.106) | (0.106) |
| 6-10 years later | | | | 0.167 | 0.168 |
| | | | | (0.111) | (0.111) |
| 11-15 years later | | | | 0.244 | 0.245 |
| | | | | (0.119) | (0.119) |
| 16-20 years later | | | | 0.408 | 0.408 |
| | | | | (0.122) | (0.122) |
| >20 years later | | | | 0.579 | 0.579 |
| | | | | (0.126) | (0.126) |
| $N$ | 118106 | 118106 | 118106 | 118106 | 118106 |

*Notes*: Appendix Table 8 reports detailed results of regressions controlling for domestic regulation presented in the main text. Standard errors are in parentheses and are clustered at the species level. Columns (1) to (3) are shown for comparison only and are identical to columns (1) to (3) in Table 3 in the main text, i.e., they present results of panel regressions of log of population size on a set of dummy variables indicating the years since a species' entry into Appendix I or II of CITES, along with a set of population and country-year fixed effects, as well as a dummy that equals one for populations of species located in countries with domestic regulation protecting the respective species (i.e., for countries listing the species in Appendix III). Column (4) presents point estimates underlying the event study graph presented in the right panel of Figure 8, i.e., not controlling for domestic regulation. Column (5) presents point estimates underlying the event study graph presented in Figure 9, i.e., controlling for domestic regulation.

# References

Metrick, A. and Weitzman, M. L. (1996). Patterns of Behavior in Endangered Species Preservation. *Land Economics*, 72(1):1–16.