ECONOMIC PRODUCTION AND BIODIVERSITY IN THE UNITED STATES

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ABSTRACT

Species extinctions and ecological degradation are accelerating to a degree unprecedented in human history. Despite such trends, causal evidence for economic drivers of biodiversity losses and effective policy responses remains sparse. We study the relationship between economic production and biodiversity using a novel dataset of wildlife at thousands of locations across the United States between 1960 and 2015. Leveraging two different government policies that influence the local economy through military procurement and environmental regulations, we show that greater economic production adversely affects biodiversity. We estimate that pollution externalities can explain a third of the observed link between biodiversity and production.

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1   Introduction

Recent decades have been marked by increasing ecological degradation. Extinction rates are currently orders of magnitude greater than baseline, non-anthropogenic levels, and some scientists have claimed that we are entering a new human-caused age of mass extinction (e.g., Pimm et al., 2014; Ceballos et al., 2015). Mitigating further ecological damage requires us to understand how economic development has contributed to these recent trends (Frank and Schlenker, 2016). This paper contributes to this understanding by establishing the causal link between economic production and biodiversity outcomes. We further provide evidence that air pollution is a key mechanism behind human-caused ecological degradation.

A major obstacle that has impeded attempts to empirically study the biodiversity-economics link is the lack of large-scale biodiversity data (e.g., Geijzendorffer et al., 2016). We make progress by using a novel database containing hundreds of studies that maintain *assemblage time-series*: academic research studies that measure the count or biomass of relevant species in a particular area over time, with some studies spanning several decades (BioTIME: Dornelas et al., 2018; Blowes et al., 2019).[1] Importantly, all included studies have consistent sampling protocols within each location over time. This means that the year-over-year variation in biodiversity metrics at the same sampling location reflects genuine changes in underlying biodiversity conditions, rather than changes in sampling methods. In total, the database contains millions of records of species counts at the species-location-year level at over 15,000 different locations. The data allows us to observe significant fractions of the total number of species in different taxonomic classes, which we call taxa. For example, we observe 80 percent of the known bird species, 40 percent of mammals, 30 percent of amphibians, and 25 percent of freshwater fish.

Our analysis examines how year-over-year changes in economic output in the United States relate to biodiversity outcomes at a given location. Our focal measure of economic output is state level GDP per capita. We examine three fundamental metrics of biodiversity: species *abundance*, which measures the total quantity of individuals that were observed at a given location in a given year; species *richness*, which measures the total number of unique species that were observed; and *Jaccard similarity*, which measures the share of common species between two adjacent years of a study and captures the stability of the composition of species.

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[1]Biodiversity data collection often faces a trade-off between fine-scale precision and representativeness across spatial or species scales (König et al., 2019). Commonly used datasets either provide only cross-sectional information on the geographic extent of species as in the International Union for Conservation of Nature Red List of Threatened Species (IUCN, 2021), or they provide panel data but only for a single taxonomic class, as in the North American Breeding Bird Survey (USGS, 2014).

Our baseline analysis is a fixed effects model with one of the biodiversity metrics as the outcome variable, GDP per capita as the regressor, and a set of location-taxon fixed effects and year fixed effects. This exercise documents a strong, negative association between economic production and biodiversity outcomes. The ordinary least squares (OLS) estimates for the GDP elasticity of the different metrics are -3.6 for abundance, -1.6 for richness, and -0.1 for similarity. The adverse effects of economic production appear to be pervasive. We find negative elasticities across multiple taxa such as amphibians, birds, fish, freshwater plants and invertebrates, and mammals. The effects emerge in areas with different levels of economic development: the negative elasticities are not statistically different across areas with the lowest, middle, or highest quintiles of sample-average GDP per capita.[2] The relationships exhibit significant distributional heterogeneity: at the lowest decile of biodiversity, the negative effects of local economic production are almost twice as large as the average effects. This finding suggests that economic development exacerbates biodiversity losses to the greatest degree in areas where biodiversity is already low. It is also consistent with the view that biodiversity itself may increase resilience to adverse shocks to the ecosystem (Hautier et al., 2015).

Our OLS results provide evidence of an association between biodiversity and economic output. To move toward a causal interpretation, we tease out changes in local economic production that come from plausibly exogenous government policies, and we examine how biodiversity outcomes respond to these quasi-experimental shocks. We focus on two policies that are well-studied in the economics literature. First, we exploit variation in local economic production driven by U.S. military buildups, which are heavily influenced by geopolitical factors and unexpected military events. Military buildups have been widely leveraged in the empirical macroeconomics literature to estimate the causal effect of government spending on total economic output, i.e., the fiscal multiplier (e.g., Hall, 2009; Barro and Redlick, 2011; Ramey, 2011; Nakamura and Steinsson, 2018). We follow this literature and exploit systematic heterogeneity in a state's receipt of federal military spending (and therefore in the state's output) in response to the national military shock (Nakamura and Steinsson, 2014). Our estimates show that increased local economic production through military spending deteriorates biodiversity outcomes. Our second research design exploits environmental regulatory shocks. We examine a series of tightenings of Clean Air Act environmental standards across U.S. states and counties since the 1990s. These policy shocks imposed sweeping emission abatement requirements on jurisdictions in violation of a set of outdoor air quality standards,

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[2]There is thus limited evidence within our study scope that the effect of economic production on biodiversity exhibits an Environmental Kuznets Curve (Grossman and Krueger, 1995). See, for example, Harbaugh et al. (2002) and Lin and Liscow (2013) for evidence in other settings.

causing substantial reductions in local output and improvements in air quality (e.g., Becker and Henderson, 2000; Greenstone, 2002; Greenstone et al., 2012; Walker, 2013). We show these regulations significantly improved biodiversity outcomes.

The biodiversity-GDP elasticity estimates capture multiple mechanisms – such as air pollution, water pollution, land use change, and so forth – through which production activities affect biodiversity. Empirically distinguishing all possible mechanisms is challenging. Here we focus on causally identifying one specific channel: air pollution externalities associated with economic production.

We measure the importance of air pollution in two steps. First, we produce causal estimates of the elasticities between biodiversity outcomes and air pollution. We use a research design that isolates variation in local pollution driven by transported pollution from distant, upwind cities (e.g., Deryugina et al., 2019; Anderson, 2020). We show that "upwind pollution" coming from areas over 300 km away generates substantial variation in local air quality, and these imported pollution shocks cause reductions in local biodiversity outcomes. Our instrumental variables (IV) estimates suggest biodiversity-pollution elasticities of -1.1 for abundance, -0.6 for richness, and under -0.1 for similarity. Second, we estimate the impact of the government policy shocks (both military spending and environmental regulations) on air pollution, and multiply these estimates by the biodiversity-pollution elasticities we obtain from step one. Together, these exercises give us the expected impact of the policy shocks on biodiversity *through air pollution*. We find that pollution accounts for 20-60 percent of the estimated total effect of the policy, suggesting air pollution is a first-order pathway for how biodiversity is affected by economic production.

Biodiversity has important ecological and economic values. Higher levels of biodiversity are associated with enhanced ecosystem stability and resilience, and can affect the physical environment such as local temperature (Tilman et al., 1996; Cottingham et al., 2001; Missirian et al., 2019). More diverse ecosystems also tend to have improved natural processes, such as nutrient recycling (Cardinale et al., 2012). Biodiversity is associated with numerous economic benefits (Brock and Xepapadeas, 2003). For example, restoration of biodiversity in the fisheries sector in recent decades has been associated with substantial increases in productivity (Worm et al., 2006); greater biodiversity in organisms such as pollinators is associated with enhanced crop yields (Dainese et al., 2019); higher plant species diversity helps mitigate households' income shocks from natural hazards such as droughts (Noack et al., 2019); biodiversity has critical value for pharmaceuticals where successful drug discovery hinges on molecular diversity (Simpson et al., 1996; Rausser and Small, 2000; Costello and Ward, 2006); and nature and biodiversity preservation also have significant non-use and

non-market value (Loomis and White, 1996; Kolstoe and Cameron, 2017).

Massive wildlife losses in recent decades highlight a pressing need to examine the trade-offs between economic growth and conservation (Foster and Rosenzweig, 2003; Polasky et al., 2005; Frank and Schlenker, 2016; Jayachandran et al., 2017; Ando and Langpap, 2018; Dasgupta, 2021). We add to the existing literature that investigates these tradeoffs for specific taxa such as birds, fish, and forest plants (Sumaila et al., 2019; Li et al., 2020; Liang et al., 2020; Garg and Shenoy, 2021; Noack et al., 2021), and which often focuses on the impact of land use change (Newbold et al., 2015; Asher et al., 2020; Daskalova et al., 2020; Cole et al., 2021). This research advances upon the current literature by analyzing the impacts of general economic production across a wide range of taxa at a national scale, and by taking advantage of quasi-experimental research designs to recover causal relationships (Dietz and Adger, 2003; Czech et al., 2012). We also add to the literature by pinning down air pollution as a critical mechanism underlying the relationship between economic production and biodiversity. This finding complements a well-established literature documenting air pollution's deleterious effects on humans (Chen et al., 2013; Dominici et al., 2014; Schlenker and Walker, 2016; Deryugina et al., 2019) and a limited area of research showing large-scale negative effects of industrial pollution on birds (Liang et al., 2020). Our results further indicate that there are additional policy levers at our disposal for enhancing conservation efforts, and also that the benefits of air pollution regulations are underestimated by ignoring the non-market ecological benefits.

The paper is organized as follows: Section 2 describes the data. Section 3 discusses properties and caveats of the biodiversity and economic measurements. Section 4 presents the baseline panel estimation of the GDP-biodiversity link. Section 5 presents estimates of the effect of two government policies. Section 6 examines the air pollution channel. Section 7 concludes the paper.

2   Data

2.1   BioTIME Database

To help readers conceptualize the data structure, we begin with a brief description of two example studies included in BioTIME. We also use these examples at other points in the paper to provide references when helpful.

Example Study 1: North American Breeding Bird Survey (Birds) The North American Breeding Bird Survey (BBS) is a long-term and large-scale monitoring program that tracks the status and trends of North American bird populations (USGS, 2014). The BBS follows a regular and consistent sampling (observing) protocol. Skilled bird observers collect observation data at the same stops along the roadside survey routes during the avian breeding season every year (June for the most part of the United States). Each survey route is approximately 24.5 miles long, with stops situated about a half mile apart. At each stop, a three-minute point count is conducted. During the count, observers record every bird heard or seen within a 0.25-mile radius. Surveys start one and a half hours before local sunrise and take about five hours to complete. Over 4,100 survey routes are located across the continental United States and Canada (Figure A.1). BBS is perhaps the most widely used data source in the study of birds; as of this writing, it has been used in over 450 scientific publications.

Example Study 2: Sevilleta Long-Term Ecological Research (Mammals) Several studies included in BioTIME are conducted under the Sevilleta Long-Term Ecological Research (SLTER) Program at the Sevilleta National Wildlife Refuge in central New Mexico (Figure A.2). The habitats, about 100,000 hectares in size, represent five regional biomes that extend through much of the central and western United States and northern Mexico. One example study in the program is its small mammals census, conducted from 1989 to 2008 (Friggens, 2008). In this study, permanent trapping stations were used to collect observational data about small mammals which were collected two to three times yearly across different seasons in multiple habitats within the refuge. During the sampling period, trapping webs, each consisting of 120 permanently marked trapping stations, were deployed for three consecutive nights. Sherman live traps (boxes that capture the animal without harming it) were placed at each station, with four traps placed at the center of the web. Upon capture, each individual is marked either permanently with tags or temporarily with Sharpies. There are 16,657 records for 27 distinct species covered in the study. The most commonly observed species is *Dipodomys merriami* (Merriam's kangaroo rat).

BioTIME has a number of features making it useful for our analysis. First, the studies included in the dataset all maintained consistent sampling protocols over time, ensuring that within-study variation in outcomes does not stem from changes in how species are detected by the researchers (e.g., sighting versus trapping). Second, each study included in the panel dataset has at least two years of sampling, and some studies span multiple decades. This allows us to exploit within-study variation, and to control for any differences across studies

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in sampling protocols with location fixed effects. Third, the dataset contains information on about 40,000 unique species or genus at over 15,000 sampling locations, spanning a wide range of biomes and ecosystems. While such coverage is by no means comprehensive compared to the overall ecosystem (we observe 80 percent of the known bird species, 40 percent of mammals, 30 percent of amphibians, 25 percent of freshwater fish, among smaller coverage for other taxa), we believe the data let us gain by far the best understanding of which kinds of organisms and biomes are affected by economic production.

We make several sample restrictions. First, we focus on years after 1966 to line up with our economic data. Second, we exclude marine ecosystem studies, which often take place tens or hundreds of miles offshore and, thus, are difficult to link to measures of economic production. Third, we aggregate the raw species sampling observations to the taxon-location-year level. The included taxa are birds, fish, mammals, terrestrial invertebrates, freshwater invertebrates, terrestrial plants, and freshwater plants.[3] Fourth, we focus on studies that report species abundance (i.e., counts of individuals), and exclude studies that only report total species biomass or only an indicator for species presence so that we have a unified measure of abundance. Last, we focus on studies in the United States. The United States accounts for about three-quarters of the total observations in the dataset; the next largest contributor, New Zealand, accounts for about 10 percent. Focusing on the United States also allows us to implement several well-understood quasi-experiments in the causal analysis. These sample restrictions give us a maximum of 66,418 taxon-location-year observations.

2.2   Measures of Biodiversity

Biodiversity is a multi-faceted concept. In this paper, we focus on three metrics: abundance, species richness, and the Jaccard similarity index. In the appendix, we report findings using other common measures of biodiversity.

Abundance is the total number of individuals observed in a given taxon, at a given location, in a given year. Abundance simply measures the pure quantity of individuals observed and is agnostic about the types of species in the sample.

Species richness is the total number of unique species present in a given taxon, at a given location, in a given year. This metric is agnostic about the composition of species or how the composition of species has changed. For example, if there is an equal loss of native species and introduction of invasive species, species richness will not change despite changes in the composition of the ecosystem.

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[3]Our raw sample also includes 30 reptile observations at one location, and one observation at a second location. Given how little data we have on reptiles we drop them from the sample.

Jaccard similarity is an inverse measure of the amount of year-to-year species turnover in a given taxon, at a given location; thus, it provides a measure of compositional changes. Let $\mathbb{S}_{ct}$ be the set of species at some time t in taxon j and location c, and let $n(\cdot)$ denote the cardinality of a set. The Jaccard similarity index is given by:

$$J_{cjt} = \frac{n(\mathbb{S}_{cjt+1} \cap \mathbb{S}_{cjt})}{n(\mathbb{S}_{cjt+1} \cup \mathbb{S}_{cjt})},$$

the number of species in taxon j present at location c in both times t and $t+1$ relative to the number of species present in either of the two times. The index is bounded between zero and one with lower values indicating some combination of loss of species, or the introduction of new species between times t and $t+1$. In the limiting case where $J_{cjt} = 0$, there are no common species in times t and $t+1$ (but there still may be observed species at the location); on the other hand, a similarity index value of $J_{cjt} = 1$ indicates that the exact same set of species is present in times t and $t+1$ and there is no species turnover.

We note three points here before proceeding to our summary statistics: First, we use log abundance and log species richness as our metrics. This allows us to interpret our coefficients as elasticities. We do not take a logarithm of the Jaccard similarity index since it is already a ratio. Second, for succinctness, from here on we refer to all three metrics together as biodiversity measures, although they are three distinct concepts. When referring to them individually, we make clear whether we are referring specifically to abundance, richness, or similarity. Third, we interpret declines in the three measures as worsening biodiversity outcomes; our reasoning is that declines in these measures indicate reductions in populations and diversity, and/or changes in composition.

Summary Statistics Table 1 reports summary statistics calculated based on taxon-location-year observations. Column 1 reports number of observations, which shows that roughly 80 percent of our observations are birds. To make sure that our results are not driven by a single taxon, we report two sets of results for our analyses: one uses the full estimation sample including all species, and the other uses a subsample that excludes birds. Our results are typically similar or stronger for the sample that excludes birds.

Column 2 shows the mean and the standard deviation of abundance by taxa. The large differences in abundance across taxa partly reflect differences in study scope as well as sampling methods. For example, consider the North Temperate Lakes Long-Term Ecological Research Program, a study that falls in the taxon category of "freshwater invertebrates." The study samples zooplankton at the deepest location of lakes in Madison, Wisconsin, by

pulling a conical net vertically through the water column, generating large abundance counts. By contrast, most studies on mammals use Sherman traps, which capture individual animals one at a time. For example, the small mammals study included in the SLTER in central New Mexico (Section 2.1) reports only 27 unique species despite its 20-year time span. As previously noted, in all regression specifications we include taxon-by-location fixed effects to ensure the identifying variation comes from year-to-year changes in biodiversity outcomes and economic conditions, holding study protocols and taxon constant.

Columns 3 and 4 show the statistics for species richness and Jaccard similarity. Freshwater plants have the highest species richness, and mammals have the lowest richness. The average Jaccard similarity index is 0.425 among all species, indicating that around half of the species at a sampling location are observed in the next year of the study. The highest species turnover (or the lowest similarity) takes place among mammals; the lowest species turnover occurs among amphibians.

Figures 1 and 2 further break down these summary statistics. Figure 1 shows, by taxa, the spatial distribution of sampling locations (upper panel), total number of sampling locations (lower-left panel), and number of taxon-location-year observations (lower-right panel). In practice, depending on the geographic scope of the study, many sampling locations may be close to each other but they may follow different sampling protocols; an example is given in the SLTER mammal study of Appendix Figure A.2. This explains why there appear to be many sampling locations but limited overall geographic coverage, especially for non-bird species. Because distinct sampling locations in the BioTIME data represent different study protocols, in our primary analysis we treat them as separate cross-sectional units.

Figure 2 further summarizes annual *changes* in abundance, species richness, and Jaccard similarity. The scatterplot on the left shows that changes in abundance and species richness are positively correlated; by contrast, their correlations with the change in the composition of species represented by the Jaccard similarity index can be of either sign. The right panel of Figure 2 shows the distributions of annual changes in the three biodiversity metrics. The vast majority of the observations fall within plus or minus one log unit range.

2.3 Economic and Environmental Data

We briefly describe the data sources from which we build the economic and environmental variables. All sources we use are standard in the literature and are publicly available.

Economic Output We measure local changes in economic production using annual state level per capita GDP from the Bureau of Economic Analysis (BEA) from 1966 to 2015. We use state-level output measures throughout the paper to capture overall economic changes in the area, so that our estimates do not reflect small-scale spatial displacement, for example, when individual animals move to a nearby location in response to a rapid deterioration of the local environment. The state level measure also allows us to match geographic resolution of some key variables in the causal inference, such as the instrumental variable on state military contracting spending. In several robustness specifications, we use county-level income from the BEA in place of state GDP.

Military Spending State level annual military spending and federal prime contracting data are from Nakamura and Steinsson (2014). The military spending data, sourced from the U.S. Department of Defense from 1966 to 2006, contain all types of military purchases such as purchase of aircraft and repairs of military facilities.[4] The federal prime contracting data identify locations where the majority of work was performed, so that one can attribute the economic production associated with the procurement to the states. The national average military spending accounts for around 3 percent of GDP; this share varies widely across states, from 10 percent in Virginia to 1 percent in Oregon.

Pollution Pollution data come from the Modern-Era Retrospective analysis for Research and Applications, Version 2 (MERRA-2) reanalysis dataset maintained by the U.S. National Aeronautics and Space Administration (NASA), where satellite and ground-based measurements of aerosol optical depth (AOD) – a measure of the amount of particles in a column of air between the top-of-atmosphere and the ground – are assimilated into Earth system modeling (product ID: M2I3NXGAS Version 5.12.4). MERRA-2 provides daily measures of AOD on a 30km-by-30km grid, approximately the same area as the average county.

Clean Air Act Nonattainment Designation Annual designation of attainment and nonattainment areas are sourced from the U.S. Environmental Protection Agency (EPA) Greenbook for years 1992 to 2015. The data contain designation information for six criteria pollutants: $PM_{2.5}$, PM_{10}, O_3, SO_2, CO, and Pb. For a given pollutant, there may be multiple versions of standards that differ, for example, by the target metric (average or maximum concentration) or the year of initial promulgation (most pollution standards tighten over

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[4]These data are based on Department of Defense DD-350 military procurement forms that document all types of military purchases greater than a certain amount. In total, the data cover 90 percent of all military purchases in the United States.

time). There are in total 12 standards during our study period. A jurisdiction (mostly county) can be in nonattainment with multiple standards in a given year. Figure 5 Panel B plots the location of nonattainment areas as of year 2019.[5]

Land Cover We measure urbanization between 2001 and 2015 using satellite data on land cover type from the Moderate Resolution Imaging Spectroradiometer (MODIS) platform maintained by NASA. The MODIS land-cover-type product is based on an ensemble-supervised classification algorithm (Friedl et al., 2010). The key inputs to the algorithm include satellite-based measurements of surface reflectance and surface temperature. The training data contain over 1,800 manually labeled sites (mostly obtained from Landsat satellite imagery) around the world. We use the yearly Land Cover Type product (product ID: MCD12C1), which classifies land cover into 17 types, such as urban and rainforest, at a spatial resolution of 0.05 degrees (about 5,600 meters). Our urbanization measure is the share of urban land within a 50-km or 100-km radius, or the county of the BioTIME sampling site.

Conservation Protected Areas Spatial data on protected area are obtained from the World Database on Protected Areas (WDPA). The data is in the form of a shapefile that outlines the location of over 250,000 marine and terrestrial protected areas and the year that protective measures were put in place for each area. To capture conservation efforts in the vicinity of the sampling sites, we compute the share of land within a 50-km radius of the sampling site that is under protection in each year. We also calculate the number of discontiguous protected areas within this radius to measure fragmentation of the conservation effort.

3 Measurement

Before we go into the econometric analysis, some discussions on the features and limitations of the data are in order. In Section 3.1, we discuss what an "ideal" biodiversity dataset looks like, how BioTIME departs from that ideal, and how these departures may influence internal and external validity of this study. In Section 3.2, we propose a set of diagnostic tests and robustness checks.

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[5]Each year, a jurisdiction's past three-year air quality metrics are calculated using data from *in situ* outdoor air pollution monitors within its administrative border. The calculated metrics, known as the "design values", are then compared with the national air quality standards. Nonattainment status is triggered when a design value exceeds the corresponding standard. Most designations occur at the county level, although nonattainment status can occasionally be assigned at a sub-county level or at a broader metropolitan-area level.

3.1 Potential Sources of Bias

In an ideal biodiversity dataset, one chooses a random set of locations across the country, and continuously monitors categories and quantities of all animal and plant species observed over time. The monitoring technology at any given location should remain fixed, so that changes in species observations reflect genuine changes in the underlying biodiversity conditions rather than changes in the monitoring procedure. BioTIME departs from this ideal as it contains observations for locations, years, and species that the collection of ecological studies chose to focus on; though by construction, BioTIME only includes studies that adopted fixed sampling protocols, many studies span several decades, and thus sampling technologies may have evolved over time.[6] We next discuss the implications of these departures.

Location Studies choose sites based on different objectives and criteria, and some studies – especially those that study birds – tend to have larger geographic coverage than others (Figure 1). Nonrandom geographic coverage is an intrinsic limitation of virtually all biodiversity data (Hortal et al., 2007; Geijzendorffer et al., 2016), and its impact on the external validity of this study is uncertain. We note, however, that nonrandom location does not necessarily pose a threat to internal validity: our research design links changes in biodiversity outcomes to year-over-year variation in economic production within the same sampling site, and therefore our conclusions are not biased by factors that are correlated with permanent differences in economic production in the cross section (e.g., heavily forested areas tend to have lower GDP but better biodiversity outcomes compared to urban areas).

Time Ecological studies cover different time periods, and we only observe biodiversity measurements in years when the underlying study reports sampling results. Non-continuous sampling may affect the internal validity of our estimates if there is endogenous sampling. One potential concern is that economic conditions may affect whether a study starts, stops, or is paused at a particular location. This may happen if, for example, strong economic growth causes better scientific funding availability, or if studies are interrupted during recessions. The impact of this type of selection on our estimates is ambiguous and depends on the non-linearity of the impact of GDP.[7] Alternatively, one might worry that a large economic

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[6]Unobservable differences in survey effort is a canonical challenge for reliability of biodiversity datasets. For example, see Newbold (2010) and Ruete (2015) for discussions on survey effort biases in museum and citizen science data.

[7]In principle, oversampling periods of high GDP, or undersampling periods of low GDP, would only bias the average estimate when the "dosage" effect of GDP is nonlinear, i.e., the marginal effects of GDP differ at high vs. low levels. Figure 3, Panel A shows some evidence that the effect of GDP is roughly linear for all three biodiversity outcomes.

boost in an area may distress the local environment so much so that the scientists abandon the sampling location; in this case, the sampling selection would cause us to understate the negative impact of GDP as the worst consequences are not observed. Endogenous sampling can be tested empirically in the same way one tests for nonrandom missingness and attrition, and we discuss relevant exercises in Section 3.2.

Species Focus The data provide information only on species that are the subjects of the underlying studies. For example, in the North American Breeding Bird Survey study, observers record birds seen or heard, but do not record any information on other animals or plants seen; in the Sevilleta Long-Term Ecological Research, the subjects of the study are small mammals that are captured in Sherman box traps. An advantage of this feature of the data is that it makes it straightforward to compute biodiversity indexes such as abundance, richness, and similarities at the sampling site level for a specific taxon, as most ecological studies examine closely related species that fall in the same taxon group.[8] As long as a study's overall species focus remains fixed across years, our analysis will give rise to internally valid estimates of how economic production affects biodiversity for the set of species being studied.

There are two caveats when extrapolating our results to the population scope. First, weights of sampled species in the data may not be proportional to their overall presence in the nature. Unfortunately, in the context of biodiversity, there is a lack of population-based surveys of species representativeness to allow for a census-based re-weighting exercise that is often feasible in economic research. Second, the data cannot capture impacts on species not covered by the underlying ecological studies. A deeper concern regards species spillovers, for example, if declines in observed predator species open up niches in the ecosystem for new prey species to flourish (positive spillover), or if the decline of keystone species adversely affects other species (negative spillover). These changes will be captured to some degree through the Jaccard similarity index that measures species turnover. We also note that these will be less of a concern for studies on birds and some other freshwater plants and invertebrates that tend to employ broad sampling strategies that cover large numbers if not all species in the respective taxon.

Sampling Technology By construction, BioTIME only includes studies that adopted fixed sampling protocols, and therefore the sampling accuracy at a given sampling site is

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[8]In fact, as we will further explain in Section 4.1, the unit of analysis of our study is a study location × taxa. That is, for the small number of studies that cover multiple taxa, we treat the same study location as separate, taxa-specific observations, so that we would never measure biodiversity using distant species (e.g., adding together birds and fish).

expected to remain fixed. However, sampling bias may come from measurement errors in the biodiversity variables, especially if the errors change over time in ways that are related to trends in economic output. This may happen, for example, if sampling practices and technologies changed over time due to technological improvement (e.g. better traps). This would raise concerns about our findings if, conditional on our included fixed effects, the spatial distribution of the adoption of improved practices and technologies happened to correlate with the spatial distribution of trends in economic output. In Section 3.2, we describe tests that checks stability of our estimates over time and across studies with different time span. More broadly, we also look for the presence of extreme samples and test sensitivity of our results to these outliers.

3.2   Empirical Tests

Endogenous Sampling   Empirically, we do not find compelling evidence indicating that sampling activities respond to year-over-year changes in economic conditions. Most of studies are uninterrupted during the study span (or strictly follow a fixed sampling cycle); in Appendix Table A.1, we report a series of tests indicating that economic conditions are not correlated with the length of the study span, when studies began, when they ended, or when they missed sampling.

Outliers and Large Deviations   Biological specimen in certain studies may sometimes contain large counts, although in Figure 2 we have shown that most year-over-year changes in the biodiversity metrics fall within reasonable ranges. In Appendix Table A.2, we show that winsorizing samples with extreme *levels* of biodiversity outcomes or excluding samples that exhibit abnormally large year-over-year *changes* in biodiversity outcomes both have little impact on our estimates.

Measurement Quality   Figure 1 shows that our data spans almost six decades. Ecological sampling practices and technologies have likely changed over such a long time horizon even if protocols have been held fixed. Without direct information on the quality of biodiversity measurements, there is not much we can do to assess the implications of measurement errors that may exist in older studies. In Appendix Table A.3, we report a set of exercises where we split our samples into older versus newer studies, or studies with shorter versus longer duration, to roughly capture any differences in measurement quality over time. We find no strong evidence of heterogeneous effects along these dimensions.

Alternative Biodiversity Measurements In our main analysis, we measure abundance by summing up numbers of individuals observed in each taxa-location-year cell, and we measure richness by counting the number of distinct species observed, regardless of the distribution of individual counts across different species. For example, consider the North American Breeding Bird Survey that we described in Section 2.1. Suppose a bird observer sees 50 American Robins and 6 Northern cardinals on a survey route, then the abundance metric of that sample is 56 and the richness metric is 2. These ways of measuring biodiversity have shortcomings. For example, in computing species richness, it might seem natural to up-weight cases where abundance is more evenly distributed across the species versus cases where we observe the same number of species, but the abundance across species is much more concentrated. The former may be a better representation of the true diversity of species.

We consider two alternative measures of species richness – the Gini index and Shannon index – that take into account *relative abundance* information. Recall from Section 2.2 that $\mathbb{S}_{ct}$ is the set of species at location c and time t. Let n_{sct} be the abundance of species $s \in \mathbb{S}_{ct}$. We define relative abundance to be $r_{sct} = \frac{n_{sct}}{\sum_{j \in \mathbb{S}_{ct}} n_{jct}}$, the share of a particular species out of the total number of individuals. The Gini diversity index is given by

$$\text{Gini Index}_{ct} = \frac{1}{\sum_{s \in \mathbb{S}_{ct}} r_{sct}^2},$$

and the Shannon diversity index is given by

$$\text{Shannon Index}_{ct} = \exp\left(- \sum_{s \in \mathbb{S}_{ct}} r_{sct} \times \log r_{sct}\right).$$

Notice that the Gini diversity index is effectively an inverse Herfindahl–Hirschman index, whereas the Shannon diversity index is analogous to entropy. Unlike species richness, which considers only the total number of unique species presence, both Gini and Shannon diversity measures adjust for the relative abundance of species. If relative abundance is identical across all species in $\mathbb{S}_{ct}$, then species richness, Gini diversity, and Shannon diversity all yield the same value. For example, if there is an equal share of 2 species in $\mathbb{S}_{ct}$, then all three indices will be 2. If there is heterogeneity in relative abundance across locations, then the Gini diversity and Shannon diversity measures give a lower value than species richness. In Appendix Table A.4, we show that our findings are similar using these alternative biodiversity indexes.

Geographic Unit of Analysis   We use state level GDP to capture local economic output. State is a focal level at which many economic outcomes and policy variables are measured. States are large geographic areas, and this bears both pros and cons for our analysis. On the positive side, using aggregate measurements of GDP ensures that our estimates do not reflect small-scale spatial displacement, for example, when individual animals move to a nearby location in response to a ramp-up of local industrial activities. However, using state level GDP foregoes more localized economic variation that could matter for biodiversity outcomes. Impacts of economic production may also exhibit spatial heterogeneity: for example, it is possible that the ecological damage of production differs across different ecosystems within the same state. In Appendix Table A.5, we report robustness checks where we aggregate economic and/or biodiversity measures to alternative geographic levels, such as counties, locally-similar ecosystems ("eco-regions"), or simply geographic bins of various sizes.

4   Economic Production and Biodiversity

4.1   Estimation Framework

We begin with a panel fixed effects regression model to estimate the within-location association between biodiversity outcomes and economic output. The workhorse regression equation is

$$Y_{cjt} = \beta \cdot \log GDP_{st} + \eta_{cj} + \eta_t + \varepsilon_{cjt} \tag{1}$$

Y_{cjt} is one of the three biodiversity metrics at sampling location c for taxon j in year t. For example, this may be the logged total number of birds observed at a particular sampling location in a given year. The regressor of interest is $\log GDP_{st}$, which is the logged per capita real GDP for the state-year. η_{cj} are location-by-taxa fixed effects; these are the key panel fixed effects that ensure the identification is based on year-over-year changes in biodiversity and economic development for the same sampling location and within species in the same taxa (i.e., we are not comparing trees to birds or ants to mice). η_t denotes year fixed effects to capture common shocks such as national recessions. ε_{cjt} is the error term. We cluster standard errors at the state level. The key parameter of interest is β, which measures the elasticity between GDP and our biodiversity metric of interest.

It is worth clarifying the unit of observation used in our panel estimation. The key geographic unit is a sampling location c. A given ecological study may have multiple sampling locations. The North American Breeding Bird Survey (Section 2.1, example study 1), for example, covers over 4,100 bird-observing "routes" that the researchers revisit every year.

In BioTIME, each route is a sampling location identified uniquely by the route's centroid latitude and longitude. Some studies sample across multiple taxa at the same location, such as the Sevilleta Long-Term Ecological Research Program (Section 2.1, example study 2), thus the cj subscripts. As we mentioned, a key advantage of BioTIME data is that the study inclusion criteria ensure there are consistent study protocols used within each study conducted at the same sampling location. This means the year-over-year variation in biodiversity outcomes within the same location-taxon reflects genuine changes in underlying biodiversity measures, rather than changes in sampling methods. This feature of the data is important for our empirical estimation because, conditional on location fixed effects, it frees us from controlling for any changes in sampling practices that might be influenced by economic conditions across time. Finally, the length of a time series of location-taxon data in our dataset depends on changes in sampling protocols and the overall study duration. If the sampling methodology changed during the course of the study, a new ID is assigned so it is treated as a new observational unit. Our final estimation data are thus an unbalanced, annual panel of sampling locations by taxon.

4.2   Main Results

Figure 3, Panel A reports the estimation results from equation (1). The three columns correspond to species abundance, species richness, and Jaccard similarity. We residualize the biodiversity metrics and log GDP with the fixed effects, and then plot one against the other using a decile bin scatterplot. The slope of the fitted line thus represents the OLS estimate $\hat{\beta}$ of equation (1). Our results indicate negative, statistically significant, and roughly linear effects of GDP on both abundance (estimated elasticity = -3.580, SE = 1.353) and richness (estimated elasticity = -1.631, SE = 0.685). The effect of GDP on similarity is negative but imprecisely estimated (estimated elasticity = -0.104, SE = 0.157). A reduction in similarity implies that there are fewer species of the same type in a sampling location compared to the previous year, due to either a loss of species or newly introduced species.

Panel B of Figure 3 shows elasticity estimates separately by taxon, where from left to right we report effects for amphibians, birds, fish, freshwater invertebrates, freshwater plants, mammals, terrestrial invertebrates, and terrestrial plants. We find that the negative associations with GDP emerge not only for birds (which comprise of 80 percent of our estimation sample), but for most of the taxon groups. Among amphibians and mammals, significantly negative effects of GDP are detected for all three biodiversity metrics. In following analyses, we report both full-sample results, and a sub-sample version that excludes birds.

Panel C of Figure 3 shows elasticity estimates by quintiles of the sampling area's average

(1966-2015) GDP per capita. We find that the GDP-biodiversity elasticities do not vary substantially across *overall levels* of GDP. Thus, there is thus limited evidence within our study scope that the GDP-biodiversity elasticity follows an environmental Kuznets relationship (Grossman and Krueger, 1995; Andreoni and Levinson, 2001; Harbaugh et al., 2002); rather, the negative effects of GDP persist across different levels of economic development. Of course, a caveat here is that the range of average GDP per capita levels we examine is high because we focus on the United States.

Panel D of Figure 3 investigates distributional heterogeneity, showing results from quantile regressions. We consider a fixed-effects-residualized version of equation (1), estimating regression quantiles using a residualized biodiversity metric as the outcome, and the residualized log GDP per capita as the explanatory variable. For each outcome, the horizontal line represents the average effect (i.e., the slope of the fitted line in Panel A of Figure 3). For all three biodiversity outcomes, we find evidence that effects are negative or zero at all quantiles, and we find heterogeneity in the size of the effect; the negative effects of GDP are the largest at the lowest quantiles of the distributions of the biodiversity metrics. This pattern suggests that economic development exacerbates biodiversity losses in areas where biodiversity is already low. The heterogeneous effects also suggest higher levels of biodiversity may increase resilience to adverse shocks to the ecosystem.

It is inherently difficult for us to compare our effect size with the prior literature due to the lack of similar studies. Here we provide one benchmark exercise where we calculate the change in bird abundance implied by our estimates, and compare that with established estimates of bird population decline. Recent work by Rosenberg et al. (2019) using the North American Breeding Bird Survey – one ecological study included in BioTIME which we described in Section 2.2 – estimated that bird population numbered around 8 billion in 1990, but had declined by about 1 billion by the end of 2015. To evaluate what fraction of such decline might be attributable to economic production growth during the time period, we estimate a modified version of equation (1) focusing on bird-related observations and with GDP per capita as the regressor. This estimation yields an estimated coefficient of -0.167 log units (representing a decline in the bird population) per $10,000 increase of GDP per capita. Multiplying this coefficient by the average growth of real GDP per capita between 1990-2015 suggests a decline of 1.04 billion birds, which is very close to the estimate in Rosenberg et al. (2019).

In Table 2, we report specifications where we replace the aggregate GDP measure in equation (1) with sector-specific income. We focus on six sectors. Two are well known to cause significant amounts of pollution: manufacturing (air pollution) and mining (water

pollution); two that physically alter the landscape: agriculture and logging; and two that are related to economic activity within urban areas: construction and service. Our results show large and consistent negative associations between manufacturing income – indicative of greater manufacturing production – and biodiversity. We also find a negative correlation between logging-related income and biodiversity metrics for non-bird species, and a positive association for agriculture.[9] This evidence suggests that industrial emission as a byproduct of economic production may be an important driver of biodiversity trends. We take a more detailed look at the role of pollution in Section 6.

4.3 Dynamic Effects

Our baseline analysis focuses on estimating the contemporaneous impact of GDP on biodiversity. Here we consider several alternative specifications that explore potential dynamic effects. We begin with distributed lag specifications, augmenting equation (1) with various lags of annual GDP. Appendix Figure A.3 reports dynamic specifications that include up to five lags of GDP. We find that the static and dynamic specifications produce similar estimates of the contemporaneous impact of GDP, whereas we do not find strong evidence of lagged impacts. In Appendix Table A.6, we further consider a specification where we include a *lead* term of GDP in addition to current year's GDP. We find the "placebo", lead coefficient to be statistically insignificant and in general an order of magnitude smaller than the main GDP effect size; once again, evidence on a lagged effect of GDP is inconclusive.

Another way to approach dynamics and causality simultaneously is with a vector autoregression (VAR) that treats both biodiversity and GDP as endogenous variables that are interdependent on each other's lags. We implement a VAR analysis adapted to our panel data setting using the method developed in Love and Zicchino (2006). We estimate bi-variate, first-order panel VARs with each of the three biodiversity outcomes and GDP as endogenous variables, and with panel- and time-specific fixed effects.[10]

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[9]We are not able to exactly pin down the source of this positive association. One potential explanation is increased agricultural output is correlated with more abundant food sources for terrestrial animals; favorable weather conditions might be correlated with both better agricultural output and terrestrial plant health.

[10]Specifically, the estimation equation is as follows:

$$\mathbf{Y}_{cjt} = \mathbf{Y}_{cjt-1}\mathbf{A} + \mathbf{u}_{cj} + \mathbf{u}_t + \mathbf{e}_{cjt}$$

Notice the correspondence with our simple panel estimation equation (1): instead of modeling the biodiversity outcome as a function of contemporaneous GDP, the panel VAR formulation assumes the biodiversity outcome is a function of lagged GDP, *and vice versa*. The vector notation therefore represents a stacked system of equations where $\mathbf{Y}_{cjt}$ denotes a (1×2) vector of dependent variables (e.g., log abundance and log GDP per capita), $\mathbf{u}_{cj}$ and $\mathbf{u}_t$ are (1×2) dependent-variable-specific location-by-taxa and year fixed effects, and $\mathbf{A}$ is a (2×2) matrix of parameters to be estimated which is assumed to be homogeneous across all

Appendix Figure A.4 summarizes the estimation results. The impulse response functions (IRFs) suggest that GDP has a significant, negative impacts on biodiversity outcomes; similar to our reduced-form findings, the IRFs suggest that the effects concentrate on the first period, while quickly converge to zero in about four periods. The IRFs suggest that the reverse causation, i.e., the impacts of biodiversity outcomes on GDP, are positive, transient, but insignificant, with effect sizes an order of magnitude smaller than the estimated effects of GDP on biodiversity.

The dynamic models described above focus on the lagged effects of GDP shocks in the near past. In Appendix Table A.7, we further explore the role of longer-run economic trajectories, estimating alternative specifications where we replace the logged GDP variable with various forms of GDP *growth*, including current growth rate, five-year moving average rate, and/or five-year moving maximum rate. Even conditional on the effect of current growth, we find large, negative effects of sustained trend of growth in the past years on biodiversity (columns 2, 5, and 8) , which is partially explained by years with a burst of growth (columns 3, 6, 9). While we view these specifications as less well-identified than the simple OLS regressions of equation (1), the evidence appears to suggest that biodiversity is influenced by the trajectory of economic growth in addition to year-over-year shocks.

4.4 Robustness

Here we describe additional robustness checks that correspond to concerns we laid out in Section 3. Appendix Table A.1 presents various "zero-stage" regressions where we test if economic conditions influence sampling in any significant ways. First, in the cross section of study locations, we test if the average GDP growth rate at the location can predict the study span. Columns 1 shows the correlation is small: the point estimate suggests that each percentage point increase in the average GDP growth (or about a 50 *percent* increase relative to the mean rate) at the study location is associated with a 1.3 percent increase in the study duration; the coefficient estimate is statistically insignificant. Second, using the panel fixed effects regression framework outlined in equation (1), we test if year-over-year changes in GDP (as well as lagged GDP) can predict when a study starts sampling (columns 2 and 3), ends sampling (columns 4 and 5), and misses samplings (columns 6 and 7). We find no statistical evidence across the board that economic conditions can predict sampling activities.

Appendix Table A.2 examines the impact of outlier observations. We find that win-

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cross-sectional units. We implement the computation using the GMM algorithm provided by Abrigo and Love (2016).

sorizing extreme samples, defined as those with the smallest and largest 1%, 5%, or 10% biodiversity outcomes, has little impacts on our estimates. We also obtain similar estimates excluding samples that exhibit abnormally large deviations in biodiversity outcomes, defined as those with over plus or minus 2, 3, or 4 standard deviations (SD) from the average year-over-year changes.

Appendix Table A.3 touches on measurement quality differences by splitting our estimation data into studies that were done in earlier vs. later periods. We first repeat our main estimation of the biodiversity-GDP relationship separately for *samples* taken before versus after 1997, the median year of sampling; in another test, we group *studies* by the first year of sampling, and then estimate the biodiversity-GDP relationship separately for those started before versus after 1993, the median of studies' start years. We find that our main findings are statistically indistinguishable among these subsamples. We further stratify studies by their duration into those that lasted less than 5 years, between 5 and 20 years, and over 20 years (roughly corresponding to studies that fall below 25th, between 25th and 75th, and over 75th percentile of the study duration distribution). We find some suggestive evidence that the biodiversity-GDP link is more precisely estimated among longer-term studies – presumably those that are on average larger in scale, more fully funded, and potentially adopting more sophisticated measurements – although the gradient with respect to duration is not statistically significant. Taken together, while these tests cannot directly quantify the influence of sampling quality on our estimates, they do suggest that differences in measurement quality are not a first-order determinant of what we find in this study.

Appendix Table A.4 shows that our species richness findings are similar using Gini Index and Shannon Index, two alternative diversity metrics that incorporates relative abundance information. We also report results using the Sorensen index, which is a commonly used measure of species similarity, as an alternative to the Jaccard similarity index.[11]

Appendix Table A.5 reports results when we change the geographic unit of analysis in various ways. First, we use a county level measure of economic activities. County is among the most granular geographic units at which historical economic data at an annual frequency is publicly available. For this exercise, we use data on county total income reported by the Bureau of Economic Analysis (BEA). Second, we use the concept of *eco-region* developed by the U.S. Environmental Protection Agency, which divides the contiguous U.S. into 85

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[11]The Sorensen similarity index is defined by

$$\text{Sorensen Index}_{ct} = \frac{2 \times n(\mathbb{S}_{ct+1} \cap \mathbb{S}_{ct})}{n(\mathbb{S}_{ct+1} \cup \mathbb{S}_{ct}) + n(\mathbb{S}_{ct+1} \cap \mathbb{S}_{ct})}.$$

where $\mathbb{S}_{ct}$ denotes the set of species at some time t in taxon j and location c, and $n(\cdot)$ denotes the cardinality of a set.

mutually exclusive areas where ecosystems are similar. Appendix Figure A.5 provides a map of EPA level-III eco-regions as of 2013. We define economic conditions by summing up income from counties that fall within the boundary of each eco-region, which is used as the independent variable in lieu of state GDP. Third, we report a series of robustness checks where we adopt a spatial-binning approach that aggregates biodiversity outcomes into 3-km, 10-km, and 16-km hexagon bins. Figure 1 shows that, depending on the geographic scope of the study, many sampling locations may be close to each other. Because distinct sampling locations in the BioTIME data represent different study protocols, in our primary analysis we treat them as independent cross-sectional units even if they are very close to each other. A potential concern is that we may overstate the effective amount of data due to spatial correlation. Hence, the spatial binning approach aggregates out some spatial correlation, though it introduces measurement error as samples collected using different protocols are simply "added up." We find that the estimation results using spatially binned data are attenuated with coarser aggregation, though the qualitative conclusions hold up.

5   Government Policies

Section 4 reveals a negative association between biodiversity and economic production, but the findings may be subject to standard endogeneity concerns. One might worry that unobserved factors, such as local extreme weather, may correlate with both GDP and biodiversity (omitted variable bias); that errors in economic measures and/or biodiversity outcomes may attenuate the elasticity estimates (measurement error); and that biodiversity may itself cause changes in economic output (reverse causality). In this section, we analyze how biodiversity outcomes respond to government policies that are known to generate plausibly exogenous shocks to the local economy: U.S. national military buildups (Section 5.1) and Clean Air Act regulations (Section 5.2). For each policy, we first estimate the direct impact of the policy shocks on the biodiversity outcomes. We then compute biodiversity-GDP elasticities implied by these quasi-experiments, and compare them to the baseline OLS counterparts. We discuss issues related to the causal interpretation in Section 5.3.

5.1   Military Buildups

We first exploit shocks to state GDP from changes in national military buildups. This strategy has been widely used in the empirical macroeconomics literature to estimate the fiscal multiplier, i.e., the causal effect of government spending on economic output (e.g., Hall, 2009; Barro and Redlick, 2011; Ramey, 2011; Nakamura and Steinsson, 2014, 2018). Several

features of military buildups in the United States make it an attractive policy experiment in our study setting. First, national military buildups are well-known to be driven by geopolitical events such as the Vietnam War and the Soviet invasion of Afghanistan. The timing and magnitude of these events are plausibly exogenous to biodiversity changes in the United States. Second, because industries that produce military equipment are unevenly distributed across the country, national changes in military buildups leads to greater military spending in some states than in others. Figure 4 Panel A, adapted from Nakamura and Steinsson (2014), plots each state's military contract spending as a share of its GDP. We assign darker lines to states with a higher share of state military spending during a base period (1966-1971 average). Note that there is substantial variation and persistent differences in the degree to which national shocks (the blue line in the middle of the chart) translate to state-specific shocks. We use this variation to tease out regional changes in economic output that are attributable to national military spending shocks. Finally, military spending, such as repair and maintenance of military facilities, strongly influences construction and manufacturing output; these sectors are associated with substantial environmental externalities, and may influence biodiversity outcomes.

We use the following equation to estimate the effect of military spending shocks on biodiversity outcomes:

$$Y_{cjt} = \beta \cdot \left(\frac{MS}{GDP} \right)_{state,1966-1971} \times MS_t + \eta_{cj} + \eta_t + \varepsilon_{cjt}, \qquad (2)$$

This equation mirrors the main estimation equation (1). On the right hand side, we define the military spending shock as $\left(\frac{MS}{GDP} \right)_{state,1966-1971} \times MS_t$, which is the initial (1966-1971 average) military contract spending (MS) a state receives as a share of its GDP, interacted with annual, national per capita military spending (MS_t). As shown in Figure 4 Panel A, although the share of military spending to GDP changed over the years, the initial share over the 1966-1971 period still captures systematically different sensitivities of GDP to military spending shocks across states. The military spending shock variable thus captures state heterogeneity in sensitivity to the national military buildups. The fixed effects variables η_{cj} and η_t are defined as in equation (1).

For this policy shock to be exogenous, we assume that the shock component – states' relative differences in response to aggregate military buildups (which are themselves largely driven by geopolitical factors) – are unlikely to be correlated with unobservable determinants of local biodiversity. That is, we assume the United States will not increase national military spending *because* states that receive larger military procurement contracts have worse

biodiversity.

Figure 4 Panel B presents the estimation results for the key coefficient of interest β, the impact of the policy shocks on biodiversity outcomes. We use the same decile bin scatterplots as Figure 3, except here the x-axis variable is fixed effects-residualized military spending shocks. We display full-sample results for all species as well as subsample results with non-bird species. For all three biodiversity outcomes, we find that military spending shocks have a statistically significant negative effect on biodiversity. The chart also shows that the negative effects of military spending shocks are larger for non-bird species for all three biodiversity outcomes.

Table 3 summarizes these findings in more details. Column 1 repeats Nakamura and Steinsson (2014) in our study sample and confirms the positive impact of the military spending shocks on state GDP. Columns 2 through 4 report changes in log abundance, log richness, and similarity index from a one unit increase in the military spending shock variable.

To facilitate comparison of effect sizes with the OLS estimates in Section 4, in columns 5 through 7, we divide the estimates in columns 2 through 4 by that in column 1, converting these estimates to the biodiversity-GDP scale using two-stage least squares (2SLS).[12] In the full sample (Panel A), we find that the implied GDP elasticities are -4.49 for the abundance measure (SE=1.59), -2.75 for richness (SE=1.23), and -0.54 for similarity (SE=0.18). By way of comparison, our OLS estimates of the GDP elasticities are -3.58 for abundance (SE=1.35), -1.63 for richness (SE=0.69), and -0.10 for similarity (SE=0.16). Similarly, in the non-bird subsample (Panel B), we find slightly larger biodiversity-GDP elasticity estimates than their OLS counterparts.

Appendix Table A.8, Panel A reports a series of robustness checks where we (a) alter the construction of the military spending shock variable by changing the baseline period of the treatment variable construction from an initial-period average (1966-1971) to a long-term average (1966-2006); (b) follow the original empirical specification in Nakamura and Steinsson (2014) and construct military spending shocks as the fitted value of state spending on national spending, allowing different sensitivity for each state; (c) use the limited information maximum likelihood (LIML) estimator which is median-unbiased with weak instruments in lieu of 2SLS; (d) add Census Division-specific decadal time trends to control for regional-specific trends in addition to the fixed effects controls; and (e) replace state GDP

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[12]The first stage estimation has a Kleibergen-Paap F-statistics of about 7 in the full sample, and about 35 in the non-bird subsample. This difference is due to the fact that the non-bird sampling locations are in states with higher military spending, and are more responsive to changes in national military buildups. In addition, as shown in Figure 1, non-bird observations are more evenly distributed across the entire study period, allowing the estimation to better exploit temporal variation in economic changes.

with county-level income as the right hand side measure of economic production. For succinctness, we only report biodiversity-GDP elasticity estimates for these robustness checks. The results are not sensitive to these specification changes.

5.2   Environmental Regulation

Next, we analyze policy shocks coming from air pollution regulations under the Clean Air Act (CAA). The CAA is one of the first and most influential pieces of environmental legislation in the United States, regulating air quality at the state, local, and plant levels through a large number of individual programs. Our research design focuses on a major amendment of the CAA that was adopted in 1990, when the National Ambient Air Quality Standards established national criteria for outdoor air quality, targeting six widespread air pollutants that harm public health.

A key component of the CAA program is its annual designation of compliance and non-compliance status. Jurisdictions (mostly counties) in compliance with the air quality standards are designated "attainment" areas, whereas those in violation with the standards are designated "nonattainment" areas. A nonattainment determination can be made for failing to achieve one or more of the pollutant standards – $PM_{2.5}$, PM_{10}, ozone (O_3), sulfur dioxide (SO_2), carbon monoxide (CO), and lead (Pb) – and/or for failing to meet one or more of the versions of the standards for a given pollutant.[13] A nonattainment designation triggers substantially elevated regulatory scrutiny; the state government is required to implement stringent regulations on the polluting industries. Such measures may include the installment of expensive pollution abatement technologies, and the use of emission permitting programs until air quality in the nonattainment area meets the standards. Figure 5 Panel A shows the fraction of sampling locations in the BioTIME data that were in nonattainment areas with respect to each of the standards.[14] Figure 5 Panel B plots the location of nonattainment counties and the number of standards being violated in each county as of year 2019.

We base our research design on two rich lines of environmental economics literature that document the economic costs and – parallel to our later air pollution results – the environmental benefits of the nonattainment regulations. A nonattainment designation has

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[13]Versions of standards reflect different target metrics or changes in regulatory stringency over time. For example, the 1997 $PM_{2.5}$ standard specifies an annual safety level of 15 ug/m^3, whereas the 2006 $PM_{2.5}$ standard specifies a 24-hour safety level of 35 ug/m^3; the 2008 O_3 standard (a daily maximum 8-hour concentration of 0.075 ppm) is a more stringent version of the 1997 O_3 standard (0.08 ppm).

[14]There are usually years of lag between the times when a pollutant standard was promulgated and when the nonattainment designation actually occurred. For example, as shown in Figure 5 Panel B, the designation of nonattainment status with respect to the 1997 $PM_{2.5}$ standard did not occur until the year 2005.

been shown to reduce productivity and output, and impose considerable compliance and fiscal costs on the local economy (e.g., Greenstone, 2002; Greenstone et al., 2012; Walker, 2013; Blundell et al., 2020; Shapiro and Walker, 2020; Hollingsworth et al., 2022) while effectively reducing air pollution (e.g. Chay and Greenstone, 2005; Sanders et al., 2020; Hollingsworth et al., 2022).

Here we repeat the same analysis as we did for military spending, but instead with the number of nonattainment designations as our treatment variable. Figure 5 Panel C plots the results once again in the decile bin scatterplot form. All estimates indicate that greater numbers of nonattainment designations, proxying for increased levels of regulatory stringency and reduced local economic production, increase biodiversity.

Table 4 provides additional details analogous to Table 3 for military spending. Column 1 shows that more stringent environmental regulations decrease GDP. We find that one additional nonattainment designation reduces local GDP by about 3.8 percent in the full sample and 5.3 percent in the non-bird subsample. The magnitude of this estimate is consistent with prior evidence: for example, Greenstone et al. (2012) analyzes production data from 1972-1993 Annual Survey of Manufacturers and finds that the NAAQS designations have reduced total factor productivity by 4.8 percent for manufacturing plants.

Columns 2 through 4 show the effects of the regulation shocks on biodiversity outcomes which correspond to the bin scatterplots from Figure 5 Panel C. As before, we find that the effects are larger for abundance and richness than and for similarity. Columns 5-7 report that the implied biodiversity-GDP elasticity estimates are -5.93 for abundance (SE=0.62), -3.19 for richness (SE=0.27), and -0.52 for similarity (SE=0.53). In Panel B, we repeat the same estimation using the non-bird subsample. Similar to our findings in Section 5.1, we find somewhat larger GDP elasticities for non-bird species.

Appendix Table A.8, Panel B reports a series of robustness checks where we (a) change the treatment variable from the *number* of nonattainment designations to an indicator variable for nonattainment of *any* pollutant standard; (b) use an overidentified model with 12 excluded instruments, each indicating nonattainment status for a separate pollution standard as shown in Figure 5 Panel A. The rest of the robustness checks mirror those in Panel A, where we (c) use the limited information maximum likelihood (LIML) estimator; and (d) add Census Division-specific decadal time trends; and (e) replace state GDP with county-level income.

5.3 A Note on Causal Interpretations

We highlight three takeaway messages from this section. First, biodiversity outcomes respond to policy shocks that are well-known to cause changes in local economic activities. We find that biodiversity responds negatively to military buildups that boost local output, and positively to regulation shocks that reduce output. Because these policies generate plausibly exogenous variation in economic production, the findings improve upon the correlational evidence in Section 4.

Second, the implied biodiversity-GDP elasticities from the policy quasi-experiments are larger than their OLS counterparts. By leveraging shocks in a 2SLS setting, the quasi-experiments-based elasticity estimates alleviate classic measurement error and endogeneity problems. However, the fact that all three methods – the panel OLS and the two different policy experiments – produce elasticities of similar order of magnitude adds confidence to the overall credibility of the estimates.

Third, although we believe the underlying policy shocks have causal impacts on both biodiversity and economic production, we have refrained from interpreting the biodiversity-GDP elasticities as the causal effect of a *ceteris paribus* increase in GDP, which is not well-defined because GDP is an accounting concept and only varies because of changes in real economic activities. Put differently, one cannot randomly assign GDP while holding everything else constant. Instead, our estimates encompass the total effect of various determinants of biodiversity – such as environmental pollution, habitat loss, and so forth – that vary with economic production. Our analysis so far remains agnostic about what are the underlying mechanisms that explain the observed biodiversity-GDP relationship. We tackle that next in Section 6. In particular, we focus on the causal effect from air pollution, which we argue can be isolated out from other contributors to biodiversity changes.

6 The Pollution Channel

Our analysis so far focuses on the overall impact of economic production on biodiversity. It is important to further understand potential channels underlying these impacts. For example, does production damage biodiversity through the generation of environmental pollutants or through habitat destruction, and can these sources of damage can be muted through regulations? In Section 6.1, we first present new evidence that air pollution – an important byproduct of economic production that has well-understood impacts on human health – is also a significant driver of biodiversity losses. In Section 6.2, we use the estimated relationship

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between biodiversity and pollution to estimate the share of the total effect of the two policy shocks in Section 5 that is due to pollution externalities. We then briefly examine the role of other mechanisms, including urbanization and land use changes in Section 6.3.

6.1 Causal Effect of Air Pollution

We begin by estimating the relationship between air pollution and biodiversity. The most accurate measurement of air pollution comes from *in situ* pollution monitors, but these ground measurements are taken only at a sparse set of locations in the U.S., and the majority of those locations are at urbanized population centers. To improve data coverage, we instead use remote sensing-based reanalysis measurements of particulate matter pollution (Aerosol Optical Depth, or AOD) from the Modern-Era Retrospective analysis for Research and Applications Version 2 (MERRA-2) provided by NASA. The estimating equation once again mirrors our main equation (1), but with pollution as the variable of interest. The first row of Table 5 shows the results. Across all biodiversity measures and both samples of all species and non-bird species, we find strong and negative pollution-biodiversity associations, with larger elasticities for non-bird species.

The challenge with attributing changes in biodiversity to pollution is that pollution may be endogenous. There may be omitted factors driving variation in both pollution and biodiversity such as temperature and sunlight. There may also be reverse causality where biodiversity, say of plant species, impacts local air quality through emission of volatile organic compounds. To get at causal effects of air pollution, we follow the recent literature and use an instrumental variable (IV) strategy that teases out plausibly exogenous variation in a county's local air pollution attributable to transported pollution from upwind areas (e.g., Deryugina et al., 2019; Anderson, 2020). We then estimate 2SLS regressions of biodiversity outcomes on local air pollution, instrumenting for local air pollution with this "upwind pollution" variable.

To illustrate the procedure, we use Susquehanna, PA as an example of how we construct the IV. The same procedure is applied to all counties in our dataset. First, we begin with a daily panel dataset of air pollution in a set of counties whose pollution levels may affect air quality in Susquehanna. Let C denote the set of contributing counties and $|C|$ denote its cardinality (i.e., the total number of counties in this set). For each county c and day t, we calculate the angle ϕ_{ct} between county c's local wind direction and the vector pointing from city c to Susquehanna (e.g., $\phi_{ct} = 0$ means county c is exactly upwind from Susquehanna on day t). The IV for Susquehanna is a time-series variable constructed using the following

formula:

$$IV_t = \frac{1}{|C|} \sum_{c \in C} max\{0, cos(\phi_{ct})\} \cdot Pollution_{ct} \cdot \frac{1/distance_c}{1/\sum_{c \in C}(1/distance_c)}$$

where the $max\{0, cos(\phi_{ct})\} \cdot Pollution_{ct}$ term is the vector component of air pollution in city c on day t that is expected to move toward Susquehanna due to wind transport – we call this "upwind pollution".[15] We assume that upwind pollution is zero if ϕ_{ct} is greater than 90 degrees, i.e., wind in city c on day t blows *away* from the direction toward Susquehanna. The last term is an inverse distance weight. The formula says that, on any date t, the IV is the average of individual cities' upwind pollution terms, inversely weighted by city c's distance to Susquehanna ($distance_c$).

The choice of contributing counties C bears a bias-variance tradeoff. If we were to only use counties that are very far away from Susquehanna, it would help with the *exclusion restriction* assumption of the IV. That is, there is little reason for pollution variation in very faraway counties to affect local biodiversity outcomes in Susquehanna, except for the fact that transported pollution from these upwind counties contributes to changes in local air quality. However, focusing on counties too far away hurts the *first stage* relevance of the instrument because their impacts on local pollution is likely weak. We take the following steps to address this bias-variance tradeoff. First, we restrict contributing counties to those that are at least 300 km away from Susquehanna. Second, we employ a data-driven method that selects the most predictive upwind counties in a "zero-stage" LASSO regression. Specifically, before constructing the IV variable, we estimate the following equation and perform variable selection using LASSO:

$$Pollution_{Susquehanna,t} = \lambda_0 + \sum_{c \in \{1,...,2996\}} \lambda_c \cdot max\{0, cos(\phi_{ct})\} \cdot Pollution_{ct} + \epsilon_t$$

where the counties c that are selected by the LASSO procedure have a non-zero coefficient λ_c and are included in our set of contributing counties C for Susquehanna, PA. For Susquehanna, LASSO selects a subset of 54 upwind counties from a total of 2,996 counties that are at least 300 km away. We then conduct the IV construction outlined in the previous equation using these 54 selected counties. We then apply this procedure to all counties in our data.

Figure 6 Panel A is an illustration of Susquehanna where we map out the location of the selected counties and use a bubble graph to represent the λ_c coefficients from a post-LASSO

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[15]The cosine places more weight on pollution from upwind counties as their wind direction points more toward Susquehanna.

regression. Figure 6 Panel B is the union of selected upwind counties for all BioTIME counties in Pennsylvania. Many predictive counties are just outside of the 300km buffer as we might expect, but there are also ones from the Midwest or West, presumably due to wind transport. Figure 6 Panel C shows decile bin scatterplots of the first stage and reduced form regression results. There is a strong positive relationship between upwind pollution and local pollution for the full sample and the non-bird sample. Upwind pollution shocks are negatively associated with all three biodiversity metrics and the relationship is stronger for non-birds.

The second row of Table 5 shows our main IV results. All elasticities are negative, and the IV and OLS estimates are generally quite similar in size, suggesting that endogeneity in our setting is not severe.[16] Comparing these estimates to the associational GDP elasticities in Figure 3, the pollution elasticities are about half the size.

6.2 Overall vs. Pollution Effects of Economic Policies

With the causal biodiversity-pollution elasticity estimates from Table 5, we now revisit our quasi-experimental policy shocks and calculate how much of the policy's effect was due to induced changes in pollution. We do this in two steps. First, our reduced form estimates of the effect of the military spending and environmental regulation policies in Section 5 provide the total marginal effect of the policy on biodiversity: $\frac{\partial biodiversity}{\partial policy}$. Second, we compare the total effect to the partial effect of the policy through pollution: $\frac{\partial biodiversity}{\partial pollution} \cdot \frac{\partial pollution}{\partial policy}$. The first term in the expression is the biodiversity-pollution elasticity estimated in Table 5 of Section 6.1. The second term – the effect of the policy shocks on air pollution – is straightforward to estimate from the data, which we report in Appendix Figure A.6. As expected, military buildups increases pollution while nonattainment designations reduce pollution.

Figure 7 presents the decomposition. The top row shows the decomposition for our military buildups policy. The gray bars show the total effect of the policy, and the blue bars show the effect of the policy through changes in air pollution. For military spending, depending on whether we look at the full sample or non-birds, the effect of pollution accounts for about one-fifth to up to two-thirds of the estimated total effect of the policy. The bottom

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[16]This is in contrast to papers in the pollution-health literature that often find causal estimates to be an order of magnitude larger than OLS counterparts (e.g., Deschênes et al., 2017; Deryugina et al., 2019). One explanation is that there are much more *common determinants* for pollution and human health than for pollution and animal/plant species health. For example, employment conditions may directly affect both pollution and health dynamics, but employment is much less likely to directly affect biodiversity except through its impact on production and pollution. Therefore, pollution may be much less endogenous to changes in biodiversity outcomes than to changes in human health.

row shows the decomposition for the CAA regulations policy. The estimates here are quite similar, with pollution again accounting for between one-fifth and two-thirds of the policy's effect.

For both policies, air pollution is a first-order pathway for how they affect biodiversity but is not the only pathway. In one example, military buildups are a general boost to local economic production and may affect biodiversity through other channels such as land use change. In another, air pollution regulation may induce firms to emit through other media such as water pollution (Gibson, 2019), offsetting some of the positive biodiversity effects.

Before concluding, we note that there are two reasons for why we may be underestimating the role of air pollution. First, we are measuring air pollution using satellites in order to have full spatial coverage. If the satellite-derived estimates of particular matter suffer from classical measurement error, then our pollution estimates will be attenuated. Second, we are only estimating the impacts of particulate matter. Although particulate matter is often used as a proxy for overall air quality, it cannot be a perfect proxy for all air pollutants. This may explain why air pollution does not explain 100% of the CAA regulation policy effects. We conclude that pollution is first order mechanism underlying the biodiversity-GDP relationship, and air pollution regulations can provide policy maker another lever for conservation.

6.3   Other Mechanisms

Habitat losses are widely accepted as major determinants of biodiversity decline (IUCN, 2021), and increased economic development and urbanization may result in destruction of habitat. Here we briefly examine this relationship in our study context to briefly explore alternative mechanisms, and to perform a validity test for whether our setting produces results widely found in other parts of the literature.

Urbanization   We first explore increasing urbanization of land as a potential driver of biodiversity losses. This exercise leverages the fact that high-resolution satellite observations of urbanization became available during the second half of our study period (post-2001). We use NASA's Moderate Resolution Imaging Spectroradiometer (MODIS) land cover data (Section 2.3) which allows us to directly measure the degree of urbanization near the sampling sites in the BioTIME dataset, unlike GDP which is reported at a larger spatial scale. Our primary measure of urbanization is the log amount of land within 50 kilometers of a sampling site that falls under the MODIS urban classification. We also test the robustness of our results

using urbanization measured within 100 km of the site or within the county of the site.[17] We then estimate the effect of urbanization on biodiversity outcomes using the identical approach outlined in equation (1) where the variable of interest is the log land area that is classified as urban in MODIS.

Appendix Table A.9 shows our results. The first row shows the urbanization elasticity of biodiversity when focusing within a 50 kilometer radius, the second row is for a 100 kilometer radius, and the third row is within the entire county. All elasticities are negative and large for the measures of urbanization within 50 or 100 kilometers of the sampling site. The magnitudes of the estimates shrink slightly going from the 50 kilometer measure to the most aggregate 100 kilometer measure, consistent with urbanization near the sampling site being a more important determinant of biodiversity. The estimates at the county-level are smallest, potentially because of measurement error: sampling sites may be near county boundaries and not the county centroid, and counties do not necessarily have regular shapes. Overall, the evidence suggests that increased urbanization is negatively associated with our three measures of biodiversity, consistent with an extensive literature showing land use change and habitat destruction is a major driver of biodiversity trends.

Land Use Policies   One interpretation of our overall findings is that there is a trade-off between economic development and biodiversity, so that the goal of ecosystem preservation necessarily comes at the cost of economic slowdown. Alternatively, one could consider protective policies that mitigate the adverse effects of economic production. Here we leverage existing policies to speak to such a possibility. We focus on the standard, direct conservation approach of biodiversity protection, in which certain areas are protected by law from industrialization, providing ecological buffers from the influence of outside economic progress.

Since early 1990s, adoption of conservation protected area policies has grown rapidly (Frank and Schlenker, 2016).[18] Destruction of habitat is one of the primary drivers of species decline (IUCN, 2021), but the literature has generally found that protected areas — which cover nearly 15% of the Earth's land and 10% of its water — have had mixed results due to management issues, funding, resource exploitation, and ecological connections to areas outside of the protected area that may be degrading (Leverington et al., 2010; Laurance et al., 2012; Watson et al., 2014; Di Marco et al., 2019; Geldmann et al., 2019).

To study the effects of protected areas, we use the standard data source from the World

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[17]The average county has an area equivalent to a circle with a radius of about 30 kilometers.

[18]According to IUCN, protected areas include national parks, wilderness areas, community conserved areas, nature reserves and so on. The areas are managed through legal or other effective means by both the federal and local authorities.

Database on Protected Areas (WDPA). The WDPA is a geospatial database on over 250,000 marine and terrestrial protected areas. The database is in the form of a shapefile that outlines the location of each protected area and the year the protected area was implemented. In each location-year, we compute the total share of land and water within 50 km of the BioTIME sampling location that is within at least one currently implemented protected area. Figure A.7 plots the location of these protected areas. We also compute the number of spatially discontiguous protected areas within 50 km. After conditioning on the share of protected land or water, this later variable helps us tease out the effect of protected areas fragmentation holding the quantity of protected area fixed. We include this variable following the recent conservation literature which has suggested that habitat fragmentation has significant negative effects on biodiversity and the local ecology (Haddad et al., 2015; Crooks et al., 2017; Newmark et al., 2017), implying that more fragmented configurations of protected areas may be less effective than contiguous networks.

Table A.10 reports how protected areas modulate the effect of GDP on biodiversity outcomes. Columns 1, 3, and 5 use the same OLS specifications used in equation (1), but also interact log GDP with the share of land within 50 kilometers that is within a protected area. Columns 2, 4, and 6 include additional interaction terms between log GDP and the number of discontiguous protected areas within 50 km.[19] Columns 1 and 2 show that the abundance-GDP relationship is attenuated by an increase in the amount of nearby land that is protected. On average across all species, if the share of protected land and water within 50 km goes from zero (no conservation areas) to 100 percent, the marginal effect of GDP halves. We estimate larger effects on non-bird species, where halving the marginal effect of GDP only requires having 15 percent to 50 percent of area in protection. Consistent with prior evidence on fragmentation, column 2 shows that having a more fragmented set of protected areas has the opposite effect. Columns 3 and 4 show that protected areas have a noisy relationship with species richness, although the signs of the effects are generally the same as for abundance. The results are overall similar for species turnover (columns 5 and 6). Columns 5 and 6 show that protected areas tend to reduce the negative effect of GDP on species turnover, but we do not detect any effect of changing the number of areas. In general, there is suggestive evidence that larger coverage of contiguous protected areas reduces the negative impact of economic production on biodiversity. A caveat is that these findings are correlational in nature: protected areas are not adopted randomly but may in fact be targeted at areas with high levels of biodiversity to begin with, or areas that are seeing increasing developmental pressures.

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[19]Regressions also include the main effect terms for the share of protected area (columns 1, 3, 5) and, additionally, the number of discontinuous areas (columns 2, 4, 6).

7　Conclusion

This paper aims to understand the role of economic production in biodiversity outcomes. We estimate the production-biodiversity relationship by leveraging high resolution ecological data collected across the United States at thousands of locations and accounting for large shares of the known species within several different taxonomic groups. We find that economic production is negatively associated with various biodiversity metrics that capture the number of individuals (abundance), the diversity of species (richness), and the intertemporal stability of the composition of species (similarity). We show that this relationship is causal by exploiting two different quasi-experimental shocks to local production: military buildups and CAA nonattainment designations. Military buildups boost local production and worsen biodiversity, while CAA nonattainment designations depress local production and increase biodiversity. We then investigate the causal role of air pollution in affecting biodiversity following our findings that air quality regulations boost biodiversity and that the growth of polluting sectors like manufacturing leads to deterioration of biodiversity. Using an instrumental variables strategy that isolates exogenous local changes in pollution caused by pollution transported from elsewhere, we find that air pollution negatively affects all of our biodiversity measures and air pollution explains a large share of the policy effect of military buildups and CAA regulations. Our results indicate that air pollution regulations can serve as conservation policy and provide ecological co-benefits in addition to the benefits they provide to humans.

We conclude our paper with several caveats and suggestions for future work. First, our study only covers the United States, a wealthy country with significant environmental protections and conservation areas. Our findings may therefore not be applicable to other contexts and future research into biodiversity impacts elsewhere around the globe would significantly further our understanding of the area. Second, BioTIME database we use in this paper allowed us to focus on well-known, macro-scale economic and policy changes, but we note that the database includes detailed, geocoordinate information for all sampling locations and across multiple species, attributes that make it a suitable source to study the determinants of biodiversity in more granular, specific contexts that are outside the scope of this study.

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8 Figures and Tables

Figure 1: Sampling Locations and Observations
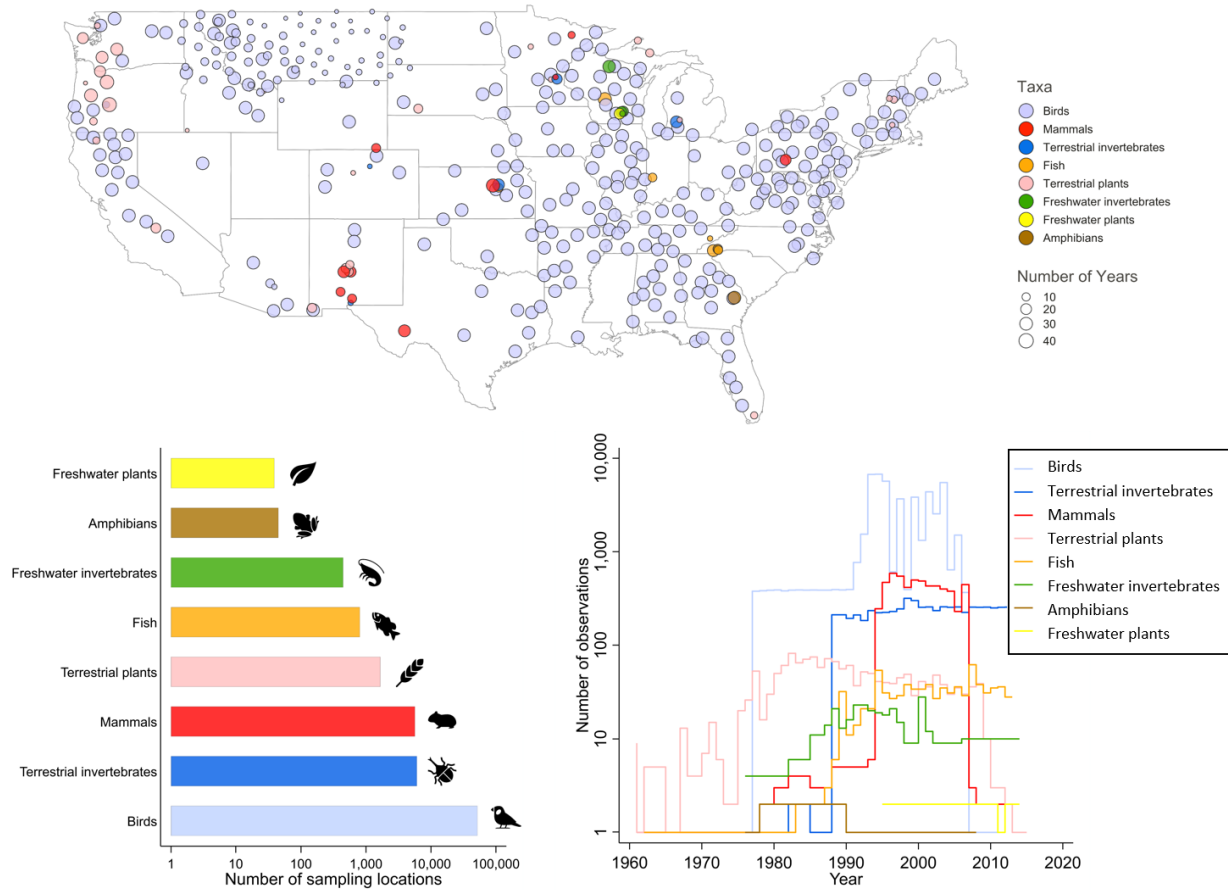
Notes: The upper panel plots sampling locations that are included in our main estimation sample. Location points are distinguished by taxa and are aggregated to a 95-by-95 km hexagon resolution to increase readability. The lower left panel reports number of sampling locations. The lower right panel reports total number of location-taxon observations in each year.

Figure 2: Year-Over-Year Variation in Species Abundance, Richness, and Similarity
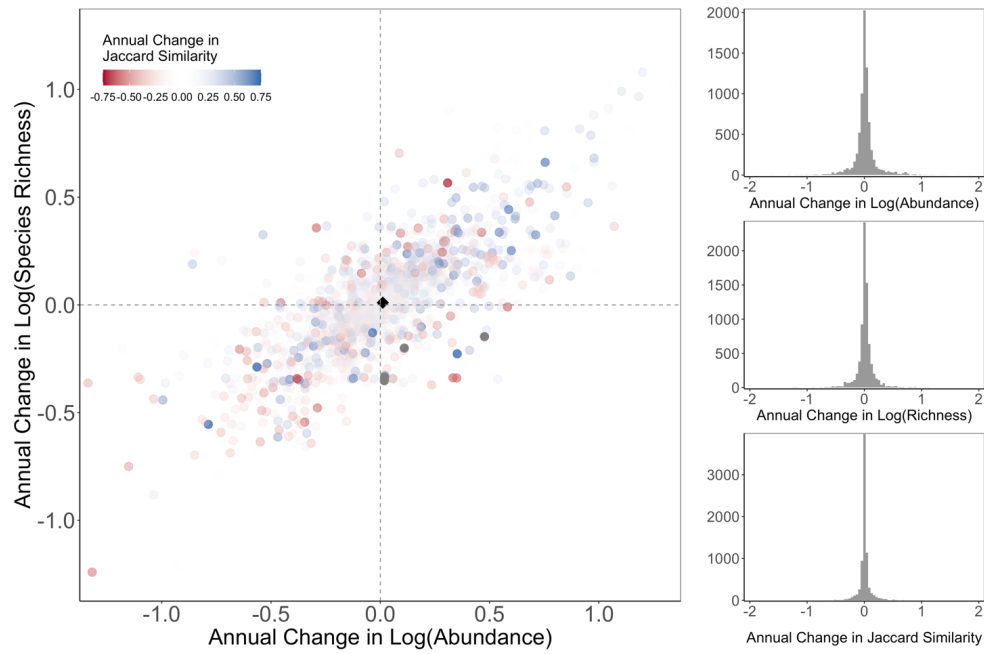
Notes: All panels plot the coefficient estimates of our biodiversity metrics on a linear year time trend. The black point in the left panel is the location mean change in species richness and abundance. The right panels plot the marginal distributions of the time trend estimates for all three biodiversity metrics.

Figure 3: The Biodiversity - GDP Relationship

(a) Average effect

(b) By taxa

(c) By sample-average GDP per capita

(d) By regression quantiles
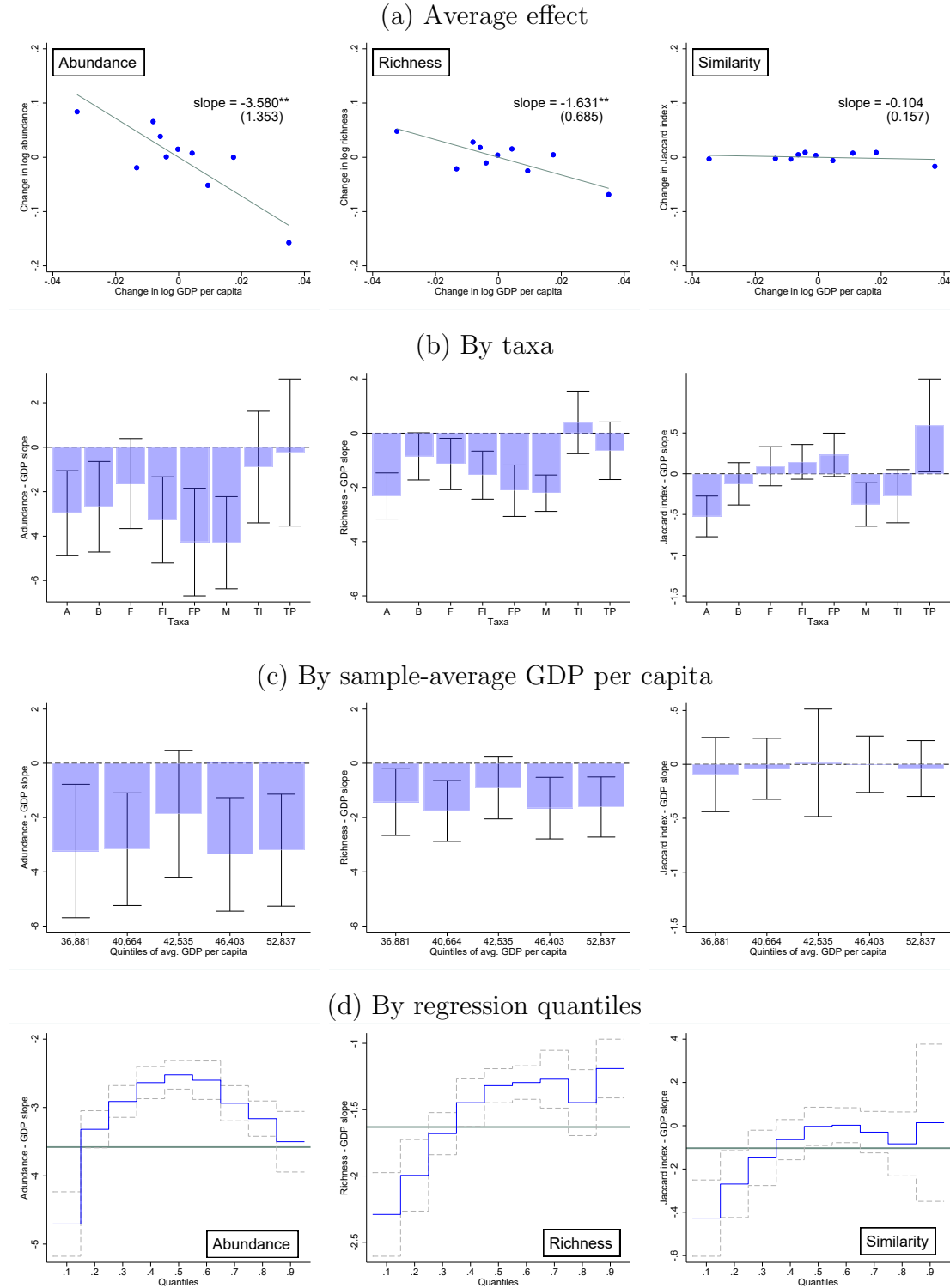
Notes: Panel (a) plots decile bin scatterplots of biodiversity and log GDP, both residualized with location-by-taxa and year fixed effects. The slope of the fitted line represents the OLS estimate $\hat{\beta}$ of equation (1). Numbers in parentheses show standard errors clustered at the state level. Panel (b)/(c)/(d) reports heterogeneous OLS estimates by taxa/sample-average GDP per capita/regression quantiles. In panel (b), abbreviations are for amphibians (A), birds (B), fish (F), freshwater invertebrates (FI), freshwater plants (FP), mammasl (M), terrestrial invertebrates (TI), and terrestrial plants (TP).

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Figure 4: Military Buildups and Biodiversity Outcomes

(a) Military contract spending as a share of state GDP
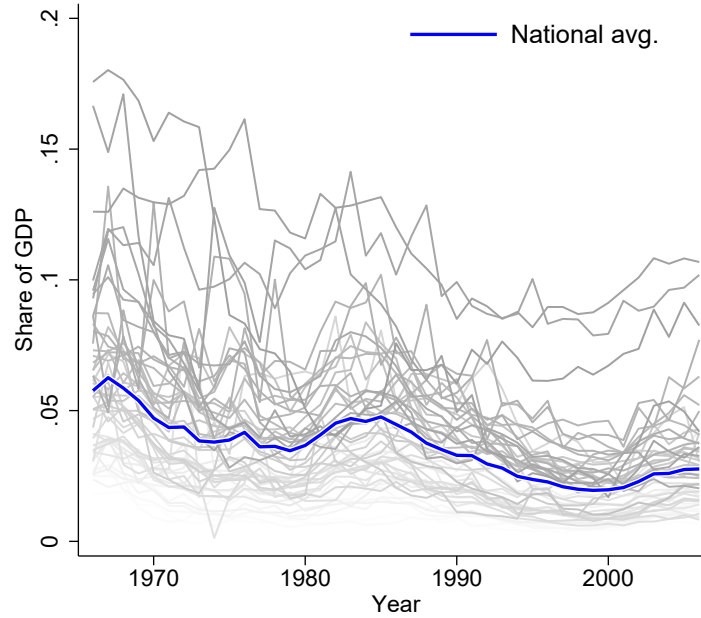
(b) Biodiversity and military spending shocks
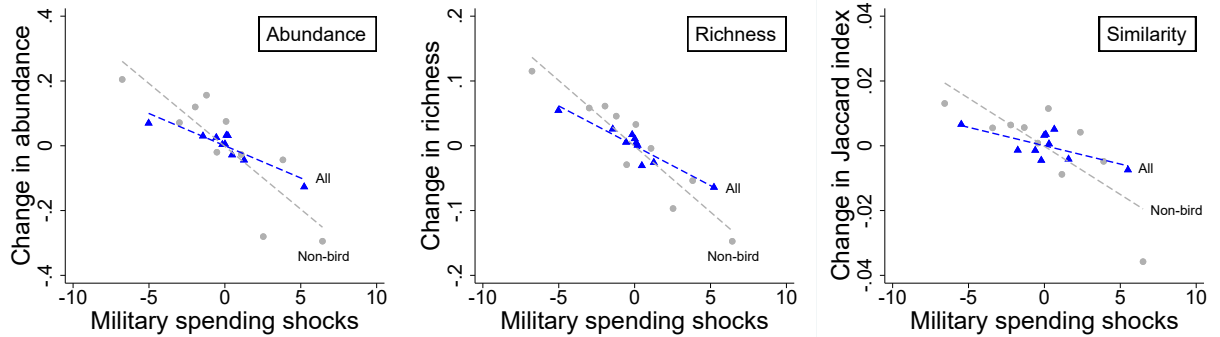
Notes: Panel (a) is adapted from Nakamura and Steinsson (2014). The graph shows state's annual prime military contract spending as a fraction of its GDP. Each line represents a state. Darker lines indicate states with a higher average military/GDP share between 1966 and 1971, the base period used to construct the shift-share shock variable. The thick, blue line in the middle represents national average. Panel (b) shows decile bin scatterplots of biodiversity and the military spending shock variable, both residualized with location-by-taxa and year fixed effects. The dashed blue line displays all-species results, and the dashed gray line displays subsample results with non-bird species.

Figure 5: Clean Air Act Regulations and Biodiversity Outcomes

(a) Share of sampling sites in nonattainment jurisdictions

(b) Location of nonattainment jurisdictions

Legend **
County Designated Nonattainment or Maintenance for 9 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 8 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 7 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 6 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 5 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 4 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 3 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 2 NAAQS Pollutants
County Designated Nonattainment or Maintenance for 1 NAAQS Pollutants

(c) Biodiversity outcomes and regulation shocks

Notes: Panel (a) shows the fraction of BioTIME sampling locations that were in counties designated by the U.S. Environmental Protection Agency as in "nonattainment" with respect to various air pollutants. Panel (b) plots counties with Clean Air Act Nonattainment or Maintenance designations with respect to the National Ambient Air Quality Standards (NAAQS) as of year 2019. Source: https://www3.epa.gov/airquality/greenbook/map/mapnmpoll.pdf. Panel (c) shows decile bin scatterplots of biodiversity and the regulation shock variable, both residualized with location-by-taxa and year fixed effects. The dashed blue line displays all-species results, and the dashed gray line displays subsample results with non-bird species.

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Figure 6: Instrumental Variables Estimation of the Effect of Air Pollution on Biodiversity

(a) Upwind pollution counties for Susquehanna, PA

(b) Upwind pollution counties for all counties in PA
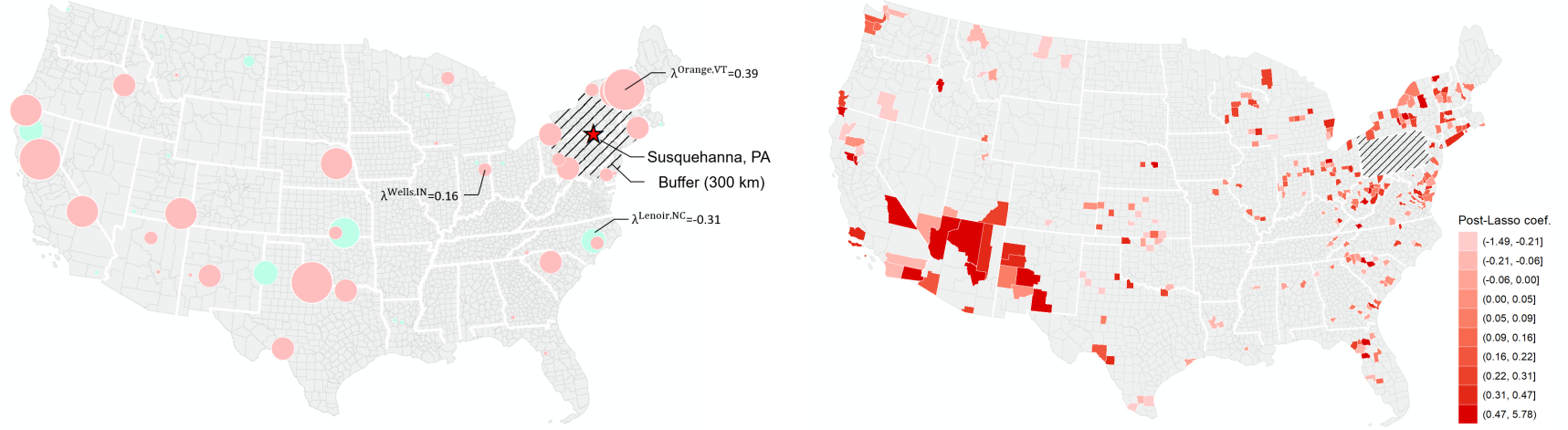
(c) Upwind pollution shocks, local pollution, and biodiversity outcomes
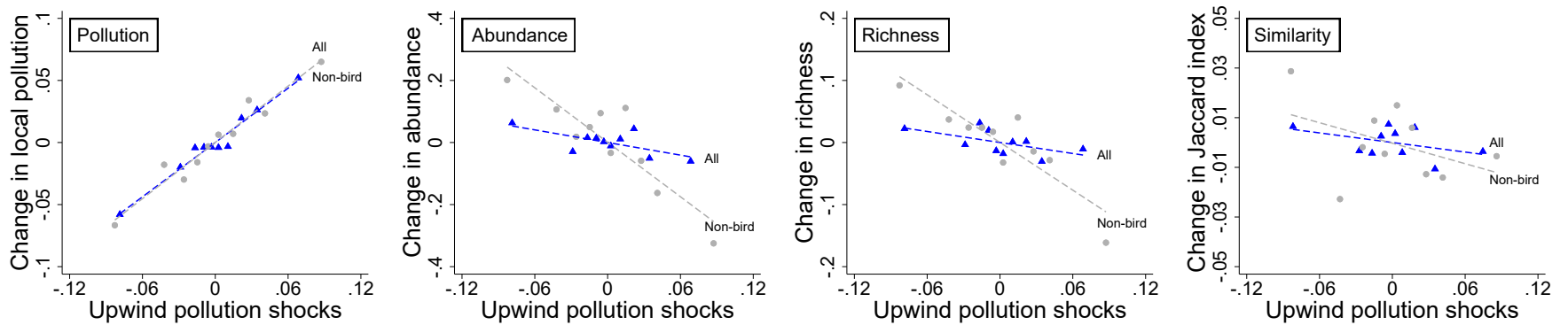
Notes: Panel (a) highlights 54 counties selected by a "zero-stage" LASSO regression of Susquehanna County, PA's daily aerosol pollution on all other 2,996 counties' upwind component vector pollution. The size of each circle is approximately proportional to the contributing county's post-LASSO elasticity coefficient. Red (green) circles correspond to positive (negative) correlation. In panel (b), we take all PA counties included in the BioTIME data, and highlight their LASSO-selected upwind pollution counties outside of the state of PA. Panel (c) shows decile bin scatterplots of local pollution and biodiversity outcomes against the upwind pollution IV. All variables are residualized with location-by-taxa and year fixed effects. The dashed blue line displays all-species results, and the dashed gray line displays subsample results with non-bird species.

Figure 7: Government Policy and Biodiversity Outcomes: Overall vs. Pollution Effects

(a) Military buildups

(b) Clean Air Act regulations
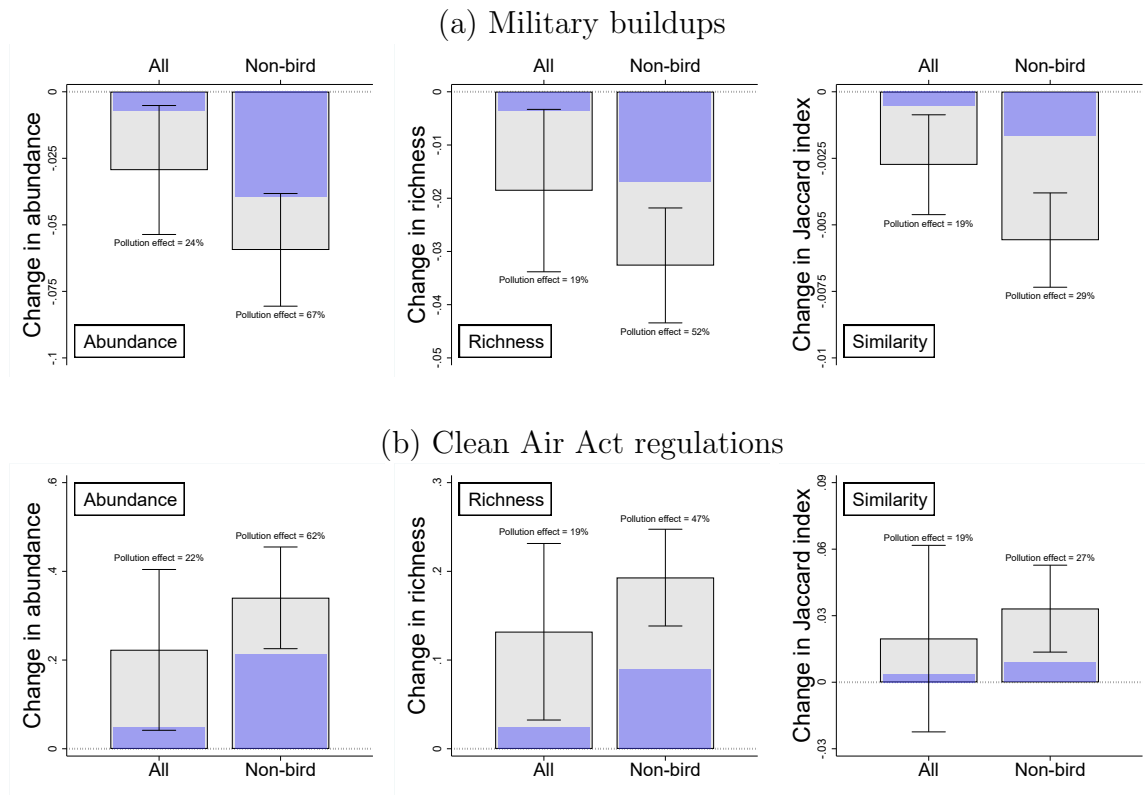
Notes: Bars and standard error range plots show the impacts of military buildup shocks (panel a) and Clean Air Act regulation shocks (panel b) on biodiversity outcomes. Blue bars ("pollution effects") indicate the predicted portion of the impacts that are explained by air pollution; these estimates are obtained by multiplying (i) the impacts of the military buildup shocks and Clean Air Act regulation shocks on pollution with (ii) the IV estimates of the effect of pollution on biodiversity outcomes.

Table 1: Summary Statistics

	(1) Observations	(2) Abundance	(3) Richness	(4) Similarity
All species	66,418	39,132 [1,203,156]	15.44 [19.62]	0.425 [0.269]
Amphibians	45	2,563 [2,557]	7.267 [2.526]	0.943 [0.099]
Birds	51,695	216.8 [1,967]	18.43 [20.5]	0.419 [0.252]
Fish	804	1,405 [6,330]	16.07 [8.501]	0.702 [0.128]
Freshwater invertebrates	445	5,566,008 [13,599,038]	24.32 [15.43]	0.715 [0.144]
Freshwater plants	39	2,747,857 [1,193,869]	83.87 [11.56]	0.470 [0.045]
Mammals	5,658	21.18 [174.2]	1.91 [1.958]	0.316 [0.339]
Terrestrial invertebrates	6,071	53.32 [520.2]	3.912 [11.12]	0.446 [0.239]
Terrestrial plants	1,661	1,281 [8,296]	6.515 [11.81]	0.651 [0.317]

Notes: Column 1 reports total number of site-by-year observations in the estimation data that correspond to different taxa groups. For biodiversity outcomes (columns 2-4), numbers show the means, and standard deviations are in brackets.

Table 2: Sector-Specific Income and Biodiversity Outcomes

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
Manufacturing	-0.504**	-0.366***	-0.021	-1.505***	-0.677***	-0.009
	(0.198)	(0.091)	(0.023)	(0.343)	(0.154)	(0.068)
Mining	-0.063	-0.008	-0.012	-0.274	0.090	-0.138***
	(0.045)	(0.025)	(0.011)	(0.188)	(0.071)	(0.045)
Timber and Logging	-0.021	-0.014	0.002	-0.287**	-0.138**	-0.007
	(0.035)	(0.021)	(0.002)	(0.114)	(0.051)	(0.008)
Agriculture	-0.002	-0.012	0.009	0.638***	0.126**	0.073***
	(0.063)	(0.023)	(0.008)	(0.158)	(0.057)	(0.017)
Construction	0.172	0.134	0.025	0.754	0.139	-0.075
	(0.356)	(0.090)	(0.059)	(0.628)	(0.206)	(0.132)
Services	-0.187	-0.289	-0.031	0.278	0.099	-0.087
	(0.558)	(0.205)	(0.060)	(1.462)	(0.372)	(0.298)
Observations	59,651	59,651	46,746	13,809	13,809	12,613

Notes: Each column corresponds to a regression. Categorizations are based on 2-digit SIC and NAICS codes. Sector income data are from U.S. Bureau of Economic Analysis 1969 to 2016. Agriculture includes agriculture and fishing. Services includes wholesale, retail, transportation, communications, electric, gas, and sanitary services, finance, and all other service. Columns 1-3 reports full sample estimation. Columns 4-6 excludes observations that correspond to bird species. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table 3: Military Spending Shocks and Biodiversity Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Policy Effect				Implied GDP Elasticity	
	GDP	Abundance	Richness	Similarity	Abundance	Richness	Similarity
Panel A. All species							
Military spending	0.299***	-1.341**	-0.823**	-0.164***	-	-	-
	(0.110)	(0.567)	(0.354)	(0.060)	-	-	-
$\widehat{GDP}$	-	-	-	-	-4.485***	-2.753**	-0.535***
	-	-	-	-	(1.594)	(1.226)	(0.183)
Kleibergen-Paap F-stat.	-	-	-	-	7.430	7.430	7.071
Observations	57,714	57,714	57,714	44,479	57,714	57,714	44,479
Panel B. Non-bird species							
Military spending	0.528***	-3.286***	-1.685***	-0.360***	-	-	-
	(0.087)	(1.075)	(0.624)	(0.057)	-	-	-
$\widehat{GDP}$	-	-	-	-	-6.225***	-3.193***	-0.638***
	-	-	-	-	(1.167)	(0.732)	(0.149)
Kleibergen-Paap F-stat.	-	-	-	-	37.05	37.05	34.46
Observations	11,861	11,861	11,861	10,335	11,861	11,861	10,335

Notes: Each panel-column is a separate regression. Outcome variables are in logs except for similarity which is a ratio (columns 4 and 7). Military spending shocks are national per capita procurement interacted with state's 1966-1971 average military-GDP ratio. Columns 5 through 7 report elasticity estimates of biodiversity outcomes with respect to GDP where the latter is instrumented for using military spending shocks in a 2SLS regression. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table 4: Environmental Regulation Shocks and Biodiversity Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Policy Effect				Implied GDP Elasticity	
	GDP	Abundance	Richness	Similarity	Abundance	Richness	Similarity
Panel A. All species							
Clean Air Act Nonattainment	-0.038***	0.226***	0.121***	-0.020	-	-	-
	(0.014)	(0.080)	(0.046)	(0.018)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-5.932***	-3.194***	-0.519
	-	-	-	-	(0.624)	(0.268)	(0.532)
Kleibergen-Paap F-stat.	-	-	-	-	7.841	7.841	8.874
Observations	54,887	54,887	54,887	42,406	54,887	54,887	42,406
Panel B. Non-bird species							
Clean Air Act Nonattainment	-0.053***	0.373***	0.193***	0.371***	-	-	-
	(0.007)	(0.038)	(0.025)	(0.007)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-7.005***	-3.631***	-0.704***
	-	-	-	-	(0.755)	(0.250)	(0.097)
Kleibergen-Paap F-stat.	-	-	-	-	50.57	50.57	49.56
Observations	13,331	13,331	13,331	12,161	13,331	13,331	12,161

Notes: Each panel-column is a separate regression. Outcome variables are in logs except for similarity which is a ratio (columns 4 and 7). Clean Air Act Nonattainment is the county's number of nonattainment designations in the county-year. Columns 5 through 7 report elasticity estimates of biodiversity outcomes with respect to GDP where the latter is instrumented for using nonattainment in a 2SLS regression. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table 5: Air Pollution and Biodiversity Outcomes

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
Pollution (OLS)	-0.703***	-0.322**	-0.074***	-2.072***	-1.020***	-0.070**
	(0.215)	(0.127)	(0.027)	(0.403)	(0.253)	(0.025)
$\widehat{\text{Pollution}}$ (IV)	-1.118**	-0.565***	-0.084**	-3.282***	-1.395***	-0.136
	(0.430)	(0.201)	(0.037)	(0.507)	(0.230)	(0.098)
Kleibergen-Paap F-stat.	271.0	271.0	224.0	208.2	208.2	319.4
Observations	53,496	53,496	41,058	12,726	12,726	11,599

Notes: Each cell corresponds to a regression. Outcome variables are in logs except for similarity which is a ratio (columns 3 and 6). Independent variables are county's annual logged Aerosol Optical Depth pollution level. The first row reports OLS regression estimates. The second row reports IV regression estimates, using county's upwind pollution shock as the instrumental variable for logged local pollution. The Kleibergen-Paap F-statistics of the first stages are reported at the bottom of the table. Columns 1-3 reports full sample estimation. Columns 4-6 excludes observations that correspond to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.
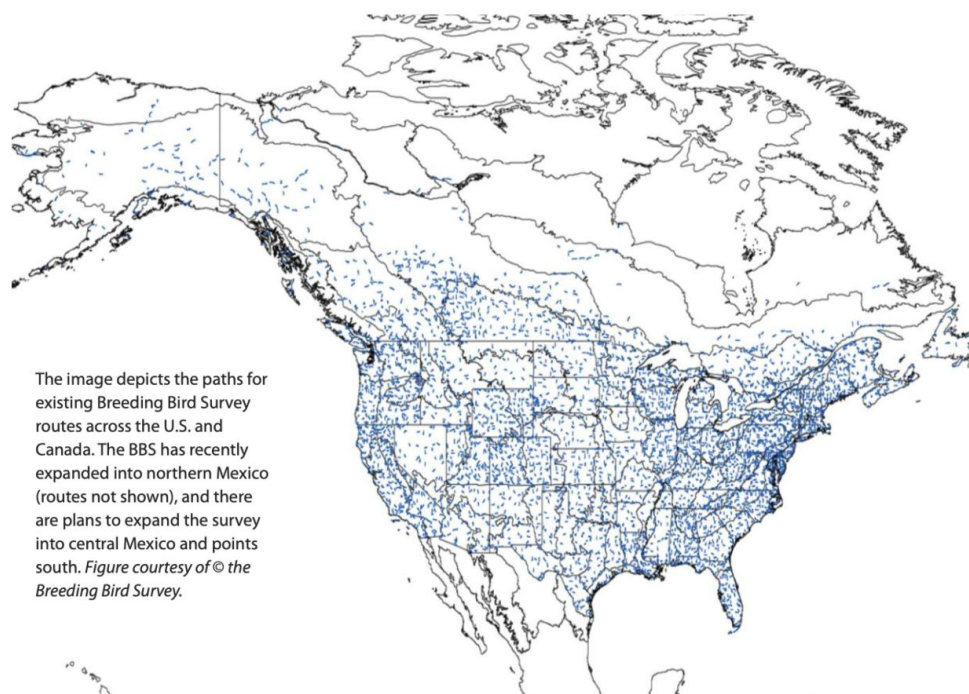
Appendix Figures and Tables

Figure A.1: North American Breeding Bird Survey routes across the U.S. and Canada

The image depicts the paths for existing Breeding Bird Survey routes across the U.S. and Canada. The BBS has recently expanded into northern Mexico (routes not shown), and there are plans to expand the survey into central Mexico and points south. *Figure courtesy of © the Breeding Bird Survey.*

Note: This figure is from Ziolkowski Jr et al. (2010). It shows the sample collecting routes in the North American Breeding Bird Survey (BBS), which is one of the studies in the BioTIME database. The BBS is a long-term and large-scale avian monitoring program that tracks the status and trends of North American bird populations. Professional bird observers collect bird population data at the same stops along the roadside survey routes during the avian breeding season every year. Over 4,100 survey routes are located across the continental U.S. and Canada.

Figure A.2: Sevilleta Long Term Ecological Research (SLTER) Program Map
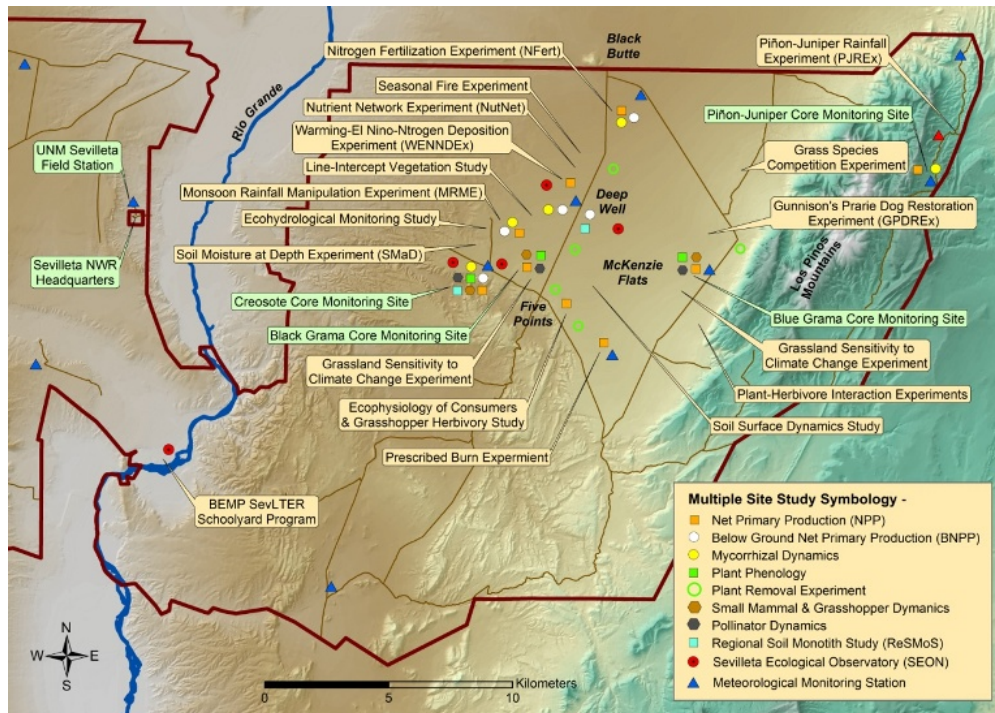
Note: This figure is from the project overview for the Sevilleta Long Term Ecological Research (LTER) Program at http://sevlter.unm.edu/. As shown in this figure, several studies included in BioTIME are conducted under the Sevilleta Long Term Ecological Research (SLTER) Program at the 100,000 hectare Sevilleta National Wildlife Refuge in central New Mexico. One study is the small mammals census from 1989 to 2008 (Friggens, 2008). There are 16,657 records for 27 distinct species covered in the study. Another study focuses on terrestrial plants in this wildlife refuge Muldavin (2001) collects 5,288 records for 123 distinct species.

Figure A.3: Dynamic Effects: Distributed Lag Models of the Biodiversity - GDP Relationship
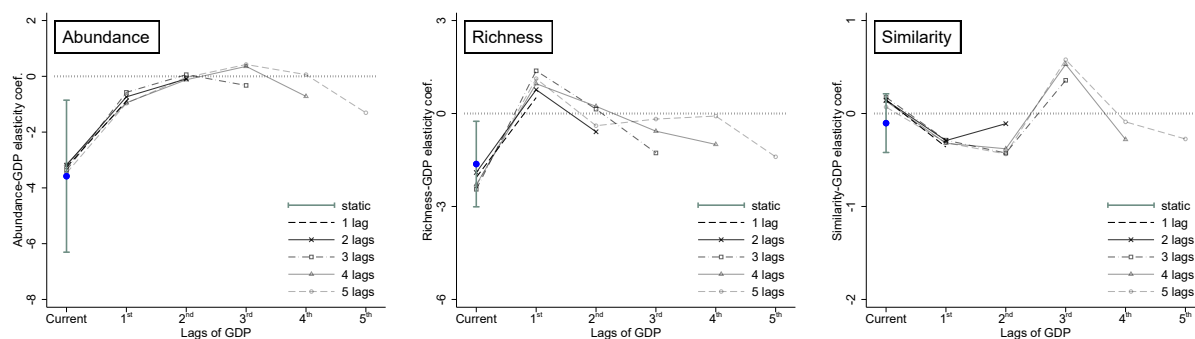
Notes: This figure plots coefficients when regressing biodiversity outcomes on the current and yearly lags of GDP. Each line represents a separate regression with different numbers of lags. For each outcome, the range bar shows point estimate and 95% confidence interval of the baseline, static specification with no lags of GDP. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level.

Figure A.4: Dynamic Effects: Panel Vector Autoregression (VAR) Impulse Response Functions

(a) Responses of biodiversity outcomes to GDP

(b) Responses of GDP to biodiversity outcomes
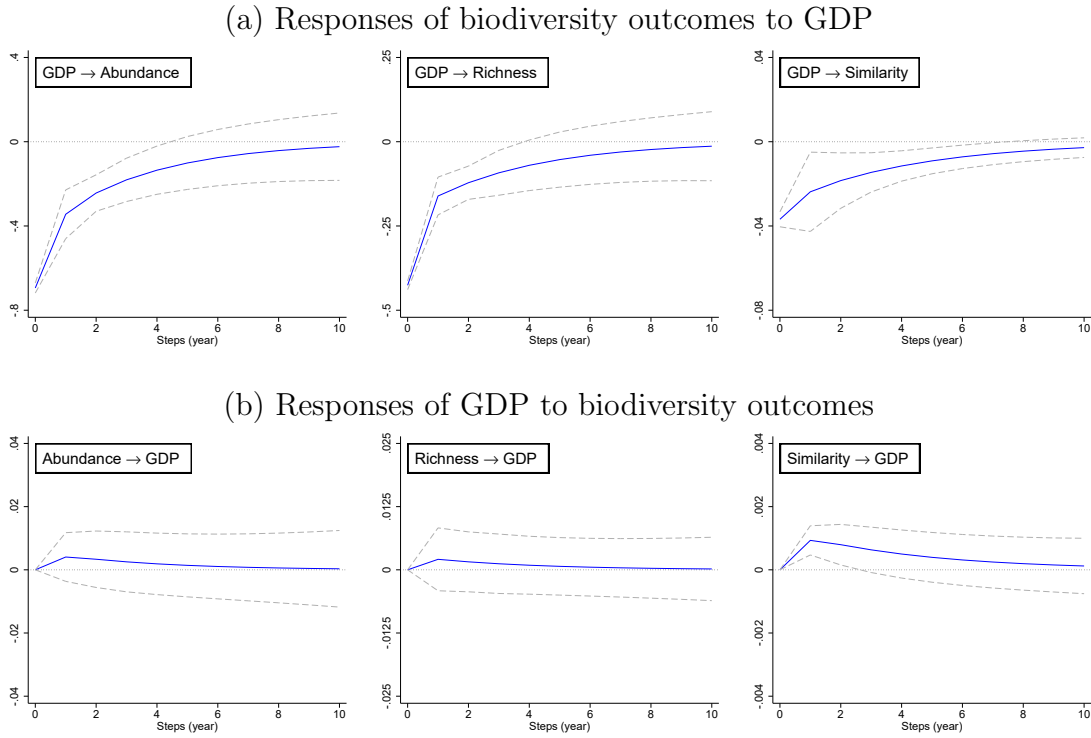
Notes: This figure plots orthogonalized impulse response functions from first-order panel vector autoregression (VAR). Three separate models are estimated for log GDP and log abundance (left column), log GDP and log richness (middle column), and log GDP and Jaccard index (right column). VAR models are estimated using GMM, with location-taxa fixed effects and time fixed effects removed prior to estimation, and with standard errors clustered at the state level. The underlying panel Granger causality Wald test statistics are 13.6 ($p < 0.001$), 22.2 ($p < 0.001$), and 3.66 ($p=0.056$) for the three variables in panel (a); and 1.07 ($p=0.301$), 0.41 ($p=0.522$), and 15.4 ($p < 0.001$) for the three variables in panel (b). Dashed lines show 95% confidence intervals constructed from 200 Monte Carlo simulations.

Figure A.5: Eco-regions of the Continental United States

Note: This map shows 85 Level III eco-regions – areas where ecosystems are generally similar – across the continental U.S. as of April 2013. Source: U.S. Environmental Protection Agency.
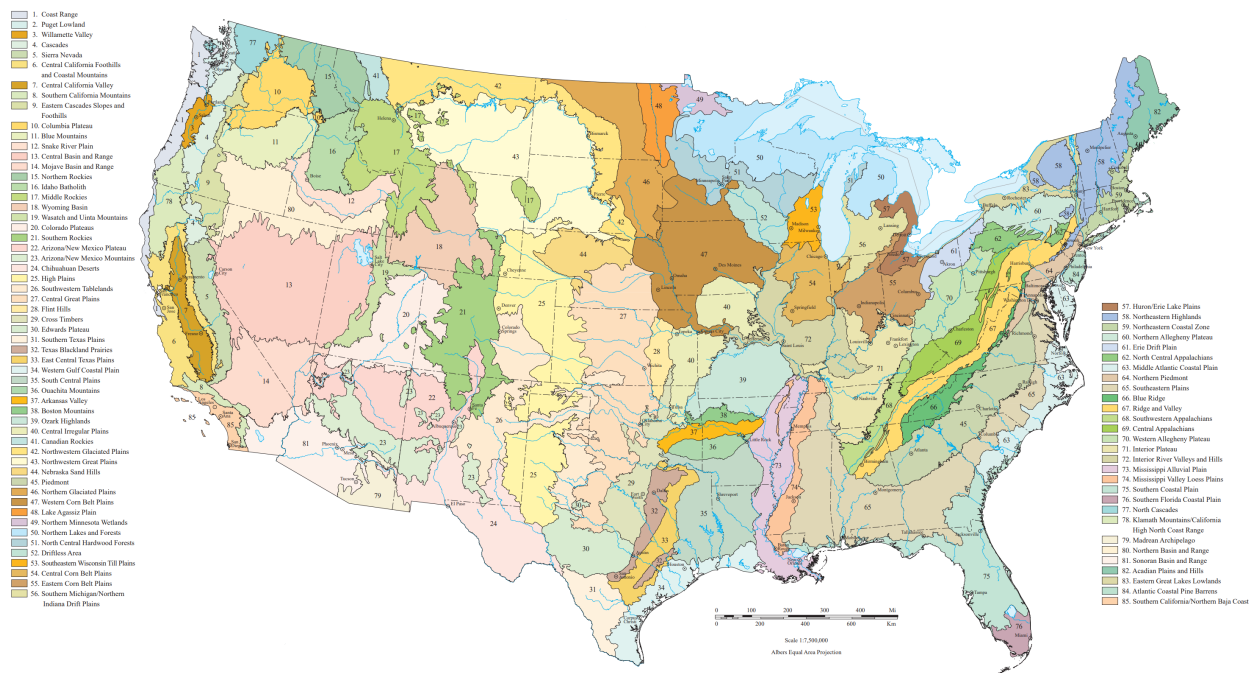
Figure A.6: Air Quality Effects of Military Spending and Environmental Regulation Shocks
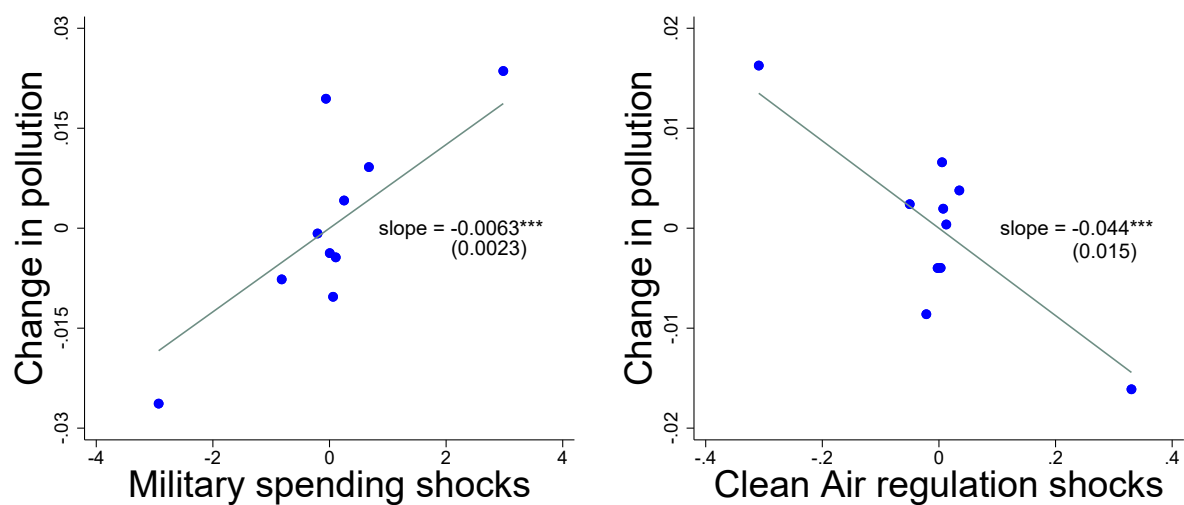
Notes: These figures show decile bin scatterplots of local pollution against the military buildup shocks (left panel) and the Clean Air Act regulation shocks (right panel). The underlying estimation follows equation (1), regressing pollution on military spending shocks and on Clean Air regulation shocks in two separate regressions. All variables are residualized with location-by-taxa and year fixed effects. Standard errors are clustered at the state level.

Figure A.7: Protected Areas Within 50 Kilometers of BioTIME Sampling Locations
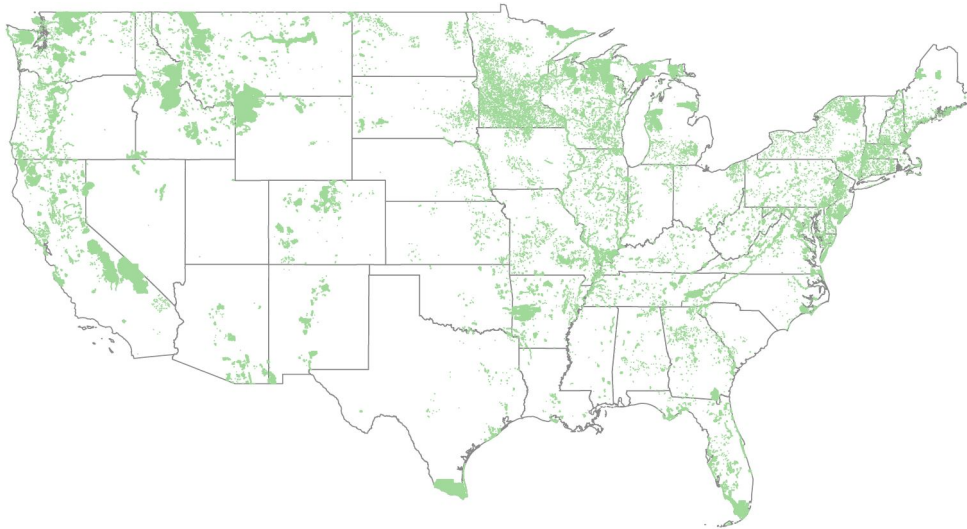
Notes: Green represents areas that were protected any time in the World Database on Protected Areas (WDPA) sample and within 50 kilometers of a sampling location in BioTIME.

Table A.1: Economic Conditions and Biodiversity Sampling Activities

	(1) Log(study duration)	(2) $\mathbb{1}$(start year)	(3)	(4) $\mathbb{1}$(end year)	(5)	(6) $\mathbb{1}$(missed year)	(7)
GDP growth (%)	0.013	-	-	-	-	-	-
	(0.054)	-	-	-	-	-	-
GDP$_t$	-	0.031	0.391	-0.213	-0.371	0.133	0.171
	-	(0.057)	(0.433)	(0.128)	(0.355)	(0.118)	(0.367)
GDP$_{t-1}$	-	-	-0.408	-	0.169	-	-0.036
	-	-	(0.482)	-	(0.351)	-	(0.321)
Data structure	cross-section	panel	panel	panel	panel	panel	panel
Observations	15,735	409,838	394,075	409,838	394,075	409,838	394,075

Notes: Estimation data underlying column 1 is a cross section of study locations. Columns 2 through 7 are based on balanced location-by-year panel data. Outcome variables are log number of years of a study location (column 1), an indicator for the study location's first sampling year (columns 2 and 3), an indicator for the study location's last sampling year (columns 4 and 5), and an indicator for nonsampling in the corresponding location-year (columns 6 and 7). GDP$_{t-1}$ is the log of lagged one year GDP. All regressions include location-by-taxa fixed effects, and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.2: Economic Production and Biodiversity Outcomes: Robustness to Outliers

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
Winsorize samples with extreme levels						
Within 1th-99th percentiles	-3.580**	-1.631**	-0.104	-5.906***	-3.304***	-0.371
	(1.353)	(0.685)	(0.157)	(0.989)	(0.271)	(0.262)
Within 5th-95th percentiles	-3.444**	-1.580**	-0.107	-5.713***	-3.232***	-0.372
	(1.315)	(0.673)	(0.154)	(0.986)	(0.267)	(0.254)
Within 10th-90th percentiles	-3.145**	-1.453**	-0.111	-5.210***	-3.018***	-0.371
	(1.219)	(0.646)	(0.148)	(0.909)	(0.255)	(0.237)
Drop samples with extreme deviations						
Within +/- 4 S.D.	-4.446***	-2.398***	-0.146	-6.453***	-3.822***	-0.377*
	(1.113)	(0.586)	(0.112)	(0.389)	(0.136)	(0.202)
within +/- 3 S.D.	-4.296***	-2.379***	-0.142	-6.293***	-3.798***	-0.373*
	(1.078)	(0.588)	(0.108)	(0.357)	(0.159)	(0.196)
Within +/- 2 S.D.	-4.027***	-2.392***	-0.192*	-6.131***	-4.051***	-0.437**
	(0.997)	(0.616)	(0.111)	(0.323)	(0.168)	(0.204)

Notes: Each cell corresponds to a separate regression, which follows equation 1. We report coefficients on log per capita GDP. Outcome variables are in logs except for similarity which is a ratio (columns 3 and 6). Columns 1-3 report full sample estimation. Columns 4-6 exclude observations that correspond to bird species. The top section winsorizes the respective biodiversity outcomes at each sampling location at 1st and 99th percentile, 5th and 95th percentiles, and 10th to 90th percentiles in the three rows. The bottom panel truncates significant changes (greater than four/three/two standard deviations from the average change in magnitude) in the biodiversity outcome at the year-to-year basis at a given sampling location. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.3: The Biodiversity - GDP Relationship: Effects by Study Vintage and Duration

	(1) Abundance	(2) Richness	(3) Similarity
Panel A. By year of sampling			
From 1961 to 1997	-3.311**	-1.100	-0.227
	(1.316)	(0.918)	(0.303)
From 1998 to 2015	-3.042***	-1.421***	-0.163
	(0.901)	(0.509)	(0.174)
Observations (1961-1997)	22,603	22,603	12,683
Observations (1998-2015)	31,360	31,360	28,256
Panel B. By year when study began			
From 1961 to 1993	-3.617***	-1.943***	0.014
	(1.027)	(0.531)	(0.111)
From 1994 to 2013	-3.358	-1.054	-0.074
	(2.015)	(1.079)	(0.140)
Observations (1961-1993)	14,535	14,535	14,208
Observations (1994-2013)	40,352	40,352	28,198
Panel C. By study length			
Duration ≥ 20y	-4.325***	-2.371***	-0.073
	(0.947)	(0.436)	(0.085)
Duration 5y to < 20y	-4.165*	-1.763*	0.072
	(2.112)	(0.959)	(0.148)
Duration ≤ 5y	0.943	1.460	-0.235
	(1.665)	(1.498)	(0.272)
Observations (≥ 20y)	12,658	12,658	12,643
Observations (5y to 20y)	26,799	26,799	23,210
Observations (≤ 5y)	15,450	15,450	6,572

Notes: Each cell represents a separate regression per equation (1). Each column corresponds to a different biodiversity metric. Panel A reports separate regressions by before and after median year of sampling. Panel B reports separate regressions by before and after median year of a study's first year of sampling. Panel C reports separate regressions corresponding to studies that fall below 25th, between 25th and 75th, and over 75th percentile of the study duration distribution. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.4: Other Measures of Biodiversity

	(1) Gini	(2) Shannon	(3) Sorensen	(4) Gini	(5) Shannon	(6) Sorensen
	Panel A. All species			Panel B. Non-bird species		
GDP	-1.845*	-2.176**	-0.213	-3.881***	-4.305***	-0.660**
	(0.946)	(1.021)	(0.197)	(0.630)	(0.666)	(0.269)
Observations	57,714	57,714	44,479	11,861	11,861	10,335

Notes: Each cell represents a separate regression. Each column corresponds to a different biodiversity metric: the Gini index (columns 1 and 4), the Shannon index (columns 2 and 5), and the Sorensen similarity index (columns 3 and 6). Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.5: The Biodiversity - GDP Relationship: Alternative Unit of Analysis

	(1)	(2)	(3)	(4)
	#Obs.	Abundance	Richness	Similarity
Panel A. All species				
Income at county	60,406	-0.697*	-0.459**	-0.026
		(0.381)	(0.225)	(0.033)
Income at Level III Eco-region	60,343	-1.014	-0.714	0.077
		(1.114)	(0.531)	(0.161)
Aggregation: 3-km hex. bin	11,915	-1.209***	-0.454***	-0.066
		(0.325)	(0.136)	(0.062)
Aggregation: 10-km hex. bin	9,881	-0.887**	-0.288*	-0.061
		(0.377)	(0.154)	(0.059)
Aggregation: 16-km hex. bin	8,896	-0.840**	-0.206	-0.076
		(0.379)	(0.187)	(0.055)
Panel B. Non-bird species				
Income at county	14,306	-3.858***	-2.220***	-0.226
		(0.873)	(0.255)	(0.140)
Income at Level III Eco-region	14,213	-3.369**	-2.375***	-0.158
		(1.524)	(0.327)	(0.306)
Aggregation: 3-km hex. bin	1,401	-1.699**	-0.631**	-0.261
		(0.672)	(0.271)	(0.159)
Aggregation: 10-km hex. bin	923	-1.719**	-0.586	-0.145
		(0.798)	(0.354)	(0.145)
Aggregation: 16-km hex. bin	694	-1.417*	-0.443	-0.132
		(0.756)	(0.518)	(0.139)

Notes: This table reports the panel OLS estimation results using equation (1) when economic production is measured at alternative geographic levels and when the data are aggregated up to hexagon bins of various resolution. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include grid-by-taxa and year fixed effects. Standard errors are clustered at the county (first row or each panel), eco-region (second row), and hexagon grid level (third to fifth rows). *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.6: The Biodiversity - GDP Relationship: Dynamic Specification

| | (1) | (2) | (3) | (4) | (5) | (6) |
	Abundance		Richness		Similarity	
Panel A. All species						
GDP_{t+1}	-	0.655	-	0.269	-	-0.106
	-	(0.848)	-	(0.607)	-	(0.120)
GDP_t	-3.580**	-3.705***	-1.631**	-2.246***	-0.104	0.271
	(1.353)	(1.199)	(0.685)	(0.671)	(0.157)	(0.271)
GDP_{t-1}	-	-1.006	-	0.417	-	-0.377
	-	(0.760)	-	(0.661)	-	(0.445)
Observations	54,887	54,176	54,887	54,176	42,406	41,729
Panel B. Non-bird species						
GDP_{t+1}	-	-0.229	-	-0.776	-	0.091
	-	(3.322)	-	(1.551)	-	(0.164)
GDP_t	-5.903***	-5.754	-3.302***	-4.043	-0.368	0.392*
	(0.990)	(4.809)	(0.271)	(2.448)	(0.262)	(0.206)
GDP_{t-1}	-	-0.420	-	1.752	-	-1.129**
	-	(1.364)	-	(1.191)	-	(0.415)
Observations	13,331	13,011	13,331	13,011	12,161	11,875

Notes: Outcome variables are in logs except for similarity which is a ratio (columns 5 and 6). GDP_{t-1} is the log of lagged one year GDP. GDP_{t+1} is the log of GDP one year in the future. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include location-by-taxa fixed effects, and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.7: The Biodiversity - GDP Growth Relationship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Abundance			Richness			Similarity	
Panel A. All species									
GDP growth	-3.00	-2.89*	-2.48*	-2.01	-1.98	-1.64	0.16	0.17	0.15
	(2.30)	(1.65)	(1.35)	(1.52)	(1.36)	(1.12)	(0.31)	(0.38)	(0.38)
Avg. GDP growth		-11.39*	-3.60		-2.93	3.53		-1.09	-1.56
(last 5-y)		(6.23)	(6.35)		(2.83)	(3.83)		(0.79)	(1.15)
Max. GDP growth			-7.68***			-6.36**			0.44
(last 5-y)			(2.16)			(2.46)			(0.43)
Observations	37,644	37,644	37,644	37,644	37,644	37,644	33,789	33,789	33,789
Panel B. Non-bird species									
GDP growth	-6.24**	-3.92***	-2.49**	-5.06***	-3.98***	-3.32***	0.32***	0.67**	0.37*
	(2.40)	(1.26)	(0.90)	(1.39)	(0.92)	(0.79)	(0.07)	(0.25)	(0.21)
Avg. GDP growth		-26.10***	-10.26**		-12.14***	-4.87**		-4.16***	-7.77***
(last 5-y)		(4.57)	(3.75)		(1.53)	(1.74)		(0.98)	(1.38)
Max. GDP growth			-9.83***			-4.51***			2.28***
(last 5-y)			(2.59)			(1.30)			(0.52)
Observations	11,236	11,236	11,236	11,236	11,236	11,236	10,443	10,443	10,443

Notes: Outcome variables are in logs except for similarity which is a ratio (columns 7-9). "GDP growth" is annual GDP per capita growth rate. "Avg. GDP growth " is the average GDP per capita growth rate for the past 5 years, from t-5 to t-1. "Max. GDP growth" is the maximum annual GDP per capita growth rate in the past 5 years. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include location-by-taxa fixed effects and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.8: Biodiversity-GDP Elasticity Estimates: Robustness Specifications

	(1) Abundance	(2) Richness	(3) Similarity
Panel A. Policy shocks = Military spending			
IV base period 1966-2006	-5.086**	-3.127*	-0.529***
	(2.175)	(1.649)	(0.190)
	[6.035]	[6.035]	[6.186]
Shocks × state FEs as IVs	-4.575**	-1.932*	-0.218
	(1.950)	(1.031)	(0.145)
	[1.794]	[1.794]	[2.168]
Multiple IVs LIML	-4.485***	-2.753**	-0.535***
	(1.594)	(1.226)	(0.183)
	[7.430]	[7.430]	[7.071]
Census Division trends	-5.844***	-3.049*	-0.750***
	(2.050)	(1.554)	(0.226)
	[6.509]	[6.509]	[7.034]
County-level income	-5.189***	-3.102***	-0.587**
	(1.174)	(0.755)	(0.241)
	[4.634]	[4.634]	[4.953]
Panel B. Policy shocks = Clean Air Act nonattainment			
1(any nonatt. status) as IV	-6.518***	-3.730***	0.267
	(1.246)	(0.552)	(0.254)
	[8.823]	[8.823]	[8.963]
Multiple pollutants nonatt. IVs	-5.261***	-3.118***	-0.275
	(0.620)	(0.291)	(0.494)
	[4.448]	[4.448]	[4.624]
Multiple IVs LIML	-5.932***	-3.194***	-0.519
	(0.624)	(0.268)	(0.532)
	[7.841]	[7.841]	[8.874]
Census Division year trends	-6.081**	-3.819***	-0.828
	(2.285)	(0.985)	(0.850)
	[9.715]	[9.715]	[10.311]
County-level income	-6.160***	-3.010***	-0.636
	(1.624)	(0.553)	(0.241)
	[4.107]	[4.107]	[4.419]

Notes: Each cell is a separate regression. Row names specify the robustness checks described in Section 5.1 and 5.2. This Table reports the implied biodiversity-GDP elasticity estimates using military spending shocks (Panel A) and Clean Air regulation shocks (Panel B) as the underlying source of policy variation. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.9: Urbanization and Biodiversity Outcomes

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
Urbanization (50-km radius)	-11.91***	-6.39**	-2.26	-16.67**	-10.53**	-5.24***
	(4.01)	(2.38)	(1.54)	(6.94)	(3.71)	(1.69)
Urbanization (100-km radius)	-11.59***	-5.15***	-3.94***	-13.79***	-7.48***	-4.47***
	(2.29)	(1.53)	(1.40)	(2.87)	(2.23)	(0.99)
Urbanization (county)	-1.73	-0.69	-0.29	-16.14***	-8.59***	-4.04
	(1.36)	(0.66)	(0.38)	(4.18)	(1.56)	(2.35)
Observations	19,611	19,611	17,188	6,830	6,830	6,752

Notes: Each cell corresponds to a regression. Outcome variables are in logs except for Similarity which is a ratio (columns 3 and 6). Independent variables are logged urban areas within 50-km radius of the sampling location (first row), logged urban areas within 100-km radius of the sampling location (second row), and logged urban areas of the county (third row). Columns 1-3 reports full sample estimation. Columns 4-6 excludes observations that correspond to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.10: Conservation Policy and the Biodiversity - GDP Relationship: Protected Areas

	(1)	(2)	(3)	(4)	(5)	(6)
	Abundance		Richness		Similarity	
Panel A. All species						
GDP	-3.798***	-3.410***	-1.721**	-1.490***	-0.219	-0.255*
	(1.341)	(1.142)	(0.684)	(0.611)	(0.147)	(0.140)
GDP × %Areas protected	1.765*	2.028*	0.732	0.892	0.890***	0.864***
	(1.023)	(1.158)	(0.500)	(0.555)	(0.252)	(0.237)
GDP × #Fragmented areas	-	-0.206*	-	-0.144	-	0.023
	-	(0.108)	-	(0.088)	-	(0.015)
Observations	54,907	54,907	54,907	54,907	42,426	42,426
Panel B. Non-bird species						
GDP	-6.510***	-4.229***	-3.277***	-2.339***	-0.652***	-0.754***
	(0.787)	(0.813)	(0.261)	(0.436)	(0.158)	(0.087)
GDP × %Areas protected	7.484	13.976**	-0.263	1.805	3.217**	3.520**
	(4.812)	(6.080)	(0.938)	(1.269)	(1.237)	(1.286)
GDP × #Fragmented areas	-	-0.731*	-	-0.147	-	-0.115
	-	(0.418)	-	(0.139)	-	(0.088)
Observations	13,351	13,351	13,351	13,351	12,181	12,181

Notes: Outcome variables are in logs except for Similarity which is a ratio (columns 5 and 6). "%Areas protected" is the fraction of protected areas within a 50km radius of the sampling location. "#Fragmented areas" is the number (in 1,000s) of discontiguous protected areas within 50km radius of the sampling location. Smaller numbers of discontiguous areas indicate that each protected area is larger on average. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include main effect terms, location-by-taxa fixed effects, and year fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.