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ABSTRACT

The environmental impacts of economic activities extend beyond those directly affecting humans. This paper provides new evidence on the link between economic activity and ecosystem decline using a novel dataset that compiles longitudinal ecological sampling information at tens of thousands of locations across the United States between 1960 and 2015. Local shocks in economic activities, such as those driven by national military buildups, led to a significant reduction in species abundance, diversity, and stability, with one-third of the observed effects explained by the causal impact of air pollution. Government environmental regulations significantly mitigated pollution externalities.

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1 Introduction

Economists have long been interested in understanding the implications of economic activities on the natural environment.[1] One well-known concept is *externality*, the market failure associated with the unpriced damage caused by byproducts of economic activities, such as air and water pollution. Empirical research has centered on how these externalities affect various aspects of human well-being – such as the causal effect of industrial pollution on human health – which has been instrumental in shaping economic activities and regulatory policies over recent decades (e.g., Landrigan et al., 2018).

Humans are only *one* of the many species that may be affected by environmental externalities. Pollution and habitat destruction can disrupt a wide range of wildlife (Foster and Rosenzweig, 2003; Polasky et al., 2005; Frank and Schlenker, 2016; Jayachandran et al., 2017), diminishing their ability to provide ecosystem services (Tilman et al., 1996; Cottingham et al., 2001; Cardinale et al., 2012). These services not only have ecological value – such as increasing the resilience of ecosystems to shocks – but many also ultimately contribute to human prosperity. For example, ecosystem diversity improves agricultural production (Worm et al., 2006; Dainese et al., 2019), mitigates income shocks from natural disasters (Noack et al., 2019), supports drug discovery (Simpson et al., 1996; Rausser and Small, 2000; Costello and Ward, 2006), and provides non-market and non-use values (Loomis and White, 1996; Kolstoe and Cameron, 2017). This coupling between the economy and ecosystems is becoming increasingly evident, and how to systematically monitor and analyze this interrelationship – as well as incorporate it into sustainable development and macroeconomic decision making – is a crucial current policy discussion (Frank and Sudarshan (2024), Frank (2024)).[2]

This paper aims to study the external effects of economic activity on the ecosystem. A major obstacle hindering empirical progress by economists has been the lack of large-scale data on species observations (Geijzendorffer et al., 2016; König et al., 2019). Existing datasets commonly provide only cross-sectional information on the geographic extent of species, such as the Red List of Threatened Species (IUCN, 2021), or panel data for only a single taxonomic class, such as the North American Breeding Bird Survey (USGS, 2014).

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[1] Economists have had early and sustained interest in understanding the value of ecosystems and their interaction with economic prosperity (e.g., Weitzman, 1992, 1998; Arrow et al., 1995; Brown Jr and Shogren, 1998; Fullerton and Stavins, 1998; Heal, 2000; Brock and Xepapadeas, 2003). This early body of work was followed by a considerable amount of applied research in the field of environmental and resource economics (Brock and Taylor, 2005; Ando and Langpap, 2018; Dasgupta, 2021).

[2] For a recent discussion, see Federal Register 2023-01608 "National Strategy to Develop Statistics for Environmental-Economic Decisions." `https://www.whitehouse.gov/wp-content/uploads/2023/01/Natural-Capital-Accounting-Strategy-final.pdf`

These limitations make it difficult to study the link between the economy and the ecosystem at a broad scale. We make progress by introducing a novel database to economic research that compiles hundreds of individual ecological studies that maintain longitudinal information on the counts or biomass of relevant species (BioTIME: Dornelas et al., 2018; Blowes et al., 2019), which lets us construct widely-used metrics for capturing changes in the ecosystem. These studies – many spanning several decades – are consistent in their sampling protocols within each sampling location over time (what ecologists call "assemblage time series"), making the year-over-year variation in sampling outcomes likely to reflect genuine changes in underlying ecosystem conditions. In total, the database contains millions of records of species counts at over 15,000 different sampling locations, providing significant coverage of various taxonomic classes. This includes 80 percent of the known bird species, 40 percent of mammals, 30 percent of amphibians, and 25 percent of freshwater fish, among others.

Our analysis consists of five main elements.

Measurement. We use the BioTIME database to generate three sets of measures that enable us to quantify changes in ecosystem conditions over time across various locations spanning the past 50 years: (1) *abundance*, which captures the total quantity of individuals observed at a given location in a given year; (2) *richness*, which captures the total number of distinct species observed; and (3) *similarity*, which measures the proportion of shared species between two consecutive years of a study and reflects the stability of the species composition. We refer to these measures collectively as *biodiversity*. We also construct other standard biodiversity metrics widely used in ecology literature, such as Gini, Shannon, and Sorensen indices.

Correlation. Next, we examine the correlation between these measures and economic activity. We use a standard panel fixed effects model with one of the biodiversity metrics as the outcome variable, state GDP per capita as the regressor, and a set of location-taxon fixed effects and year fixed effects.[3] This panel data exercise documents a strong, negative association between economic activity and biodiversity outcomes: a one percent increase in economic activity is associated with a 3.6 percent decrease in species abundance and 1.6 percent decrease in richness in a given sampling location. We also find that economic activity is associated with reduced stability – a one percent increase in economic activity in a year decreases the share of species common in two consecutive years by 10% – although this association is imprecisely estimated.

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[3] We use state per capita GDP to capture the intensity of economic output in the broad geographic area. In the Appendix, we report robustness results using alternative definitions of economic activity, such as state total GDP (without the normalization by population), or county total or per capital *income* measured at the county level instead of the state level. We also report a robustness check using county GDP though the measurement is only available after 2001 which covers 30% of our study sample.

Our exercise also reveals a set of descriptive features of the correlation: First, the negative association between economic activity and biodiversity appears to be widespread across various taxa, including mammals, birds, amphibians, fish, and freshwater plants and invertebrates. Second, there is substantial distributional heterogeneity, where the negative association at the lowest decile of biodiversity is almost twice as large as the average. This finding suggests that economic activity may lead to biodiversity losses to a greater extent in regions with lower biodiversity, consistent with the idea that biodiversity enhances ecosystem resilience against adverse shocks (Hautier et al., 2015). Third, both the current *shock* in economic activities and longer-term activities *trajectory*, such as the growth rate over the past few years, matter for biodiversity outcomes.

Causality. Does economic activity *cause* changes in biodiversity? The associational evidence may be confounded by omitted factors correlated with both ecosystem outcomes and local economic output. The direction of causation may also go in the reverse direction, for example, if better ecosystem conditions lead to increased economic output through improved natural resources for production or tourism.

To establish causality, we isolate changes in local economic activities coming from plausibly exogenous government actions. We exploit variation in local economic activities driven by U.S. military buildups, which are heavily influenced by geopolitical factors and unexpected military events. This research strategy has been widely used in the empirical macroeconomics literature to estimate the causal effect of government spending on total economic output, i.e., the fiscal multiplier (Hall, 2009; Barro and Redlick, 2011; Ramey, 2011; Nakamura and Steinsson, 2018). Following Nakamura and Steinsson (2014), we 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. We find that increased production resulting from military spending has a significant, negative effect on biodiversity outcomes. The magnitude of the biodiversity-GDP link estimated using the quasi-experimental variation is greater, but of the same order of magnitude as the panel correlational estimates. We will provide additional discussion on effect sizes below.

Channels. Why does economic activity worsen biodiversity? Our estimates encompass various mechanisms – such as air and water pollution, land use changes, among other factors – through which economic activities can affect biodiversity. Disentangling all these mechanisms empirically is challenging; we focus on assessing the importance of one particular channel: air pollution externalities associated with economic activities.

Numerous studies have established the negative impact of air pollution on human health (e.g., Chen et al., 2013; Dominici et al., 2014; Schlenker and Walker, 2016; Deryugina et al., 2019), but pollution's detrimental effects may extend beyond the human population. For example, pollution can harm avian species (Brown et al., 1997; Liang et al., 2020), cause the acidification of lakes through emissions of sulfates and nitrates, which impacts freshwater communities (Brönmark and Hansson, 2002), and alter habitat conditions, food supplies, or species interactions (Agathokleous et al., 2020).[4]

We measure the importance of the air pollution channel 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. Second, we estimate the impact of the military spending shocks 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 military shocks on biodiversity *through air pollution*. We find that pollution accounts for 20-60 percent of the reduced form effect of military shocks, suggesting air pollution is a first-order pathway underlying the GDP-biodiversity link.

Besides pollution, another crucial channel that has received considerable attention in the literature is land use (e.g., Noack et al., 2021; Madhok, 2023). We briefly explore this channel, leveraging remote-sensing measurements of urbanization which are available for the later half of our study sample. We present descriptive evidence indicating that escalated urbanization correlates with a significant decrease in biodiversity outcomes.

Regulations. Since air pollution is a key channel through which military buildups affect ecosystems, the final part of the paper considers the role of environmental regulations.

We investigate whether the environmental regulations implemented by the United States to safeguard human health have also resulted in co-benefits for the protection of ecosystems. We study the impact of the landmark Clean Air Act (CAA) implemented by the U.S. Environmental Protection Agency, which set forth a set of nationwide standards for outdoor air quality. Each year, counties across the U.S. are assessed against these standards, and those that fail to comply are labeled as violating the standards (referred to as "nonattainment"). Regulators impose extensive emission reduction mandates on jurisdictions that fail to meet specified outdoor air quality benchmarks, resulting in significant decreases in local economic activities and improvements in air quality (e.g., Becker and Henderson, 2000; Greenstone,

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[4] The present knowledge on the effects of air pollution on biodiversity is largely confined to studies conducted in laboratories or focused on individual cases related to the toxicity of pollution exposure (Newman, 1979; Llacuna et al., 1993; Gilmour et al., 2001; Salmón et al., 2018).

2002; Greenstone et al., 2012; Walker, 2013). We present novel findings that the CAA regulations had a notable, positive impact on biodiversity outcomes: nonattainment designations led to significant improvements in species abundance, richness, and stability.

Building upon our earlier findings on the role of land use changes, we further examine protected areas policy – a widely adopted approach to land protection, which includes the designation and management of national parks, wilderness areas, and nature reserves, among others, to promote conservation (IUCN, 2021). Our analysis indicates that protected areas policy may help to alleviate the negative effects of economic activity shocks on the environment.

Massive wildlife losses in recent decades indicate a new age of human-caused mass extinction (e.g., Pimm et al., 2014; Ceballos et al., 2015), and highlight the pressing need to consider the trade-offs between economic growth and conservation (Foster and Rosenzweig, 2003; Polasky et al., 2005; Frank and Schlenker, 2016; Jayachandran et al., 2017; Watson et al., 2019). An emerging economics literature examines the coupling between the economy and ecosystems. Some of this literature focuses on the value of ecosystems to society, such as the value of vultures in mitigating human mortality and wolves in reducing car accidents (Raynor et al., 2021; Frank and Sudarshan, 2024). On the other hand, a large share of the literature studies how economic activity affects ecosystems. Earlier work in this area performed correlational studies at state or national levels on economic activity and broad measures of biodiversity (Dietz and Adger, 2003; Czech et al., 2012). More recent work has been increasingly focused on smaller spatial scales, a limited number of species, and narrower economic activities of interest in order to leverage newly available granular data and quasi-experimental variation in economic drivers (Asher et al., 2020; Li et al., 2020; Liang et al., 2020; Cole et al., 2021; Garg and Shenoy, 2021; Noack et al., 2021; Strobl, 2021; Madhok, 2023).[5]

We advance this literature in several ways. First, we study the effects of economic activity on biodiversity across a wide range of taxa such as birds, mammals, and plants using actual measurements from the ecology literature. Second, we provide some of the first causal estimates of the effects of economic activity by using quasi-random variation in local GDP induced by changes in national military spending. Third, we provide new evidence that particulate matter is a key mechanism behind the economic activity-biodiversity relationship and that there appears to be heterogeneous effects on bird vs non-bird taxa.

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[5] For example, Asher et al. (2020), Garg and Shenoy (2021), and Madhok (2023) all study economic development (e.g. roads) and forest cover in India, while Li et al. (2020), Liang et al. (2020), Noack et al. (2021), Strobl (2021), and Cole et al. (2021) study how pesticides, pollution, and agricultural land use affects birds.

While our estimates speak to causal effects for the species and ecosystems covered within the BioTIME sample, broader quantitative applicability requires further comparative analyses with studies of similar approach and scope. The issue of non-representativeness is a ubiquitous problem with current observational ecosystem data, which includes BioTIME despite it being a significant attempt to enhance coverage and internal consistency.[6] With that said, we provide a back-of-envelope calculation that lends credibility to our estimates: multiplying our estimated effect of economic activity on bird abundance by the economic growth during 1990-2015 yields a similar estimated bird population decline as a recent published estimate that North American bird populations fell by 13% during this time period (Rosenberg et al., 2019).

Another clarification pertains to our interpretation of the *causal effect of economic activity* and the underlying exclusion restriction. Economic activities are not increased in a vacuum, but through technological change or changes in clean and dirty inputs. We take our estimates as encompassing all underlying channels that accompany the shift in economic activities, such as changes in pollution and land use. Although we cannot isolate all possible channels individually, we prioritize one specific channel (air pollution) and establish its causal significance.

The rest of the paper is organized as follows. Section 2 describes data and measurement. Section 3 reports the correlational analysis. Section 4 presents causal analysis. Section 5 discusses regulations. Section 6 concludes the paper.

2 Data and Measurement

2.1 The BioTIME Database

To help readers conceptualize the structure of the BioTIME database, 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. In the Data Appendix, we present a comparison of BioTIME with popular alternative open-source datasets, highlighting some of its advantages.

Example Study 1: North American Breeding Bird Survey The North American Breeding Bird Survey (BBS) is a long-term and large-scale monitoring program that tracks

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[6] In economic analysis, reweighting by known population moments is a typical approach to address non-representativeness. However, in our study context, this is not a viable solution since the lack of information on population moments is precisely the issue.

the status and trends of North American bird populations (USGS, 2014). This study accounts for 17.6% of observations (location-taxa-year level) in the analysis data. 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). Hence for BBS, each survey route is a *sampling location* in BioTIME. 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 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). This study accounts for 0.13% of observations (location-taxa-year level) in the analysis data. 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).

Advantages of BioTIME in this study BioTIME has a number of features making it useful for our analysis. First, existing alternative datasets[7] provide only cross-sectional or limited taxonomic data, hindering broad-scale economy-ecosystem studies. BioTIME aggregates longitudinal data from hundreds of ecological studies on species counts or biomass, enabling the construction of metrics for ecosystem changes. Second, the studies included in the

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[7] A detailed comparison of BioTIME and alternative biodiversity datasets is in the Data Appendix.

BioTIME 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). Third, 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 in sampling protocols with location fixed effects. Fourth, 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 believe the data let us gain by far the best understanding of which kinds of organisms and biomes are affected by economic activities.

Steps on Data Processing 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 activities. 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.[8] Fourth, we exclude studies that report only total species biomass or only species presence indicators, to ensure a unified measure of abundance. These studies account for less than 1 percent of the abundance data. We obtain virtually identical results if including these samples in richness and similarity analyses. 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 Section 3, we report sensitivity checks 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. This idea originated not within ecology but from population

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[8] 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.

dynamics studies, including Malthus's work on population growth (1798) and Charles Elton's foundational contributions to animal ecology (1927). Abundance simply measures the pure quantity of individuals observed and is agnostic about the types of species in the sample. It is worth noting that virtually all studies examine closely related species that fall in the same taxon group. Therefore, abundance is never measured by combining distant species, such as birds and fish.

Richness is the total number of unique species present of a particular taxon (e.g., birds) that characterize a particular biological community, habitat, or ecosystem type (Colwell et al., 2009). 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.

The first two measures, abundance and species richness, are known as α diversity (Fisher et al. (1943)), and they form the foundation for many other biodiversity indices, such as the Shannon Index. The practice of quantifying species richness traces back to early naturalists such as Charles Darwin and Alfred Russel Wallace, who emphasized the importance of understanding the diversity of life across regions. By the 20th century, as ecology matured into a formal discipline, species richness had become a central concept, frequently used to compare ecosystems and assess the impact of environmental changes.

Jaccard similarity is an inverse measure of the amount of year-to-year species turnover in a given taxon, at a given location, in a given year; thus, it provides a measure of compositional changes. Similarity indices, such as the Jaccard and Sorensen indices, are developed to estimate β diversity that measures the variation in species diversity between habitats or spatial scales.[9] Let $\mathbb{S}_{cjt}$ 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);

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[9] The Jaccard Similarity Index was introduced to ecology by Paul Jaccard in the early 20th century, although Grove Karl Gilbert had independently developed a similar index earlier in a different context (Jaccard, 1912).

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.

Different scenarios may yield varying effects on these three biodiversity measures. Consider two examples. In the first case, if fewer migratory birds arrive at a breeding habitat this year compared to the previous year, we would expect a decrease in abundance. Species richness might also decline if fewer species arrive, though it could remain stable or even increase if invasive species are introduced. The change in similarity would likely be negative, reflecting greater differences in species composition across the two years, although it could remain unchanged if the same species are present. In the second case, consider the introduction of an invasive species, such as brown trout outcompeting native humpback chub in the Glen Canyon National Recreation Area.[10] This introduction could increase species richness but decrease similarity, and its impact on abundance would depend on whether the invasive species thrives or native species decline more sharply.

We highlight three points before proceeding. First, we use log abundance and log species richness as outcomes in econometric analysis. 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 the sake of brevity, we will henceforth refer to all three metrics together as biodiversity, even though they are 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 indicative of deteriorating biodiversity outcomes. Our reasoning is that declines in these measures indicate reductions in populations and diversity, and increases in the instability of species 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 findings tend to be more pronounced 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."

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[10] See https://home.nps.gov/glca/planyourvisit/brown-trout-harvest.htm for more information and a National Park Service program to manage the non-native brown trout.

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 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.

The upper panel of Figure 3 presents trends in three biodiversity metrics, following the framework developed by the BioTIME team (Dornelas et al., 2014). We restrict the sample to U.S.-based, non-marine studies from 1960 onward. All three metrics exhibit negative overall trends (black lines). The lower panel of Figure 3 shows the annual average percentage change in abundance by state, revealing widespread declines, with only a few states showing slight increases. Comparing these results with the study distribution in the upper panel of Figure 1 indicates that states with increasing trends tend to have sparse data, while the more robustly sampled states predominantly show declines.

2.3 Potential Sources of Bias

Some discussions on the features and limitations of the data are in order. In an "ideal" scenario, we would randomly select a set of locations across the country and continuously monitor the categories and quantities of all observed animal and plant species over time. The monitoring technology adopted at each location would remain constant to ensure that variations in species observations reflect genuine changes in the underlying biodiversity conditions rather than changes in the monitoring procedure. The BioTIME dataset departs from this ideal as it comprises observations for specific locations, years, and species that were the focus of ecological studies; though by construction, BioTIME only includes studies that adopted fixed sampling protocols, many of these studies extend over several decades, and as such, sampling technologies may have undergone changes over time.[11] 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 activities within the same sampling site, and therefore our conclusions are not biased by factors that are correlated with permanent differences in economic activities 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

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[11] 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.

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.[12] Alternatively, one might worry that a large economic 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 3.3.

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.[13] 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 activity 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 (Gonzalez et al., 2016; Cardinale et al., 2018). 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

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[12] 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 4, Panel A shows some evidence that the effect of GDP is roughly linear for all three biodiversity outcomes.

[13] In fact, as we will further explain in Section 3.1, 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).

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 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 3.3, we describe tests that check 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.

We recognize the study's limitations in addressing observability issues associated with BioTIME's sampling methods. Different methods are affected to varying extents. For instance, the North American Breeding Bird Survey (BBS) uses *point counts*, which can be influenced by environmental changes like land use alterations. Without data on these conditions during sampling, we cannot control for these factors. This limitation means that if higher GDP leads to land use changes that enhance bird visibility in the BBS, we may underestimate GDP's negative effect on biodiversity. Similar methods prone to observability issues include *acoustic monitoring* (bats). In contrast, the Sevilleta Long-Term Ecological Research (LTER) study uses *trap webs* at fixed stations to capture small mammals, making it less susceptible to observability issues related to road or open-space visibility. Other methods less affected by observability issues include *infrared cameras* (mammals), *sweep nets* (insect pollinators), *quadrats* (plants), and *conical nets* (zooplankton). In most of our estimation results, we present both full sample analyses and subsamples excluding birds, with similar patterns observed across both. Thus, while acknowledging that sampling methods could result in biased counts, most studies included in BioTIME do not suffer significantly from observability issues.

2.4 Other 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 activities 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 spillovers, 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 the Appendix we report robustness results for both of our correlational and causational analyses using alternative definitions of economic activity, such as state total GDP (without the normalization by population), or county total or per capital *income* measured at the county level instead of the state level. We use county income instead of county GDP because the latter is only available after 2001 which covers 30% of our sample. We do report a robustness check within this subsample for the sake of transparency.

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. 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. The federal prime contracting data identify locations where the majority of work was performed, so that one can attribute the economic activity 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 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 8 Panel B plots the location of nonattainment areas as of year 2019.[14]

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). We create an annual, longitudinal measure of 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

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[14] 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.

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 The Biodiversity-Economic Activity Association

3.1 Estimation Framework and Baseline Findings

We begin with a panel fixed effects regression model to estimate the within-location association between biodiversity outcomes and economic activity. 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 likely 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. The average sampling location has data for 13 years (standard deviation = 10 years).

Baseline Findings Figure 4, 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).[15] 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.

It is inherently difficult for us to directly 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 2.1 – 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 activities growth during the time period, we estimate equation (1) focusing on bird-related observations. This estimation yields an estimated coefficient of -0.475 log units (representing a decline in the bird population) per 1% increase in GDP per capita. Multiplying this coefficient by the average change in logged real GDP per capita between 1990-2015 suggests a decline of 0.80 billion birds, which is 80% of the estimate in Rosenberg et al. (2019).

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[15] The Jaccard similarity index captures the turnover rate of species in a location between two consecutive years. It is constructed to range between 0 and 1, whereas the abundance and richness outcomes are measured on a log scale. Our IV estimates suggest that a log increase in GDP reduces the Jaccard similarity index by 0.54 percentage points out of a mean index value of 0.43. This is, in fact, of the same order of magnitude relative to the effect sizes observed for abundance and richness.

3.2 Features of the Biodiversity-Economic Activity Association

Effects by Taxa Panel B of Figure 4 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.

Environmental Kuznets Relationship Panel C of Figure 4 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.

Distributional Heterogeneity Panel D of Figure 4 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 4). 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.

Sector-Specific Output 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 services.

Our results show large and consistent negative associations between manufacturing income – indicative of greater manufacturing activities – 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.[16] This evidence suggests that industrial emission as a byproduct of economic activities may be an important driver of biodiversity trends. We take a more detailed look at the role of pollution in Section 4.2.

Dynamics 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.4 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.2, 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) 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.[17]

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[16] In Appendix Table A.1 and Figure A.3, we further examine the source of the positive correlation between agricultural income and biodiversity. A plausible explanation is that increased agricultural output may be associated with afforestation, agroforestry, or shifts toward biodiversity-friendly farming practices that enhance plant diversity. Agricultural landscapes may also support a variety of plant species through mechanisms such as habitat heterogeneity, fallow land rotation, and crop diversification. Empirically, we show that: (1) the positive ag-biodiversity correlation is mainly driven by increases in income in the crop and animal farming subsectors, and terrestrial plants drive the positive effect, while other taxa exhibit negative responses to agricultural income, and (2) using USDA data, that a state's receipt of federal government direct payments for conservation programs – including the Conservation Reserve Program, Agricultural Conservation Easement Program, Environmental Quality Incentives Program, Conservation Stewardship Program, Regional Conservation Partnership Program, and Conservation Technical Assistance – has a strong positive correlation with biodiversity outcomes. We find no positive agriculture-biodiversity correlation once conditioning on the conservation spending.

[17] 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

Appendix Figure A.5 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.3, 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.

We additionally explore alternative specifications for longer-term dynamics, including a long-difference model and data aggregation into 5- or 10-year periods. Given the variation in study durations and coverage years, we compare long-difference results across two samples in Table A.4. Panel A focuses on studies over ten years, calculating the average biodiversity change from start to end. The estimated coefficients of GDP-biodiversity are consistent with our baseline specification, except for the similarity index. The Jaccard similarity index quantifies the turnover rate of species between two consecutive years, hence it might not adequately capture biodiversity changes over extended periods. Panel B includes all studies, the estimated coefficients also align with the baseline specification. In Panel C and D, we aggregate the data into 5- or 10-year periods. Our findings indicate that while aggregating the data diminishes the precision of the estimated effects, the magnitudes remain comparable to those in the baseline.

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

3.3 Sensitivity Checks

We describe robustness checks that correspond to concerns we laid out in Section 2.3.

Endogenous Sampling to Economic Conditions Appendix Table A.5 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. Simply put, we find 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. Appendix Table A.6 examines the impact of outlier observations. We find that winsorizing 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.

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 reported to be 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. Appendix Table A.7 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.[18] 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.

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 (Colwell et al., 2009).[19] 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

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[18] This finding could provide insights into the significance of conducting long-term ecosystem monitoring to enhance the dependability of monitoring outcomes. We are grateful to an anonymous reviewer who pointed us to this important dimension of the ongoing discussion on the national accounting and measurement of biological resources.

[19] The Shannon Index and the Simpson Index (1 minus Gini) were originally developed in the fields of communication theory (Shannon (1948)) and cryptanalysis (Good (1982)). Their significance for ecological research was recognized later, with key contributions from MacArthur (1955) and Simpson (1949), respectively.

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.

Appendix Table A.8 shows that our species richness findings are similar using Gini Index and Shannon Index. 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.[20]

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, if individual animals move to a nearby location in response to opening of a new factory. This could be particularly relevant for more mobile species such as birds. There are two potential downsides of using a state level output measure. First, using state level GDP foregoes more localized economic variation that could matter for biodiversity outcomes. Impacts of economic activities may also exhibit spatial heterogeneity: for example, it is possible that the ecological damage of production differs across different ecosystems within the same state. Second, suppose species in a local

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[20] The Sorensen Index was created later by botanists Thorvald Sorensen and Lee Raymond Dice in the mid-1900s (Sorensen (1948), Dice (1945)). 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.

ecosystem (smaller than the size of a state) were already in a form of "equilibrium," all the available spaces (or "niches") are filled up with different species, and they spread out until they cannot anymore because of lack of resources or because other species are competing with them for the same space. In this equilibrium, a disturbance in a given niche will not, in the short run, lead to strong spatial displacement where species fill up that vacuum.

Appendix Table A.9 compares baseline results (panel I) with those if we instead use county level *income*(panel II). Comparing results in panels I and II, we get smaller OLS estimates, especially with bird species; our 2SLS results using the military buildups shock quasi-experiment renders similar results whether we use state or county measures. We view these patterns as broadly consistent with errors in local-scale output measurement for mobile species (e.g., attenuation bias when associating the exposure of bird species to economic activities in the very county where they are observed).

Appendix Table A.10 and Table A.11 illustrates how effect sizes vary with levels of output measurements and data aggregation. First, in Table A.10, instead of measuring output at the state or county level, we use the concept of *eco-region* developed by the U.S. Environmental Protection Agency, which divides the contiguous U.S. into 85 mutually exclusive areas where ecosystems are similar. Appendix Figure A.6 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.

Second, 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 in Appendix Table A.11. Unlike the previous table, in this table, both biodiversity outcomes and economic activities are aggregated into spatial 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. For these non-standard geographic delineations that do not obey state borders, only OLS results are reported as the military shocks used for the 2SLS analysis are measured at the state level.

4 Causality and Mechanisms

The negative association between biodiversity and economic activity may be subject to 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 examine how biodiversity outcomes react to U.S. national military expansions that are known to produce plausibly exogenous shocks to the local production economy. We then present a framework to identify the pollution channel using variation in local pollution driven by transported pollution from distant, upwind cities, which cause reductions in local biodiversity outcomes.

4.1 National Military Buildups Shocks

We 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 5 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:

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

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 5 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. In assessing the validity of the instrumental variable analysis, we conducted several tests to evaluate the plausibility of the identifying assumptions following Goldsmith-Pinkham et al. (2020) in the Appendix.

Figure 5 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 4, 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 3, 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).[21] 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.[22]

Appendix Table A.12 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 per capita GDP with state total GDP, county total income, or county per capita income as the right hand side measure of economic activities. For succinctness, we only report biodiversity-GDP elasticity estimates for these robustness checks. We tend to obtain smaller, though more precise coefficient estimates when using total rather than per capita measures of economic activities. Overall, the results are robust to these specification changes.

A Note on Causal Interpretations We highlight three takeaway messages from this analysis. First, biodiversity outcomes respond to shocks that are well-known to cause changes in local economic activities. Because military shocks generate plausibly exogenous variation in economic activities, the findings improve upon the correlational evidence in Section 3.

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[21] 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. To address the possibility of weak instruments in the full sample, we use limited information maximum likelihood (LIML) estimator which is median-unbiased with weak instruments in lieu of 2SLS. Results shown in Appendix Table A.12 demonstrates that the LIML results are comparable to those from 2SLS.

[22] The one-step 2SLS method employs cluster-robust standard errors at the state level, which aligns with the variation of the instrument. We also implement a two-step approach to explicitly separate the first and second stages and use cluster bootstrapping to compute standard errors. The two-step approach produces very similar estimates (Appendix Table A.13) with the standard one-step 2SLS method. The one-step method additionally allows for taxa fixed effects, which is important given potential differences across taxa, even within the same location or state.

Second, the implied biodiversity-GDP elasticities from the quasi-experiment are larger than their OLS counterparts. By leveraging shocks in a 2SLS setting, the quasi-experiment-based elasticity estimates alleviate classic measurement error and endogeneity problems.

Third, we do not interpret the biodiversity-GDP elasticities as the causal effect of a *ceteris paribus* increase in economic activities. GDP is an accounting concept and only varies because of changes in real economic activities. Therefore, 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 or habitat loss from changing land use – that vary with economic activities. 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 4.2. In particular, we focus on the causal effect from air pollution, which we show can be isolated out from other contributors to biodiversity changes.

Heterogeneous Effects We find that the negative impact of military spending shocks on biodiversity is most pronounced in locations with military bases. Military expenditures may be subject to exemptions from environmental laws, with broader exceptions expected at military bases. For instance, the 107th Congress granted an exemption from the Migratory Bird Treaty Act, and the 108th Congress exempted certain provisions of the Marine Mammal Protection Act and parts of the Endangered Species Act (Bearden (2007)). Additionally, specific engines and equipment related to national defense may be exempt from requirements under the Clean Air Act and Clean Water Act (CRS-Report (2002)). These exemptions from environmental regulations potentially exacerbate biodiversity loss.

We examine the heterogeneous effects of military spending shocks on biodiversity across sampling sites, distinguishing between locations in counties with at least one military base (30% of the sample) and those without any military bases. The Military Bases dataset is sourced from the Office of the Assistant Secretary of Defense (OASD) and is part of the US Department of Transportation (USDOT)/Bureau of Transportation Statistics (BTS) National Transportation Atlas Database (NTAD). The dataset contains the locations of Department of Defense (DoD) sites, including installations and training areas.

Figure A.7 below indicates a more pronounced negative impact on biodiversity in proximity to military bases, consistent with exemptions from environmental regulations obtained by various activities on these bases. Nonetheless, we also detect a smaller but significant effect in areas without military bases. To further probe this heterogeneity, we interact GDP with (i) an indicator for base presence and (ii) the share of county land occupied by military bases. Results in Table A.14 show that base presence slightly amplifies the biodiversity loss from GDP (Column 2), while greater land coverage by military bases intensifies the effect (Column 3).

4.2 The Pollution Channel

Our analysis proceeds as follows. We first present new evidence that air pollution – an important byproduct of economic activities that has well-understood impacts on human health – is also a broad driver of biodiversity losses. Next, we use the estimated relationship between biodiversity and pollution to estimate the share of the effect from military shocks in Section 4.1 that is due to pollution externalities.

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 county's pollution as the explanatory variable of interest. The first row of Table 4 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. When examining the association across different taxa, Figure A.8 indicates that mammals, terrestrial invertebrates, and terrestrial plants are most affected by air pollution, while birds and freshwater plants are also significantly affected, though to a lesser extent. Amphibians, fish, and freshwater invertebrates exhibit relatively smaller impacts.

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.[23]

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:

$$IV_t = \frac{1}{|C|} \sum_{c \in C} max\{0, cos(\phi_{ct})\} \cdot Pollution_{ct} \cdot \frac{1/distance_c}{1/\sum_{c \in C}(1/distance_c)} \tag{3}$$

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".[24] 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

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[23] In the Appendix, we compare the performance of our IV with that of Deryugina et al. (2019). Replicating the approach used in Deryugina et al. (2019), we find that our main findings remain consistent, but the reduced precision reinforces our preference for the LASSO instrument as the primary identification strategy.

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

selection using LASSO:

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

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 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.[25] 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 4 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.[26] Comparing these estimates to the associational GDP elasticities in Figure 4, the pollution elasticities are about half the size.[27]

Our estimates of the pollution channel likely capture both short-term shocks and longer-term transmission effects. Conceptually, biodiversity is affected by acute pollution events—via

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[25] To assess the robustness of our LASSO-based selection of upwind counties, we compare the characteristics of the destination county with those of both LASSO-selected upwind counties and unselected counties. This analysis demonstrates that our algorithm does not systematically select counties with similar attributes (e.g., industry sectoral income, land use patterns, pollution-generating activities, or population levels). Details are included in the Appendix.

[26] 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.

[27] In Appendix Table A.15, we add weather controls, including county annual average maximum and minimum temperature, and precipitation into the 2SLS regressions under our original setting. The estimates changed little with the augmented specification.

toxicity, habitat degradation, or behavioral responses—as well as by cumulative exposure over time. Given the annual frequency of our data, we cannot separate these timescales empirically, and we are not aware of studies that do so. Nonetheless, physiological evidence suggests multiple channels. For example, birds inhale more airborne particles than humans at equivalent concentrations due to higher respiratory rates and constant exposure. Fine particulates can penetrate deep into their lungs, affecting cardiovascular and respiratory function. Medium-term exposure to traffic-related pollutants such as PAHs has been linked to reduced reproduction and impaired development (Albers, 2006). Long-term exposure has been associated with heritable DNA mutations in birds, with potential cancer risks (King et al., 2014). Indirect effects also arise through food chain disruptions, such as eutrophication from NO_x, which depletes aquatic prey populations.

Overall vs. Pollution Effects With the causal biodiversity-pollution elasticity estimates from Table 4, we now revisit our military spending quasi-experiment and calculate how much of the effect was due to induced changes in pollution, using a framework similar to Heckman et al. (2013). We decompose the total marginal effect of military spending policy on biodiversity into different components and compute the one component that reflect the partial effect of the policy through pollution:

$$E(\triangle \text{biodiversity}) = \triangle \tau + \alpha^k \cdot E(\triangle \text{pollution}),$$

where $E(\triangle \text{pollution}) = \beta^k \cdot \triangle \text{military spending}$ is the change in air pollution caused by military spending shocks. Here, $\alpha^k \cdot E(\triangle \text{pollution})$ is the effect of miliary spending shocks on biodiversity through the air pollution channel, k. The term $\triangle \tau = \sum_{j \in J, j \neq k} \gamma^j \cdot \triangle \text{military spending}$ include all unmeasured effects of military spending on biodiversity due to other channels – indexed by j – such as habitat change, water pollution, etc. Therefore, we have

$$E(\triangle \text{biodiversity}) = \sum_{j \in J, j \neq k} \gamma^j \cdot \triangle \text{military spending} + \alpha^k \cdot \beta^k \cdot \triangle \text{military spending}.$$

The reduced form effect of military shock on biodiversity can thus be written as:

$$\begin{aligned}\frac{\partial \text{biodiversity}}{\partial \text{military spending}} =& \frac{\partial \text{biodiversity}}{\partial \text{pollution}} \cdot \frac{\partial \text{pollution}}{\partial \text{military spending}} + \\ & \frac{\partial \text{biodiversity}}{\partial \text{habitat}} \cdot \frac{\partial \text{habitat}}{\partial \text{military spending}} + \\ & \frac{\partial \text{biodiversity}}{\partial \text{water}} \cdot \frac{\partial \text{water}}{\partial \text{military spending}} + ... \\ =& \alpha^k \cdot \beta^k + \sum_{j \in J, j \neq k} \gamma^j\end{aligned}$$

We recover the effect of military spending on biodiversity through the air pollution channel — $\alpha^k \cdot \beta^k$ — in two steps. First, our reduced form estimates of the effect of the military spending in Section 4.1 provide the total marginal effect of the policy on biodiversity: $\frac{\partial \text{biodiversity}}{\partial \text{military spending}} = \alpha^k \cdot \beta^k + \sum_{j \in J, j \neq k} \gamma^j$. Second, we compare the total effect to the partial effect of the policy through pollution: $\frac{\partial \text{biodiversity}}{\partial \text{pollution}} \cdot \frac{\partial \text{pollution}}{\partial \text{military spending}} = \alpha^k \cdot \beta^k$. The first term in the expression is the biodiversity-pollution elasticity (α^k) estimated in Table 4. The second term – the effect of the military spending shocks on air pollution (β^k) – is straightforward to estimate from the data, which we report in Appendix Figure A.9. As expected, military spending increases pollution.

Figure 7 presents the decomposition. The gray bars show the total, reduced-form effect of military shocks on biodiversity, and the blue bars show the effect through changes in air pollution. 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 evidence thus suggests air pollution is a first-order pathway for how military spending affects biodiversity but is not the only pathway. Military buildups are a general boost to local economic activities and may affect biodiversity through other channels such as land use change. However, there are potential 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, by using a remote-sensing measure of pollution that is based on particulate pollution detection, we are only estimating the impacts of particulate matter pollution. Although particulate matter is often used as a proxy for overall air quality, it is not a perfect proxy for all air pollutants.

4.3 Land Use

Our current investigation primarily centers on the impact of air pollution, while other contributing mechanisms are harder to establish causally. Here we examine an additional channel that is frequently studied in the literature – land use changes – and present correlational evidence to support our findings.

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.

We 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 2.4) 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.[28] 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.16 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.

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

5 Environmental Regulations

The evidence on the role of air pollution and land use in the biodiversity-economic activity link raises the possibility that environmental regulations may provide policy maker another lever for ecosystem conservation. In this section, we empirically test the impact of several landmark air pollution and land-use regulations in the BioTIME dataset.

5.1 Pollution Regulations

The Clean Air Act (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.[29] 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 8 Panel A shows the fraction of sampling locations in the BioTIME data that were in nonattainment areas with respect to each of the standards.[30] Figure 8 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 the environmental benefits of the nonattainment

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[29] 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).

[30] 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 8 Panel B, the designation of nonattainment status with respect to the 1997 $PM_{2.5}$ standard did not occur until the year 2005.

regulations. A nonattainment designation has 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).

We repeat the same analysis as we did for military spending in Section 4.1, but instead with the number of nonattainment designations as our policy variable. Figure 8 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 activities, increase biodiversity.

Appendix Table A.17 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 8 Panel C. As before, we find that the effects are larger for abundance and richness than and for similarity.

The implied GDP-biodiversity elasticities from the CAA shocks are in line with those estimated in Section 4.1. 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 earlier findings, we find somewhat larger GDP elasticities for non-bird species. Appendix Figure A.11 further shows that the pollution decomposition exercise also gives similar results: about 20-60% of the reduced form effects of the CAA shocks came through the causal effect of air pollution. The similarity in effect size estimates between the two sets of quasi-experiments adds confidence to our research design.

5.2 Land Use Policies

Given our earlier findings on the negative association of urbanization and biodiversity outcomes, it is natural to ask whether land protection policies can help mitigate the adverse effects of production shocks. Since early 1990s, adoption of conservation protected area policies has grown rapidly (Frank and Schlenker, 2016).[31] 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 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. Figure A.12 plots the location of these protected areas in the US. 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. 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.

Lacking a quasi-experimental design, we report a correlational exercise in Table A.18, which reports how protected areas modulate the panel correlation between GDP and biodiversity outcomes. Columns 1 and 2 show that the abundance-GDP relationship is attenuated by an increase in the amount of nearby land that is protected.[32] 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

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[31] 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.

[32] 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).

turnover (columns 5 and 6). In general, there is suggestive evidence that larger coverage of contiguous protected areas reduces the negative impact of economic activity 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.

6 Conclusion

This paper analyzes a compilation of studies that maintain longitudinal ecological observations spanning the last five decades and provides new insights on the environmental effects of economic activity. First, the impacts of externalities extend well beyond affecting just human health. Greater economic activity is broadly associated with reduced ecosystem diversity, including metrics that capture the number of individuals (abundance), the diversity of species (richness), and the intertemporal stability of the composition of species (similarity). This association is widespread across terrestrial, avian, and aquatic groups, and is particularly strong when biodiversity conditions are already poor. Second, economic activity causes biodiversity changes. We directly identify this causal impact by examining changes in local economic activity resulting from national military buildups, which are largely driven by plausibly exogeneous geo-political events. Third, air pollution externalities are a first-order channel. Using an instrumental variables strategy that isolates exogenous local changes in pollution caused by pollution transported from elsewhere, we find that air pollution negatively impacts biodiversity and serves as a key channel through which military buildups affect ecosystems. Regulations that protect the environment – even though typically designed to safeguard human health – provided substantial co-benefits to ecosystem conservation. This finding is highly relevant for policymaking, as species extinctions and ecological degradation are accelerating to a degree unprecedented in human history.

By explicitly examining the link between GDP and measures of ecosystem diversity derived from past ecological observations, this research helps shed light on the importance of broadly-scaled and long-term monitoring of the ecosystem, and how monitoring results can be utilized to quantify the interconnection between the environment and the economy.

Our findings suggest several future promising lines of research. First, we find that air pollution is one mechanism linking economic activity to biodiversity. However, there are other consequences of economic activity – such as noise and water pollution – that may have adverse effects on biodiversity but for which there are little or no causal studies. Second, our paper does not quantify the economic value of the estimated changes in biodiversity.

For example, biodiversity has non-market value (e.g. Loomis and White, 1996; Kolstoe and Cameron, 2017), and the existence of keystone species has been shown to be important for human health (Frank and Sudarshan, 2024). Future work estimating the costs of biodiversity loss will be valuable, especially understanding the costs of declines in particular species that are critical for healthy ecosystem function. For example, non-market valuation methods can help us understand the benefits park-goers receive from seeing rare or famous species, and quasi-experimental variation in biodiversity may help us understand its value as a productive input into agricultural production.

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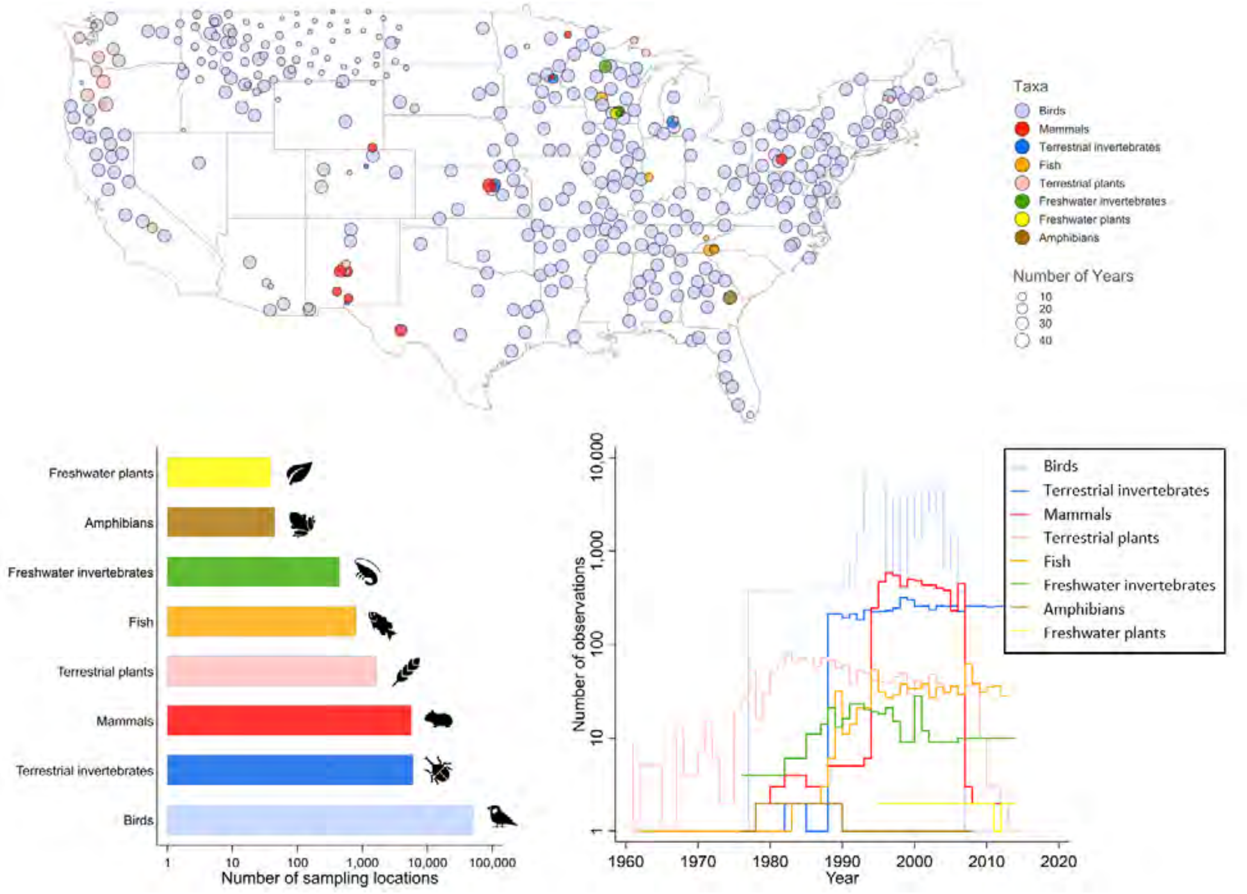
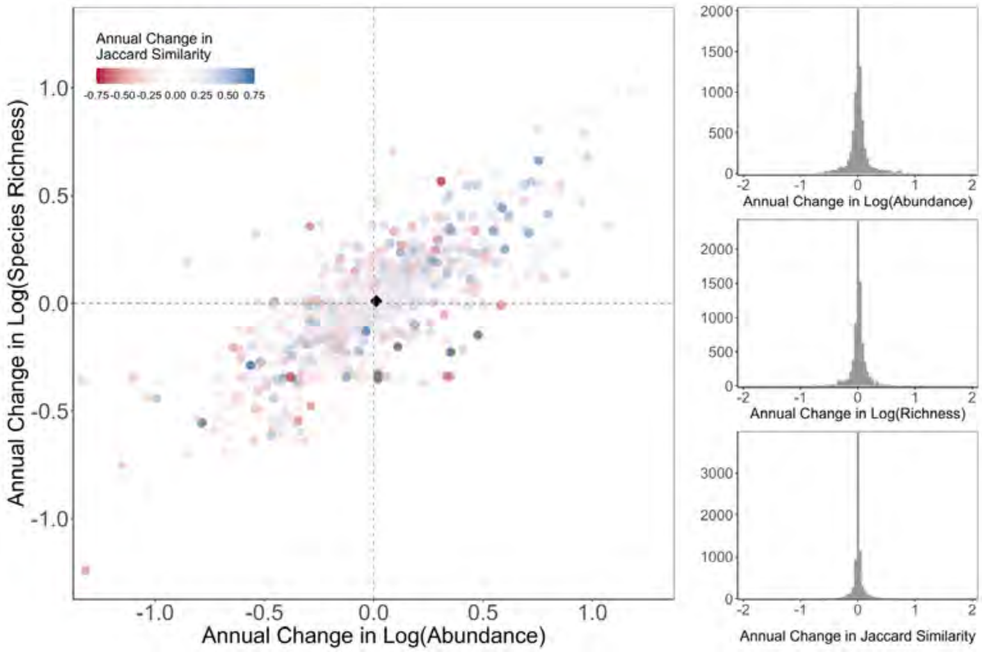
7 Figures and Tables

Figure 1: Sampling Locations and Observations



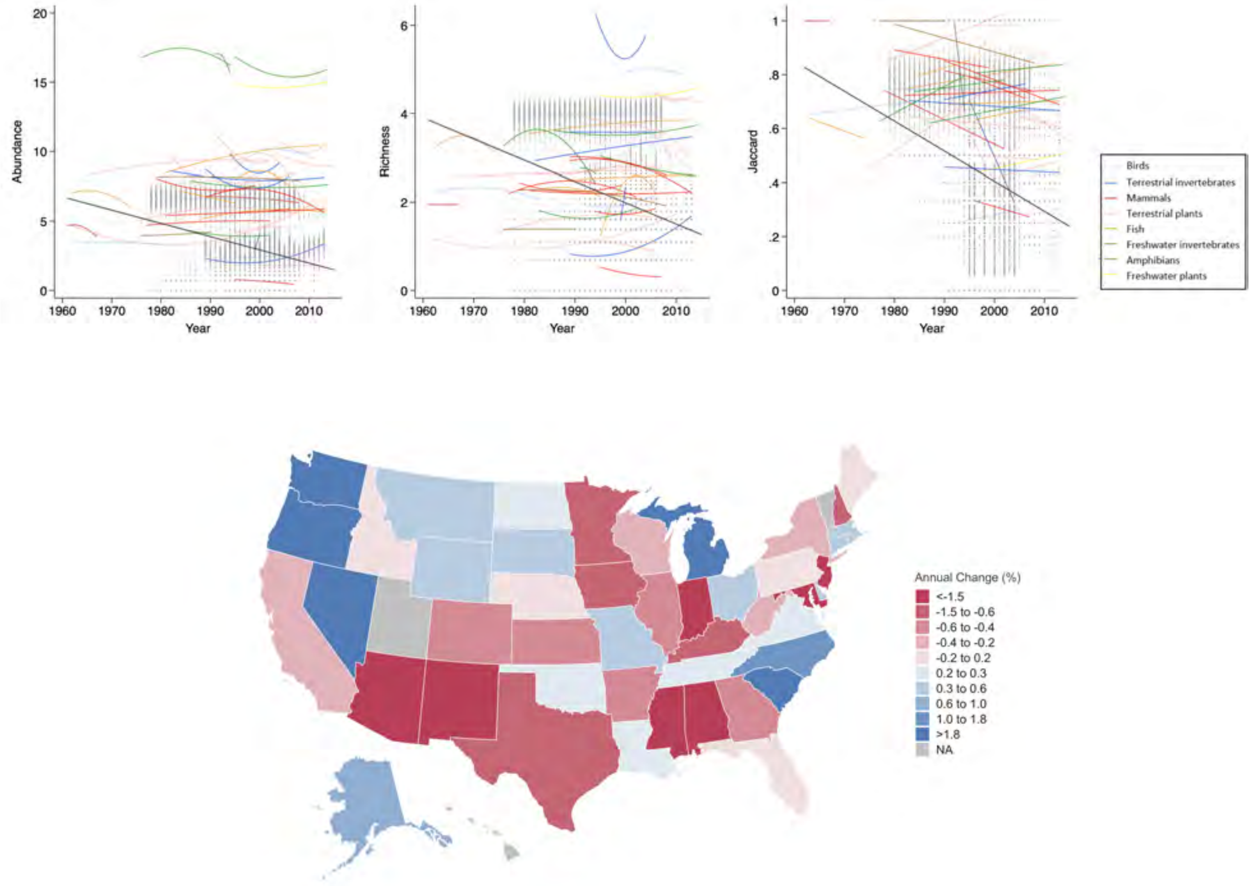
Notes: The upper panel plots sampling locations that are included in our main estimation sample. Location points are distinguished by taxa and are aggregated to a 95-by-95 km hexagon resolution to increase readability. The lower left panel reports number of sampling locations. The lower right panel reports 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.

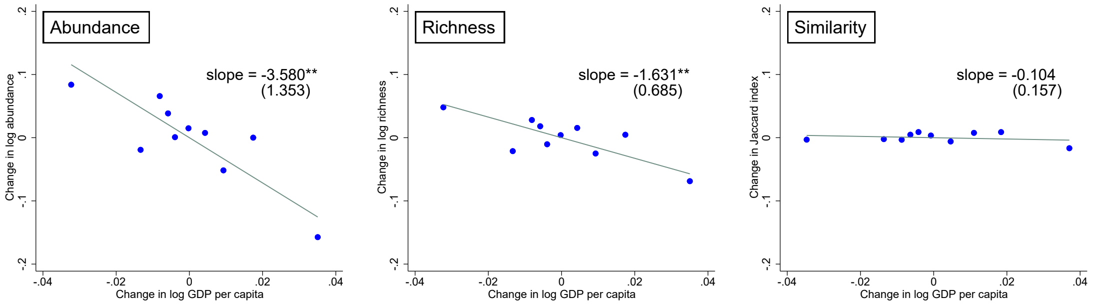
Figure 3: Temporal Changes in Biodiversity



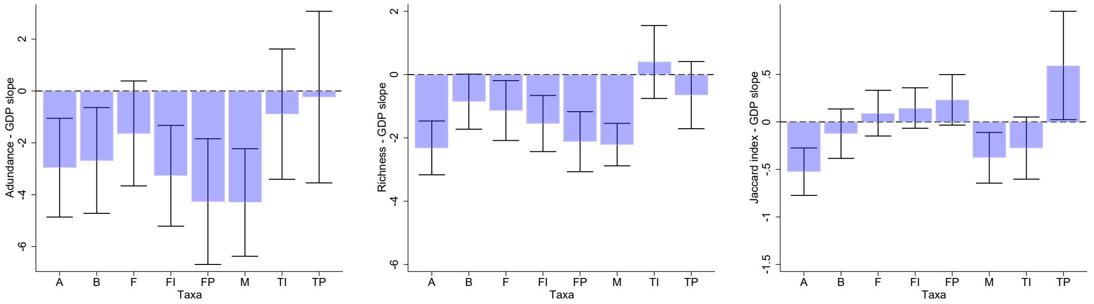
Notes: The three figure in the upper panel show temporal changes in abundance, richness and Jaccard similarity. Data points for all studies are represented by gray circles, and models fitted by solid lines that suggest the trend in time series separately for each study. The thick black line in each figure shows the overall trend in the corresponding biodiversity metrics estimated using all studies combined. The color of the fitted trend lines shows the taxa of the study. Color coding is the same as Figure 1. The map in the bottom panel shows the average annual percentage change in abundance across states during the sampling years. Specifically, we use the following equation: $Y_{cjt} = \sum_{s=1}^{50} \beta_s \cdot 1[State_c = s] \cdot T_t + \eta_{cj} + \epsilon_{cjt}$, where Y_{cjt} represents the log of abundance. $1[State_c = s]$ denotes indicator variables for sampling locations c in state s. T_t indicates a linear trend for the sample years from 1961 to 2015. Location-by-taxa fixed effects η_{cj} are included to account for differences across sampling locations and taxa. Thus, β_s represents the annual average change in abundance for each state s. The state-specific coefficients, displayed in the map below using 10 quantiles, indicate that most states experienced a decline in biodiversity over the years (shaded in red), while only a few states show slight increases (shaded in blue).

Figure 4: The Biodiversity - GDP Association

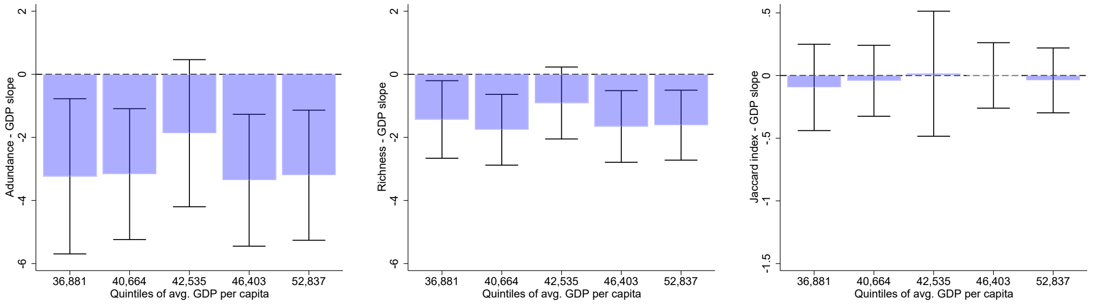
(a) Average association



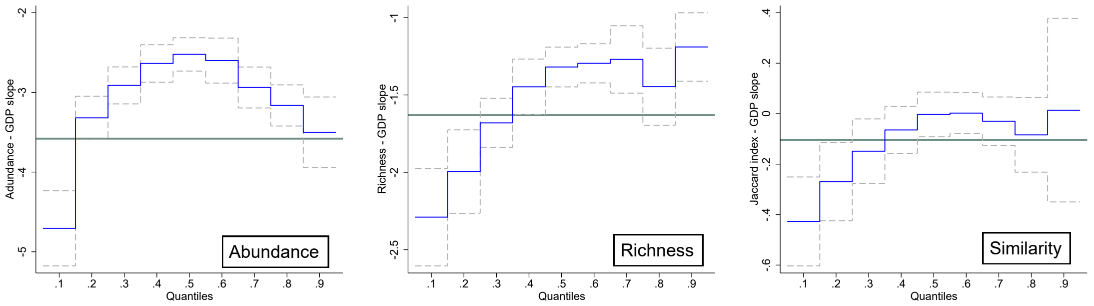
(b) By taxa



(c) By sample-average GDP per capita



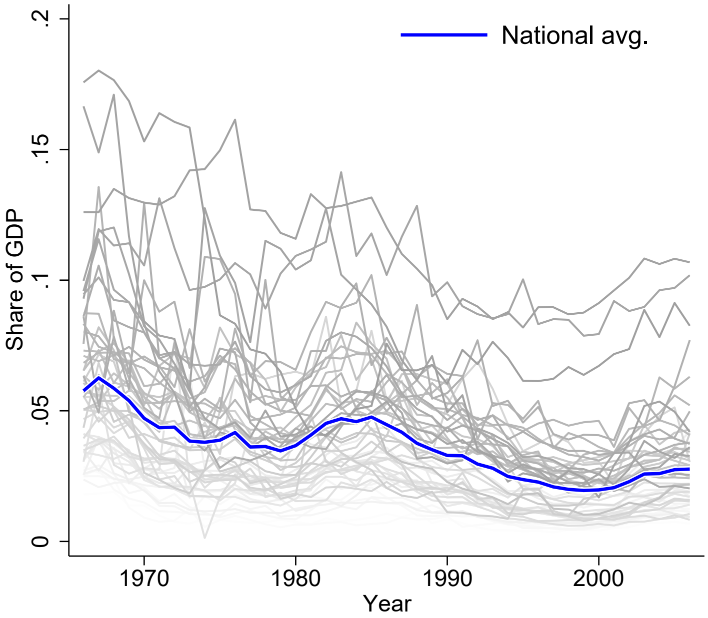
(d) By regression quantiles



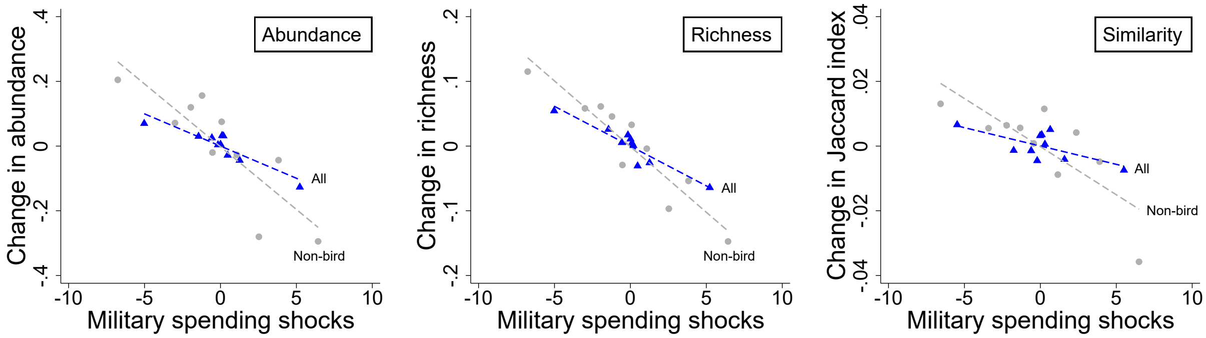
Notes: Panel (a) plots decile bin scatterplots of biodiversity and log GDP, both residualized with location-by-taxa and year fixed effects. The slope of the fitted line represents the OLS estimate $\hat{\beta}$ of equation (1). Numbers in parentheses show standard errors clustered at the state level. 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), mammal (M), terrestrial invertebrates (TI), and terrestrial plants (TP). Specifically, the regression equation in Panel (b) is $Y_{cjt} = \beta \cdot \log GDP_{st} \times I_j + \eta_{cj} + \eta_t + \varepsilon_{cjt}$, where I_j is an indicator variable for taxon j. Panel (c) replaces I_j to a group indicator of quintiles of sample-average GDP per capita. The quantile regression equation in Panel (d) is $Y_{q,cjt} = \beta \cdot \log GDP_{q,st} + \eta_{q,cj} + \eta_{q,t} + \varepsilon_{q,cjt}$ for q in 1st to 10th quantiles.

Figure 5: Military Buildups and Biodiversity Outcomes

(a) Military contract spending as a share of state GDP

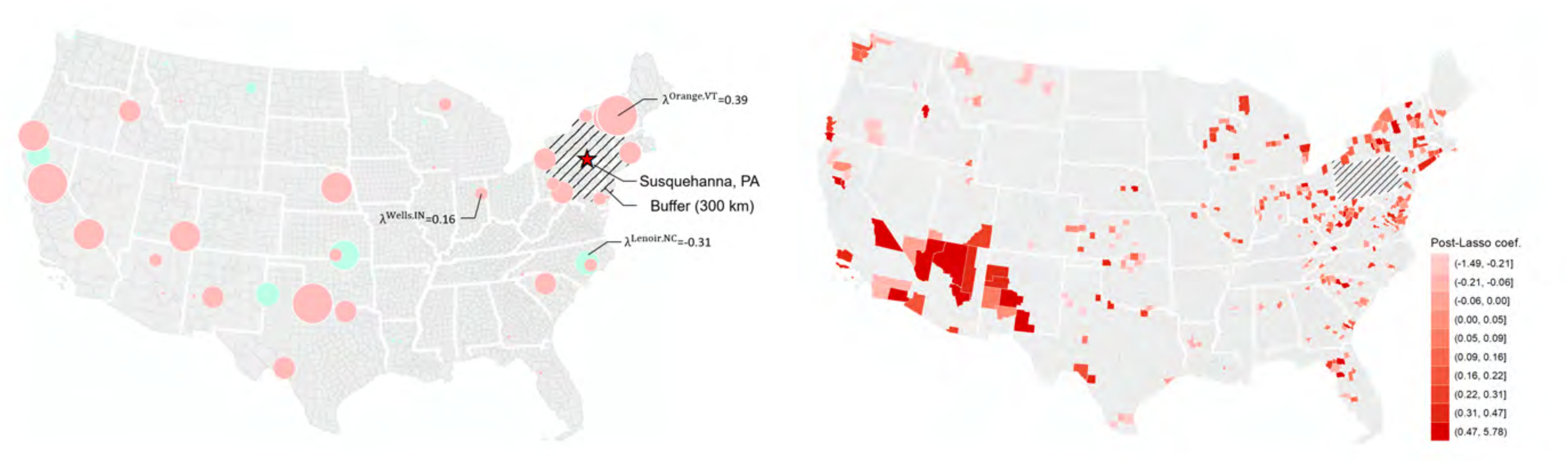


(b) Biodiversity and military spending shocks



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

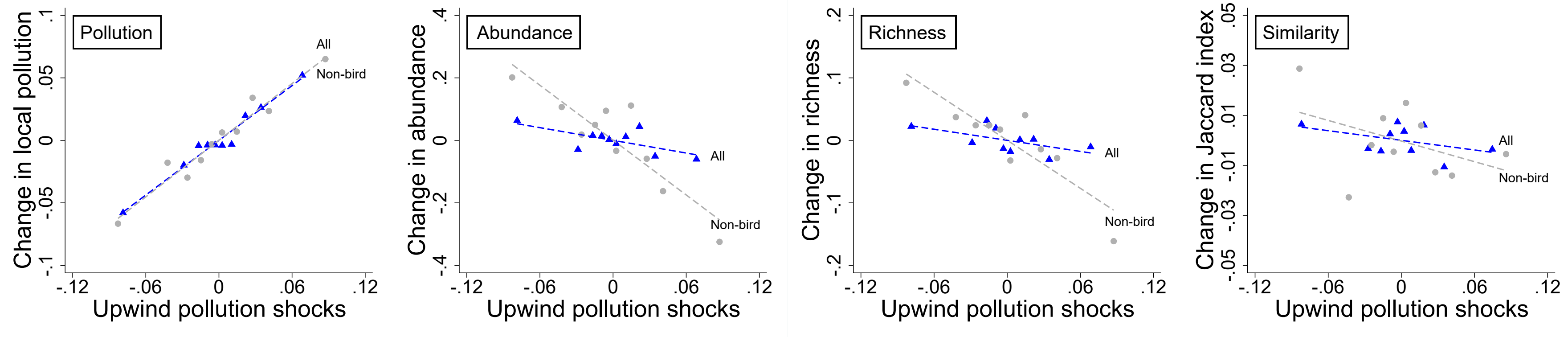
Figure 6: Instrumental Variables Estimation of the Effect of Air Pollution on Biodiversity



(a) Upwind pollution counties for Susquehanna, PA

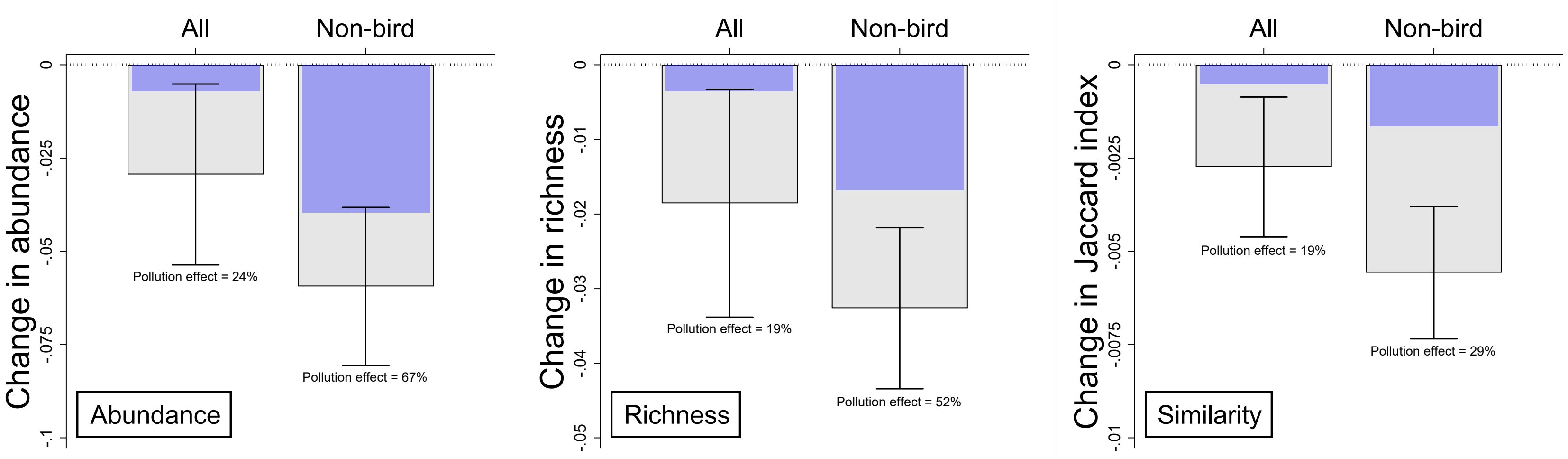
(b) Upwind pollution counties for all counties in PA

(c) Upwind pollution shocks, local pollution, and biodiversity outcomes



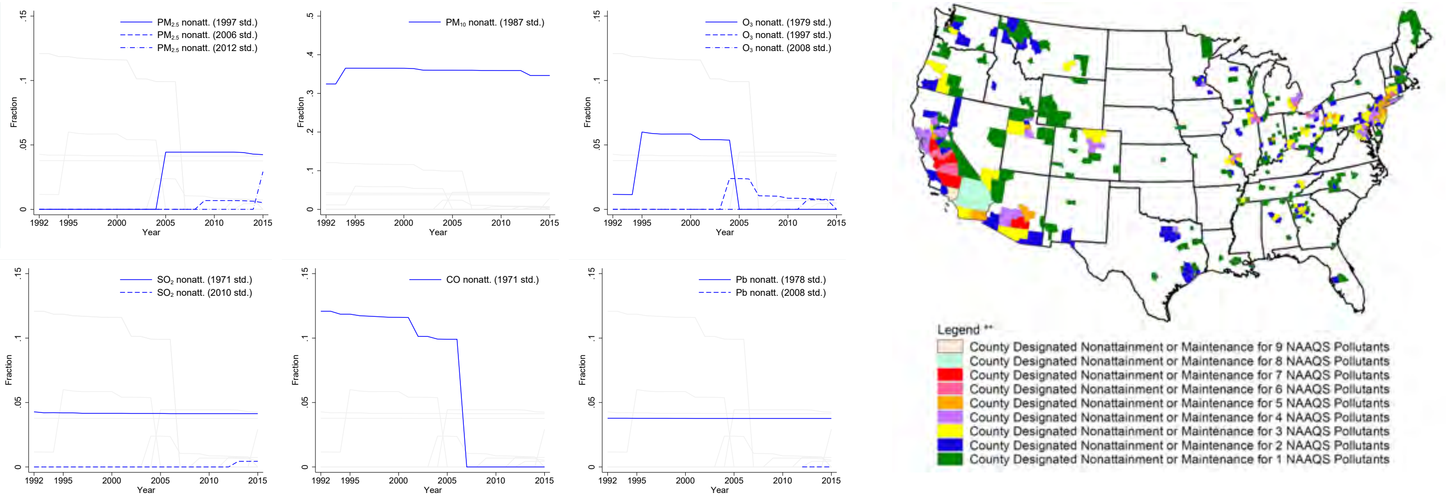
Notes: Panel (a) highlights 54 counties selected by a "zero-stage" LASSO regression of Susquehanna County, PA's daily aerosol pollution on all other 2,996 counties' upwind component vector pollution. The size of each circle is approximately proportional to the contributing county's post-LASSO elasticity 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. Specifically, the regression is $Y_{cjt} = \beta \cdot IV_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$.

Figure 7: Military Buildups and Biodiversity Outcomes: Overall vs. Pollution Effects



Notes: Bars and standard error range plots show the impacts of military buildup shocks 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 on pollution with (ii) the IV estimates of the effect of pollution on biodiversity outcomes.

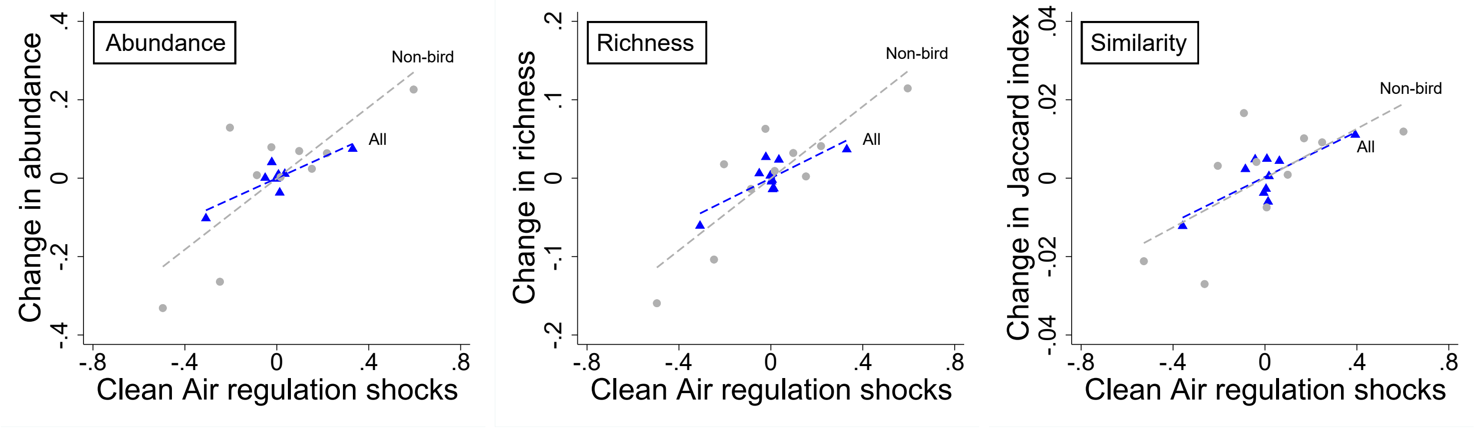
Figure 8: Clean Air Act Regulations and Biodiversity Outcomes



(a) Share of sampling sites in nonattainment jurisdictions

(b) Location of nonattainment jurisdictions

(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. Specifically, the regression is $Y_{cjt} = \beta \cdot Nonattainment_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$, where $Nonattainment_{county,t}$ is the total number of nonattainment designations in the county-year.

Table 1: Summary Statistics

	(1) Observations	(2) Abundance	(3) Richness	(4) Similarity
All species	66,418	39,132	15.44	0.425
		[1,203,156]	[19.62]	[0.269]
Amphibians	45	2,563	7.267	0.943
		[2,557]	[2.526]	[0.099]
Birds	51,695	216.8	18.43	0.419
		[1,967]	[20.5]	[0.252]
Fish	804	1,405	16.07	0.702
		[6,330]	[8.501]	[0.128]
Freshwater invertebrates	445	5,566,008	24.32	0.715
		[13,599,038]	[15.43]	[0.144]
Freshwater plants	39	2,747,857	83.87	0.470
		[1,193,869]	[11.56]	[0.045]
Mammals	5,658	21.18	1.91	0.316
		[174.2]	[1.958]	[0.339]
Terrestrial invertebrates	6,071	53.32	3.912	0.446
		[520.2]	[11.12]	[0.239]
Terrestrial plants	1,661	1,281	6.515	0.651
		[8,296]	[11.81]	[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 farming, fishing, and hunting. 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. Specifically, the regression is $Y_{cjt} = \sum \beta_i \cdot \log Industry_{i,st} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$, where $Industry_{i,st}$ refers to sector income from industry i. 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)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-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)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-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. The first stage regression is $\log GDP_{st} = \beta \cdot \left(\frac{MS}{GDP}\right)_{state,1966-1971} \times MS_t + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. Column 2 through 4 reports reduced form estimates following $Y_{cjt} = \beta \cdot \left(\frac{MS}{GDP}\right)_{state,1966-1971} \times MS_t + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. 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. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

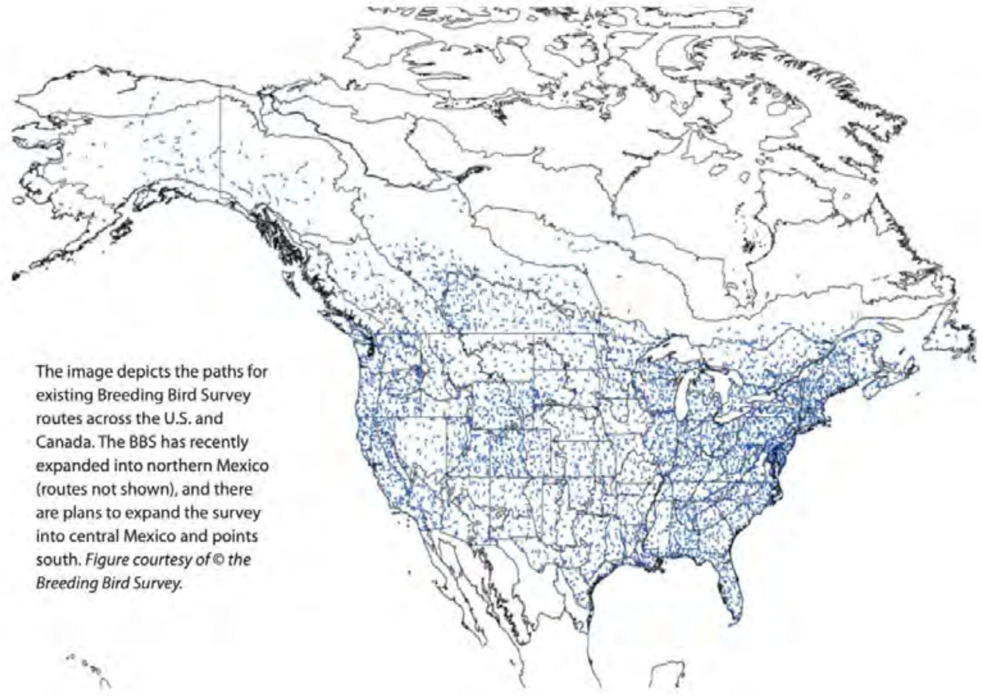
Table 4: Air Pollution and Biodiversity Outcomes

	(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 results estimated following $Y_{cjt} = \beta \cdot \log Pollution_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. The second row reports IV regression estimates, using county's upwind pollution shock as the instrumental variable for logged local pollution. Upwind pollution shocks are constructed following equation (3). 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

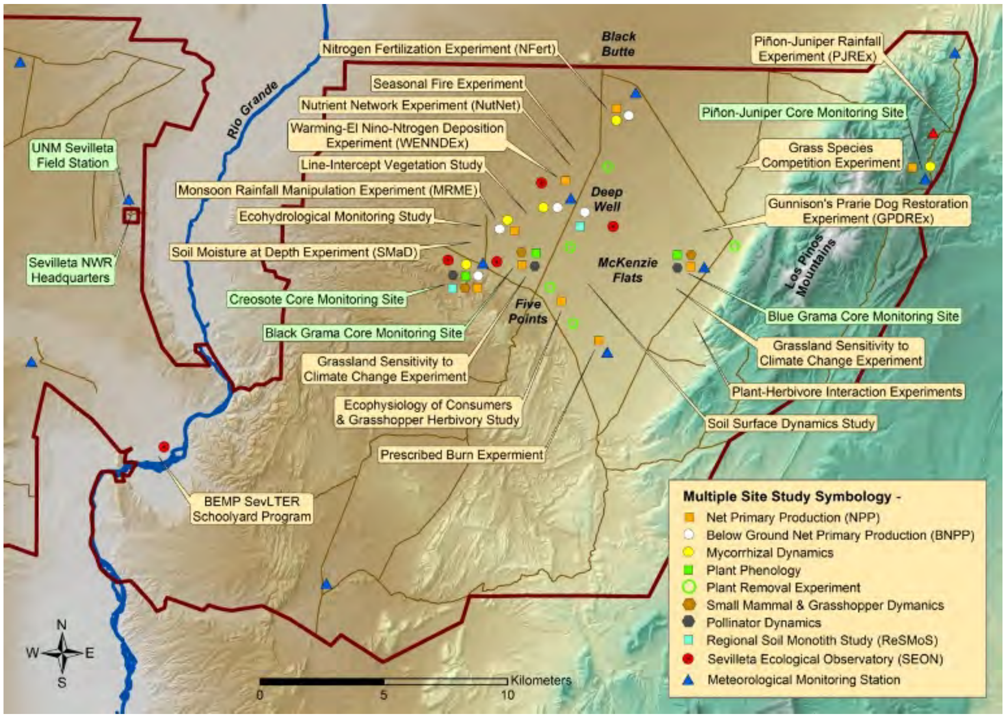
Figure A.1: North American Breeding Bird Survey routes across the U.S. and Canada



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.*

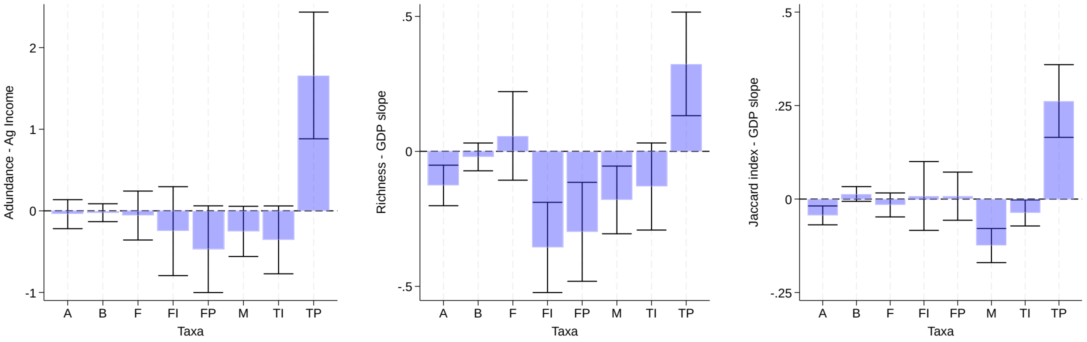
Note: This figure is from Ziolkowski Jr et al. (2010). It shows the sample collecting routes in the North American Breeding Bird Survey (BBS), which is one of the studies in the BioTIME database. The BBS is a long-term and large-scale avian monitoring program that tracks the status and trends of North 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



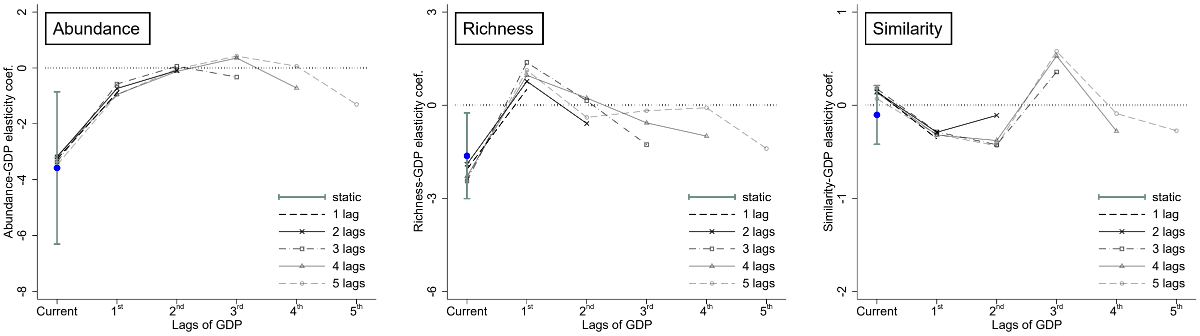
Note: This figure is from the project overview for the Sevilleta Long Term Ecological Research (LTER) Program at http://sevlter.unm.edu/. As shown in this figure, several studies included in BioTIME are conducted under the Sevilleta Long Term Ecological Research (SLTER) Program at the 100,000 hectare 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: Biodiversity - Agricultural Income, by Taxa



Note: This figure reports the heterogenous effect of agricultural income on biodiversity outcomes by taxa. The regressions use the same specification as our baseline estimation in equation (1), but replacing GDP with agricultural income. The taxa abbreviations are for amphibians (A), birds (B), fish (F), freshwater invertebrates (FI), freshwater plants (FP), mammal (M), terrestrial invertebrates (TI), and terrestrial plants (TP).

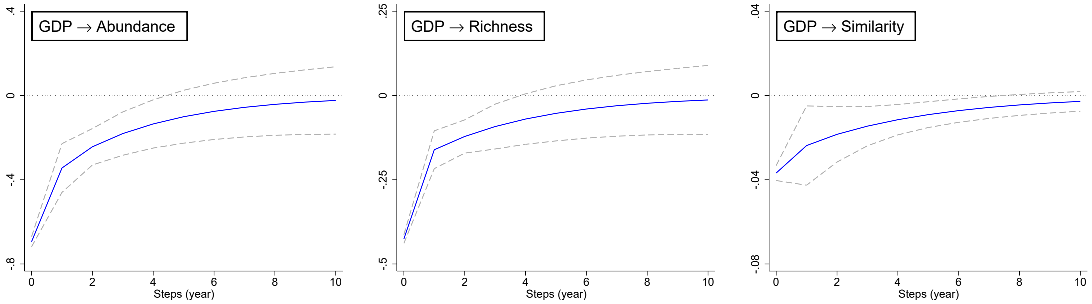
Figure A.4: Dynamic Effects: Distributed Lag Models of the Biodiversity - GDP Relationship



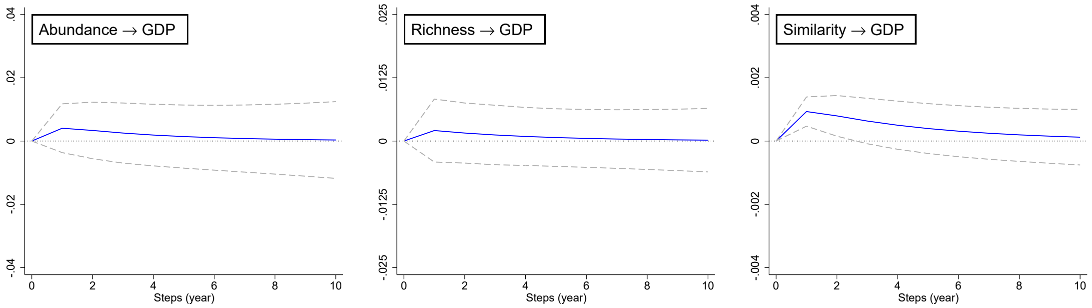
Notes: This figure plots coefficients when regressing biodiversity outcomes on the current and yearly lags of GDP. Each line represents a separate regression with different numbers of lags. For each outcome, the range bar shows point estimate and 95% confidence interval of the baseline, static specification with no lags of GDP. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. Specifically, the regression is $Y_{cjt} = \sum_{\tau=0}^{5} \beta_\tau \log GDP_{s,t-\tau} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$.

Figure A.5: Dynamic Effects: Panel Vector Autoregression (VAR) Impulse Response Functions

(a) Responses of biodiversity outcomes to GDP

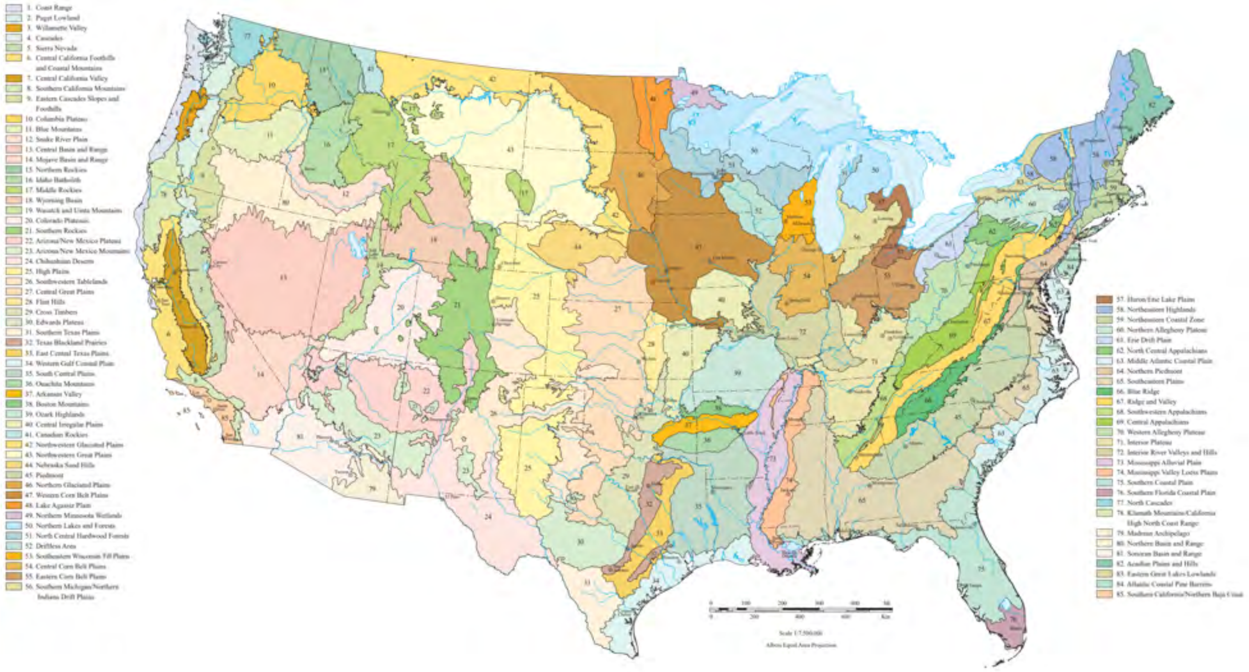


(b) Responses of GDP to biodiversity outcomes



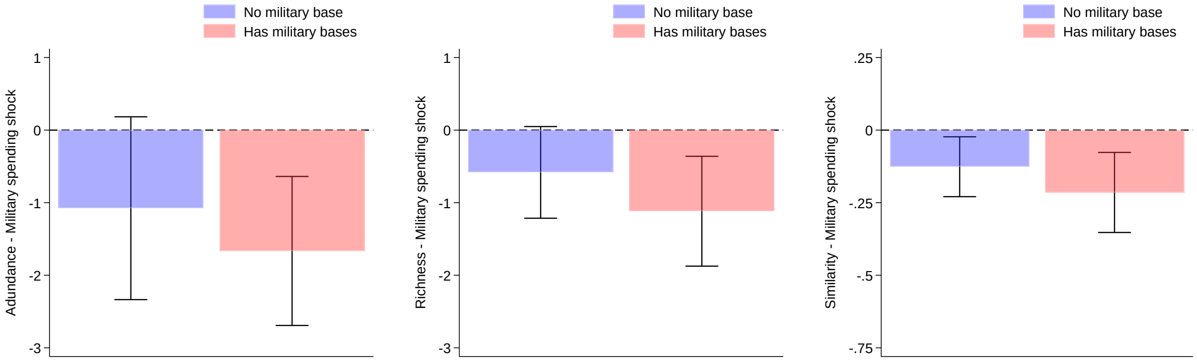
Notes: This figure plots orthogonalized impulse response functions from first-order panel vector autoregression (VAR). Three separate models are estimated for log GDP and log abundance (left column), log GDP and log richness (middle column), and log GDP and Jaccard index (right column). VAR models are 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.6: Eco-regions of the Continental United States



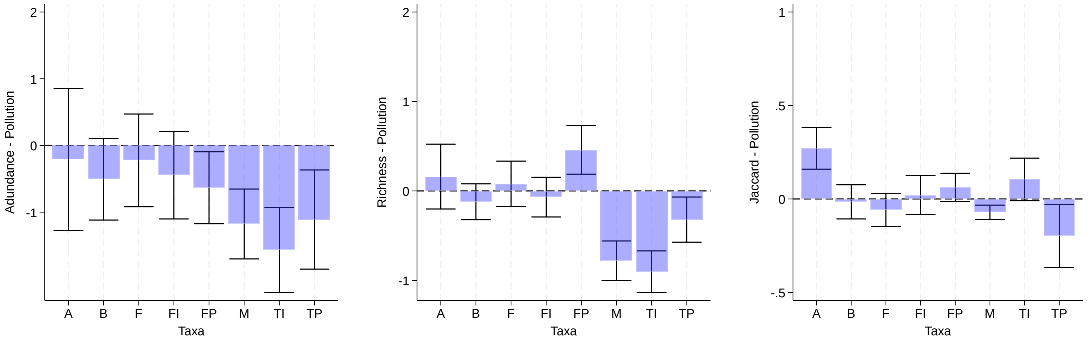
Note: This map shows 85 Level III eco-regions – areas where ecosystems are generally similar – across the continental U.S. as of April 2013. Source: U.S. Environmental Protection Agency.

Figure A.7: Heterogeneous Effect of Military Spending Shocks on Biodiversity: Military Bases



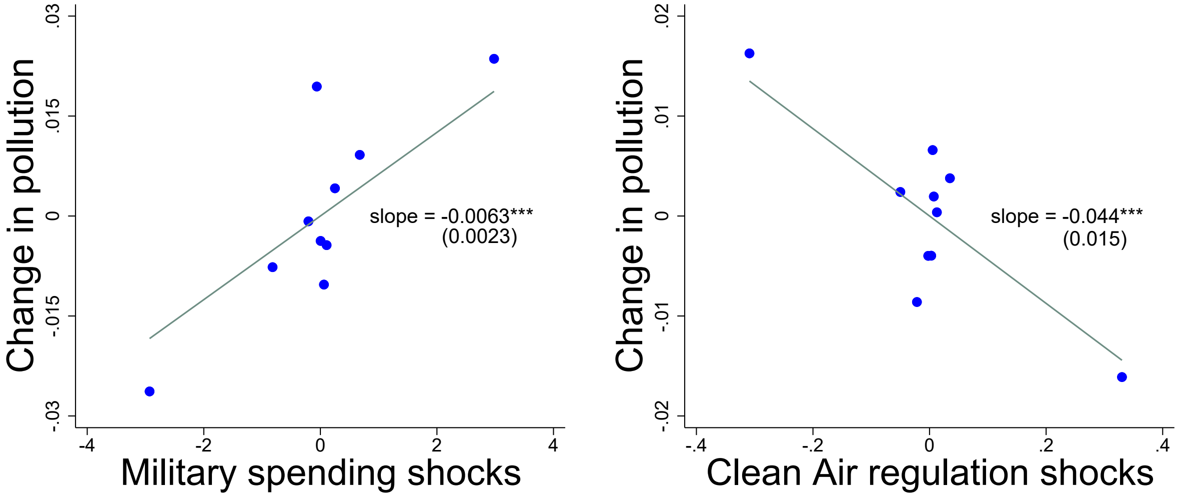
Notes: This set of figures shows the heterogeneous effects of military spending shocks on biodiversity across sampling sites, distinguishing between locations in counties with at least one military base (30% of the sample) and those without any military bases. The figures indicate a more pronounced negative impact on biodiversity in proximity to military bases, consistent with exemptions from environmental regulations obtained by various activities on these bases. But even locations without military bases experience a lesser, though still discernible, effect.

Figure A.8: Effect of Air Pollution on Biodiversity by Taxa



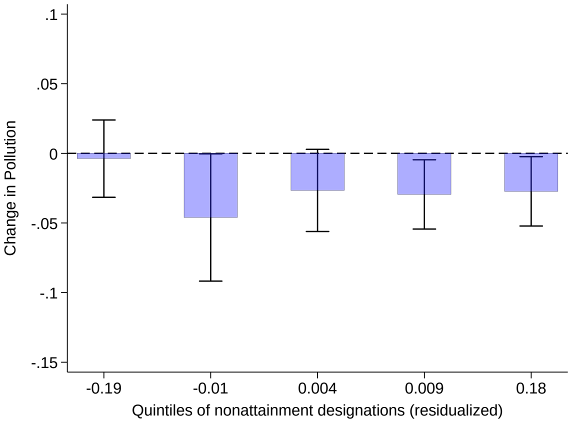
Notes: This figure reports heterogeneous effects of air pollution on biodiversity by taxa. The regressions follow equation (1) by changing the independent variable GDP to a county's annual logged Aerosol Optical Depth pollution level. All regression include location-by-taxa and year fixed effects. Standard errors are clustered at the state level. Abbreviations are for amphibians (A), birds (B), fish (F), freshwater invertebrates (FI), freshwater plants (FP), mammal (M), terrestrial invertebrates (TI), and terrestrial plants (TP).

Figure A.9: Air Quality Effects of Military Spending and Environmental Regulation Shocks



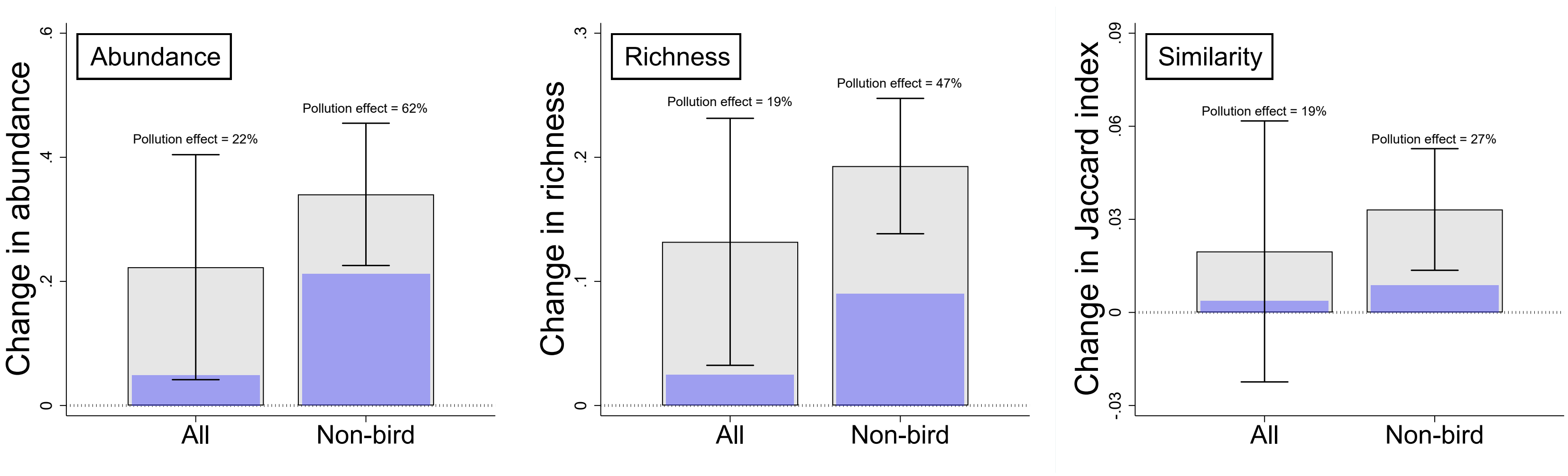
Notes: These figures show decile bin scatterplots of local pollution against the military buildup shocks (left panel) and the Clean Air Act regulation shocks (right panel). The underlying estimation follows equation (1), regressing pollution on military spending shocks and on Clean Air regulation shocks in two separate regressions. All variables are residualized with location-by-taxa and year fixed effects. Standard errors are clustered at the state level. Specifically, the regressions are $\log Pollution_{county,t} = \beta \cdot \left(\frac{MS}{GDP}\right)_{state,1966-1971} \times MS_t + \eta_{cj} + \eta_t + \varepsilon_{cjt}$, and $\log Pollution_{county,t} = \beta \cdot Nonattainment_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. We test that the pattern in the right panel is not driven by the two outliers in Figure A.10.

Figure A.10: Air Quality Effects of Environmental Regulation Shocks



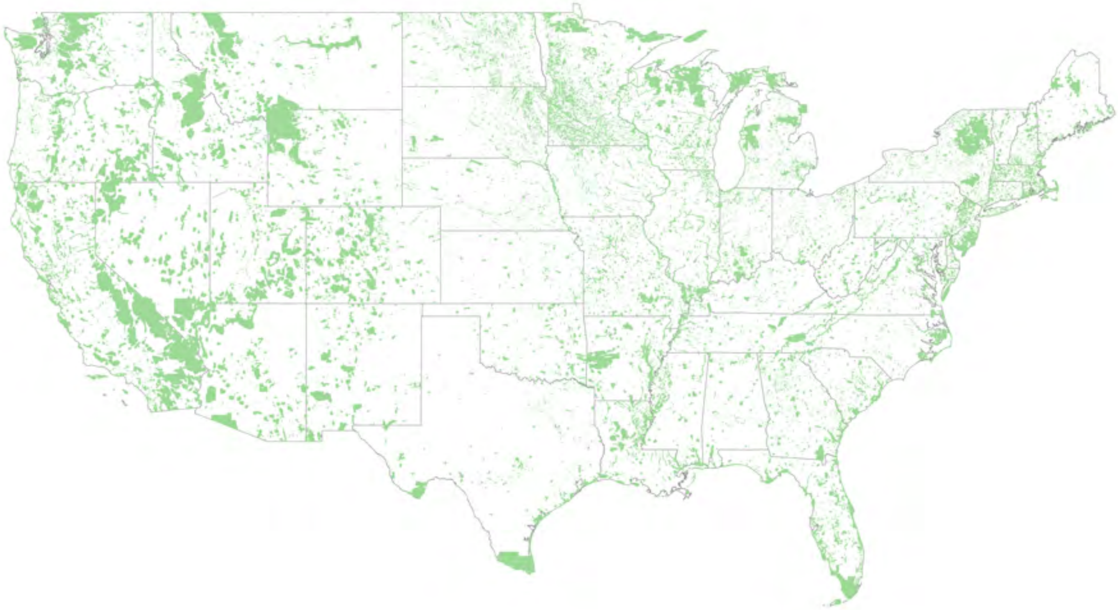
Notes: This figure compliments the right panel of Figure A.9 to show that the pattern in the right panel is not driven by the two outliers. The pattern in the right panel of Figure A.9 is partly driven by the nature of county-year CAA nonattainment status, which is a categorical variable with many zeroes and transitions into and out of nonattainment. When such variables are residualized by fixed effects, they often exhibit a cluster near zero and several distinct clusters further from zero. To test explicitly, we residualize both the log of pollution and CAA shocks, controlling for location-by-taxa and year fixed effects. We then categorize the distribution of the residualized CAA shocks into five bins with equal observations. The horizontal axis of the figure represents the mean of the residualized CAA shock in each bin. The regression shown in the figure is $log(pollution)_{cjt} = Nonattainment_{county,t} \times I_{CAA\_5gp} + \eta_{cj} + \eta_t + \epsilon_{cjt}$, where I_{CAA_5gp} is the residualized CAA shock quintile group. If the downward-sloping fitted line were driven by outliers at low and high values of the CAA shock, we would expect to see negative coefficients only in the 1^{st} and 5^{th} groups, while the middle three groups would produce insignificant or non-negative coefficients. However, all five groups show negative elasticity estimates, indicating that there is a consistent negative relationship between CAA shocks and pollution, confirming that our results are not driven by outliers. The reason why the binscatter plot in Figure A.9 appears to have two outliers is due to the narrow distribution of residualized CAA shocks, which concentrate around zero. This concentration is evident from the means of the five equal-observation bins shown on the horizontal axis.

Figure A.11: Clean Air Act Regulations and Biodiversity Outcomes: Overall vs. Pollution Effects



Notes: Bars and standard error range plots show the impacts of Clean Air Act regulation shocks 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 Clean Air Act regulation shocks on pollution with (ii) the IV estimates of the effect of pollution on biodiversity outcomes.

Figure A.12: Location of Protected Areas



Notes: Green represents areas that were protected any time in the World Database on Protected Areas (WDPA) sample.

Table A.1: Agriculture 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		
I. Subsectors of agriculture						
Agricultural income: crop & animal farming	0.049	-0.014	0.010	0.758**	0.136	0.109**
	(0.077)	(0.027)	(0.008)	(0.304)	(0.102)	(0.044)
Agricultural income: fishing & hunting	0.011	0.003	-0.002	0.011	0.003	-0.015**
	(0.008)	(0.004)	(0.003)	(0.042)	(0.019)	(0.007)
Agricultural income: ag support	-0.048	-0.012	0.007	-0.495	-0.197*	0.012
	(0.120)	(0.036)	(0.015)	(0.290)	(0.101)	(0.027)
II. Federal government conservation program spending						
Agricultural income	-0.019	-0.025	0.010	-0.214	-0.155	0.019
	(0.075)	(0.030)	(0.010)	(0.286)	(0.100)	(0.020)
Gov conservation spending	0.056	0.024*	0.003	0.382*	0.222***	0.034
	(0.035)	(0.014)	(0.005)	(0.190)	(0.070)	(0.030)

Notes: All income and spending variables are in log. In panel I, agricultural income is broken down to crop & animal farming (NAICS = 111-112), fishing & hunting (NAICS = 114), and ag support (NAICS = 115). Specifically, the regression is $Y_{cjt} = \beta_1 \cdot \log crop_{st} + \beta_2 \cdot \log fish_{st} + \beta_3 \cdot \log support_{st} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. In panel II, "Gov conservation spending" is federal government payments to the state-year under conservation programs including the Conservation Reserve Program, Agricultural Conservation Easement Program, Environmental Quality Incentives Program, Conservation Stewardship Program, Regional Conservation Partnership Program, and Conservation Technical Assistance. Data are sourced from USDA. Specifically, the regression is $Y_{cjt} = \beta_1 \cdot \log Ag_{st} + \beta_2 \cdot \log Conservation_{st} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. 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 A.2: 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.3: 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 (last 5-y)		-11.39*	-3.60		-2.93	3.53		-1.09	-1.56
		(6.23)	(6.35)		(2.83)	(3.83)		(0.79)	(1.15)
Max. GDP growth (last 5-y)			-7.68***			-6.36**			0.44
			(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 (last 5-y)		-26.10***	-10.26**		-12.14***	-4.87**		-4.16***	-7.77***
		(4.57)	(3.75)		(1.53)	(1.74)		(0.98)	(1.38)
Max. GDP growth (last 5-y)			-9.83***			-4.51***			2.28***
			(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.4: The Biodiversity - GDP Relationship: Alternative Data Aggregation Methods

	(1) Abundance	(2) Richness	(3) Similarity
Panel A. [Long-Difference] Select durations >= 10 years			
GDP	-3.596***	-1.475***	0.674***
	(0.321)	(0.252)	(0.167)
Observations	2968	2968	2744
Panel B. [Long-Difference] All studies, 10-year difference			
GDP	-2.772***	-1.810***	0.040
	(0.090)	(0.052)	(0.029)
Observations	9682	9682	7022
Panel C. [Fixed Effects] Aggregate to 5 years			
GDP	-5.408***	-1.259	-0.237
	(1.721)	(0.840)	(0.192)
Observations	15486	15486	12580
Panel D. [Fixed Effects] Aggregate to 10 years			
GDP	-3.027	-0.560	0.114
	(2.265)	(1.195)	(0.143)
Observations	10768	10768	10292

Notes: Outcome variables are differences in log of abundance, log of richness, and level of the Jaccard index. Independent variable is the difference in logs of state per capita GDP. In Panel A, only studies lasting more than 10 years are included. Differences are taken between the first and last years of each study. Each observation represents an ecological study at the location-by-taxa level. In Panel B, all studies are included. Differences are taken between the current year's value and its lagged 10-year value of each study. Observations are at the location-by-taxa-by-year panel. In Panel C & D, we only include studies that last at least 5 years, then aggregate the data into 5- or 10-year periods. All regressions include location-by-taxa and year group (each 5 or 10 years) fixed effects. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.5: Economic Conditions and Biodiversity Sampling Activities

	(1) Log(study duration)	(2)	(3)	(4)	(5)	(6)	(7)
		$\mathbb{1}$(start year)		$\mathbb{1}$(end year)		$\mathbb{1}$(missed year)	
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)
$\text{GDP}_{\text{t-1}}$	-	-	-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.6: The Biodiversity - GDP Relationship: 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 1^{th}-99^{th} 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 5^{th}-95^{th} 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 10^{th}-90^{th} 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.7: The Biodiversity - GDP Relationship: 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 $\geq$ 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 $\leq$ 5y	0.943	1.460	-0.235
	(1.665)	(1.498)	(0.272)
Observations ($\geq$ 20y)	12,658	12,658	12,643
Observations (5y to 20y)	26,799	26,799	23,210
Observations ($\leq$ 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.8: The Biodiversity - GDP Relationship: 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.9: The Biodiversity - GDP relationship: State versus County Output Measurement

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
I. State Level GDP (coverage: 1969-2015)						
OLS	-3.580**	-1.631**	-0.104	-5.903***	-3.302***	-0.368
	(1.353)	(0.685)	(0.157)	(0.990)	(0.271)	(0.262)
2SLS	-4.485***	-2.753**	-0.535***	-6.225***	-3.193***	-0.638***
	(1.594)	(1.226)	(0.183)	(1.167)	(0.732)	(0.149)
Observations	57,714	57,714	44,479	11,861	11,861	10,335
II. County Level Income (coverage: 1969-2015)						
OLS	-0.697*	-0.459**	-0.026	-3.858***	-2.220***	-0.226
	(0.381)	(0.225)	(0.033)	(0.873)	(0.255)	(0.140)
2SLS	-5.189***	-3.102***	-0.587***	-5.983***	-2.992***	-0.647**
	(0.724)	(0.505)	(0.216)	(0.511)	(0.130)	(0.284)
Observations	57,362	57,362	44,310	11,629	11,629	10,280

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 different levels of measurement for economic activities. In panel I, the independent variable is the state annual level GDP per capita from 1969 to 2015 obtained from the BEA, which is our baseline specification. In panel II, the independent variable is the BEA county level income per capita from 1969 to 2015. For both panels, the first row reports OLS regression estimates, and the second row reports IV regression estimates, using national military spending shocks as the instrument. Columns 1-3 report full sample estimation, while columns 4-6 exclude observations corresponding to bird species. All regressions include location-by-taxa and year fixed effects. Standard errors are clustered at the state level for panel I and at the county level for panels II. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.10: The Biodiversity - GDP Relationship: Level of Output Measurement

	(1) #Obs.	(2) Abundance	(3) Richness	(4) Similarity
Panel A. All species				
State Level GDP	54,887	-3.580** (1.353)	-1.631** (0.685)	-0.104 (0.157)
Income at County	60,406	-0.697* (0.381)	-0.459** (0.225)	-0.026 (0.033)
Income at Level III Eco-region	60,343	-1.014 (1.114)	-0.714 (0.531)	0.077 (0.161)
Panel B. Non-bird species				
State Level GDP	13,331	-5.903*** (0.990)	-3.302*** (0.271)	-0.368 (0.262)
Income at county	14,306	-3.858*** (0.873)	-2.220*** (0.255)	-0.226 (0.140)
Income at Level III Eco-region	14,213	-3.369** (1.524)	-2.375*** (0.327)	-0.158 (0.306)

Notes: This table reports the panel OLS estimation results using equation (1) when economic activities is measured at alternative geographic levels. First, we repeat the baseline state-level results, and county-level income OLS results from Appendix Table A.9 for reference. Then, we use the concept of *eco-region* developed by the U.S. Environmental Protection Agency, which divides the contiguous U.S. into 85 mutually exclusive areas where ecosystems are similar. 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. 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 (first row or each panel), county (second row), and eco-region (third row). *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.11: The Biodiversity - GDP Relationship: Level of Data Aggregation

	(1) #Obs.	(2) Abundance	(3) Richness	(4) Similarity
Panel A. All species				
Aggregation: 3-km hex. bin	11,915	-1.209*** (0.325)	-0.454*** (0.136)	-0.066 (0.062)
Aggregation: 10-km hex. bin	9,881	-0.887** (0.377)	-0.288* (0.154)	-0.061 (0.059)
Aggregation: 16-km hex. bin	8,896	-0.840** (0.379)	-0.206 (0.187)	-0.076 (0.055)
Panel B. Non-bird species				
Aggregation: 3-km hex. bin	1,401	-1.699** (0.672)	-0.631** (0.271)	-0.261 (0.159)
Aggregation: 10-km hex. bin	923	-1.719** (0.798)	-0.586 (0.354)	-0.145 (0.145)
Aggregation: 16-km hex. bin	694	-1.417* (0.756)	-0.443 (0.518)	-0.132 (0.139)

Notes: This table reports the panel OLS estimation results using equation (1) when the panel data are aggregated up to hexagon bins of various resolution. Both biodiversity outcomes and economic activities are aggregated to the same resolution. Panel A reports full sample estimation. Panel B excludes observations that correspond to bird species. All regressions include hexagon-by-taxa and year fixed effects. Standard errors are clustered at the hexagon grid level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.12: The Biodiversity - GDP Relationship (2SLS): Robustness Specifications

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
IV base period 1966-2006	-5.086**	-3.127*	-0.529***	-6.881***	-3.610***	-0.600***
	(2.175)	(1.649)	(0.190)	(0.652)	(0.454)	(0.098)
	[6.035]	[6.035]	[6.186]	[47.40]	[47.40]	[55.32]
Shocks × state FEs as IVs	-4.575**	-1.932*	-0.218	-7.720***	-4.339***	-0.406***
	(1.950)	(1.031)	(0.145)	(0.555)	(0.362)	(0.085)
	[1.794]	[1.794]	[2.168]	[84.36]	[84.36]	[106.6]
Multiple IVs LIML	-4.485***	-2.753**	-0.535***	-6.225***	-3.193***	-0.638***
	(1.594)	(1.226)	(0.183)	(1.167)	(0.732)	(0.149)
	[7.430]	[7.430]	[7.071]	[37.05]	[37.05]	[34.46]
Census Division trends	-5.844***	-3.049*	-0.750***	-4.755***	-2.079***	-0.910***
	(2.050)	(1.554)	(0.226)	(0.764)	(0.498)	(0.236)
	[6.509]	[6.509]	[7.034]	[16.92]	[16.92]	[16.07]
Total state GDP	-2.419***	-1.485**	-0.301***	-4.370***	-2.241***	-0.469***
	(0.852)	(0.612)	(0.105)	(0.773)	(0.499)	(0.119)
	[16.79]	[16.79]	[15.10]	[37.10]	[37.10]	[32.94]
Total county income	-2.328***	-1.392***	-0.288**	-3.968***	-1.984***	-0.452**
	(0.590)	(0.370)	(0.109)	(0.368)	(0.149)	(0.210)
	[14.83]	[14.83]	[13.65]	[10.84]	[10.84]	[10.59]
Per capita county income	-5.189***	-3.102***	-0.587**	-5.983***	-2.992***	-0.647**
	(1.174)	(0.755)	(0.241)	(0.568)	(0.160)	(0.302)
	[4.634]	[4.634]	[4.953]	[9.213]	[9.213]	[9.967]

Notes: Each cell is a separate regression. Row names specify the robustness checks described in Section 4.1 and 5.1. This table reports the implied biodiversity-GDP elasticity estimates using military spending shocks as the underlying source of policy variation. Numbers in brackets are Kleibergen-Paap F-statistics of the first stage estimation. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.13: Military Spending Shocks and Biodiversity Outcomes: 2-Step with Bootstrap SEs

	(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	0.181**	-1.341**	-0.823**	-0.164***	-	-	-
	(0.067)	(0.567)	(0.354)	(0.060)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-7.391**	-4.537**	-0.905**
	-	-	-	-	(3.443)	(1.887)	(0.370)
Observations	1,367	57,714	57,714	44,479	57,714	57,714	44,479
Panel B. Non-bird species							
Military spending	0.105	-3.286***	-1.685***	-0.360***	-	-	-
	(0.09)	(1.075)	(0.624)	(0.057)	-	-	-
$\widehat{\text{GDP}}$	-	-	-	-	-31.261**	-16.033*	-3.424***
	-	-	-	-	(15.330)	(8.885)	(1.042)
Observations	397	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. The first stage regression is at the state-level: $\log GDP_{st} = \beta \cdot \left(\frac{MS}{GDP}\right)_{state,1966-1971} \times MS_t + \eta_s + \eta_t + \varepsilon_{st}$. Column 2 through 4 reports reduced form estimates following $Y_{cjt} = \beta \cdot \left(\frac{MS}{GDP}\right)_{state,1966-1971} \times MS_t + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. Column 5 through 7 reports second stage estimates following $Y_{cjt} = \beta \cdot \log \widehat{GDP}_{st} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. 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 bootstrapped clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.14: The Biodiversity - GDP Relationship: Heterogeneous Effect of Military Bases

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Abundance			Richness			Similarity	
Panel A. All species									
GDP	-3.580**	-3.567**	-3.162***	-1.631**	-1.621**	-1.305**	-0.104	-0.094	-0.014
	(1.353)	(1.366)	(1.161)	(0.685)	(0.691)	(0.548)	(0.157)	(0.140)	(0.132)
GDP × I(base)		-0.253			-0.187			-0.095	
		(0.485)			(0.394)			(0.080)	
GDP × Base area share			-1.341***			-1.043***			-0.217**
			(0.153)			(0.142)			(0.089)
Observations	54,887	54,887	54,887	54,887	54,887	54,887	42,406	42,406	42,406
Panel B. Non-bird species									
GDP	-5.903***	-4.794***	-2.739**	-3.302***	-3.376***	-2.318***	-0.368	0.022	0.030
	(0.990)	(1.017)	(1.074)	(0.271)	(0.388)	(0.628)	(0.262)	(0.176)	(0.426)
GDP × I(base)		-1.451**			0.097			-0.495***	
		(0.604)			(0.234)			(0.130)	
GDP × Base area share			-2.383***			-0.741**			-0.313
			(0.775)			(0.346)			(0.202)
Observations	13,331	13,331	13,331	13,331	13,331	13,331	12,161	12,161	12,161

Notes: Military spending shock is constructed as in equation (2). Column 1 reproduces the baseline estimations. Column 2 add the interaction term of a dummy variable indicating whether a sampling site is located in counties with at least one military base (30% of the sample). In Column 3, we interact the military spending shock with a continuous variable measuring the proportion of county land occupied by military bases. The dummy variable (Column 2) and the share (Column 3) has no variation across years, so the effects are absorbed in location fixed effects. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.15: Air Pollution and Biodiversity Outcomes: Add Weather Controls

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
$\widehat{\text{Pollution}}$ (IV)	-1.240**	-0.627**	-0.084**	-3.183***	-1.392***	-0.096*
	(0.479)	(0.251)	(0.034)	(0.508)	(0.257)	(0.048)
TMAX	-0.004	-0.001	-0.002***	0.013	0.009**	-0.006***
	(0.004)	(0.004)	(0.000)	(0.008)	(0.003)	(0.001)
TMIN	0.009*	0.004	0.002***	0.014**	0.002	0.006***
	(0.005)	(0.003)	(0.000)	(0.006)	(0.003)	(0.001)
PRCP	-0.007**	-0.003	-0.002**	-0.007	0.002	-0.005**
	(0.003)	(0.002)	(0.001)	(0.006)	(0.002)	(0.002)
Kleibergen-Paap F-stat.	250.8	250.8	212.5	243.4	243.4	380.7
Observations	52,860	52,860	40,422	12,726	12,726	11,599

Notes: 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 IV regression estimates use the county's upwind pollution shock as the instrumental variable for logged local pollution. Weather controls, including a county's annual maximum and minimum temperature, and precipitation, are added to the baseline estimations. 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. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Table A.16: 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.17: 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. The first stage regression is $\log GDP_{st} = \beta \cdot Nonattainment_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. Column 2 through 4 reports reduced form estimates following $Y_{cjt} = \beta \cdot Nonattainment_{county,t} + \eta_{cj} + \eta_t + \varepsilon_{cjt}$. 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.18: 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. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Data Appendix

BioTIME is an open-source database of biodiversity time series, collecting raw data on species identities and abundances in ecological assemblages over time. BioTIME does not create original data but serves as a meta-database containing hundreds of ecological datasets from well-known sources. The data were acquired from the largest databases, including Global Biodiversity Information Facility (GBIF), Ocean Biogeographic Inforamtion System (OBIS), and Ecological Data Wiki. The original data paper by Dornelas et al. (2018) provides a detailed description of how datasets from various studies are collected and processed to be compiled as time series.

Alternative datasets of biodiversity An inventory of global biodiversity datasets can be found in IUCN SSC Species Monitoring Specialist Group (2023) IUCN (2023), where over 50 datasets useful for studying biodiversity status are summarized. BioTIME is included in this summary along with several other biodiversity datasets. Here, we present a brief comparison of BioTIME with popular alternative open-source datasets, highlighting some of its advantages.

Category	Example datasets	Description of example data	Advantages of BioTIME
Already Included in BioTIME	Ocean Biogeographic Inforamtion System (OBIS), Global Biodiversity Information Facility (GBIF), North America Breeding Bird Survey (BBS)	OBIS: Huge global database on marine species linked to GBIF. Over 164 million records of over 137,000 species from more than 3,300 datasets (as of October 2020).	These are the most comprehensive datasets on biodiversity. They are already included in BioTIME, eliminating the need to process these datasets individually.

		GBIF: Houses over 1.6 billion species occurrence records from over 54,600 data sets (as of October 2020). BBS: A long-term and large-scale monitoring program that tracks the status and trends of bird population annually. BBS: A long-term and large-scale monitoring program that tracks the status and trends of bird population annually.	
Endangered species	IUCN Red List of Threatened Species	Extinction risk of species with data on range, population trends, habitat use, life history traits, use and trade, threats, conservation actions currently in place and conservation actions needed.	The IUCN Red List is cross-sectional, focusing on current extinction risks for specific species, whereas BioTIME is longitudinal, capturing biodiversity data over time. BioTIME encompasses a broader range of species without exclusive focus on extinction risks, offering a comprehensive perspective on global biodiversity status.

Population level data	Living Planet Index	Trends in over 27,000 populations of more than 4,700 vertebrate species.	The LPI primarily focuses on population-level trends of individual vertebrate species, whereas BioTIME facilitates the study of assemblage (community-level) dynamics and temporal changes across diverse environments.
Taxon-specific or Biome-specific	FishBase, International Waterbird Census Database	FishBase: A global biodiversity information system on fishes: taxonomy, biology, trophic ecology, life history & uses, and historical data going back 250 years. Links to several other marine data sources. International Waterbird Census Database: Current and historic estimates, trends and 1% thresholds for over 800 waterbird species and 2,300 biogeographic populations worldwide.	BioTIME enables comparison of biodiversity trends across taxa by collecting more than 50,000 species in 30 biomes.

Cross-sectional data	PREDICTS	Focuses on the impact of human activities on biodiversity, particularly land use and climate change.	PREDICTS primarily relies on space-for-time substitutions to provide data for assessing biodiversity patterns across different habitats and regions. In contrast, BioTIME complements PREDICTS by focusing on temporal changes within ecological communities across various biomes.
Citizen Science data	eBird	Citizen science bird observations collected by bird lovers.	Citizen science data, despite providing large quantities of information, often faces selection issues that complicate its usability compared to data collected through scientific research, such as in BioTIME.

Advantages of BioTIME in this study Existing biodiversity datasets commonly provide only cross-sectional information on the geographic distribution of species, such as the IUCN Red List of Threatened Species (IUCN, 2021), or panel data limited to a single taxonomic class, such as the North American Breeding Bird Survey (USGS, 2014). These constraints hinder broad-scale studies investigating the link between the economy and ecosystems. We overcome these constraints by utilizing a novel database, BioTIME, which aggregates data from hundreds of ecological studies maintaining longitudinal information on species counts or biomass (Dornelas et al., 2018; Blowes et al., 2019). BioTIME allows us to construct widely-used metrics for capturing ecosystem changes. The included studies, many spanning decades, adhere to consistent sampling protocols within each location over time, known as ‘assemblage time series’. This consistency makes year-over-year variations in sampling outcomes likely to reflect genuine changes in underlying ecosystem conditions.

BioTIME offers several features that enhance its utility for our analysis. First, all studies included in the dataset maintain consistent sampling protocols over time, ensuring that variation in outcomes within each study is not influenced by changes in species detection methods (e.g., sighting versus trapping). Second, each panel dataset study includes at least two years of sampling, with some extending over multiple decades. This allows us to leverage within-study variation and control for any differences in sampling protocols across studies using location fixed effects. Third, the dataset encompasses information on approximately 40,000 unique species or genera across more than 15,000 sampling locations, spanning diverse biomes and ecosystems. While this coverage is not exhaustive of the entire ecosystem, we believe the data provide a robust understanding of how different organisms and biomes are influenced by economic activities.

Data Processing Steps We apply several sample restrictions when processing the dataset for analysis. First, we focus on years after 1966 to align with our economic data. Second, we exclude marine ecosystem studies, which often occur far offshore and are challenging to link with measures of economic activity. Third, we aggregate raw species sampling observations to the taxon-location-year level. The taxa included are birds, fish, mammals, terrestrial invertebrates, freshwater invertebrates, terrestrial plants, and freshwater plants. Fourth, we exclude studies that report only total species biomass or only species presence indicators, to ensure a unified measure of abundance. These studies account for less than 1 percent of the abundance data. Including these samples in richness and similarity analyses yields virtually identical results. Lastly, we focus on studies conducted in the United States. This country contributes approximately three-quarters of the total observations in the dataset, with New Zealand being the next largest contributor at about 10%. Focusing on the United States also allows us to implement several well-understood quasi-experiments in our causal analysis. These sample restrictions result in a maximum of 66,418 taxon-location-year observations.

Robustness Tests on the Validity of the Military Spending IV

In assessing the validity of the instrumental variable (IV) analysis, we conducted several tests to evaluate the plausibility of the identifying assumptions following Goldsmith-Pinkham et al. (2020).

First, correlates of the share variable. Before presenting the tests, we would like to add a note about the causal interpretation of the biodiversity-GDP relationship, which partly reiterates a discussion included in Section 4.1 of the paper. The correlates tests proposed by Goldsmith-Pinkham et al. (2020) are designed to diagnose whether the instruments predict the outcome through alternative channels beyond those posited by the researcher. Applying this argument to our study context is somewhat challenging: we do not interpret the biodiversity-GDP elasticities as the causal effect of a ceteris paribus increase in GDP. That is, we do not believe that GDP itself directly influences biodiversity. GDP is an accounting concept that varies only because of changes in real economic activity. In other words, one cannot randomly assign GDP while holding everything else constant. Instead, our estimates capture the total effect of various determinants of biodiversity—such as environmental pollution or habitat loss due to changing land use—that underlie GDP changes. This makes it somewhat unclear which characteristics should be included in the correlates test. For example, we could include industry composition or land use, but these factors may themselves be channels driving the biodiversity-GDP relationship.

That said, we run the diagnostic tests in the style of Goldsmith-Pinkham et al. (2020). We examine the correlation between the military spending-to-GDP share during the base period (1966–1971) and various characteristics from the same time frame: state-level population; sector-specific income from manufacturing, mining, timber and logging, agriculture, construction, and services; total public road mileage (to capture industry composition and stages of urban development, as you alluded to in your comment); and Conservation Reserve Program (CRP) enrollment status and total protected area sizes (to capture habitat-related factors). The regression results are presented in Table A.20 below. We generally do not find a significant relationship between military spending share and any of these individual characteristics, with the only exception being the presence of protected areas which shows marginal significance and small effect size.

Second, parallel trends. Note that in our setting there is no pre-period and so it is not possible to test for parallel pretrends.

Third, alternative estimators and overidentification tests. Appendix Table A.12 reports a series of alternative estimators 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. For succinctness, we only report biodiversity-GDP elasticity estimates for these robustness checks. Overall, the results are robust to these specification changes.

We also use both the main IV specification and an alternative IV approach as described in (b) above to conduct overidentification tests. To address potential weak instrument concerns, we use both 2SLS and LIML estimators. The results in Table A.21 show that all overidentification tests fail to reject the null hypothesis.

Table A.20: Relationship Between Military Spending Shares and Site Characteristics

	(1) Military Spending Share (in percentage)
Population (in 1000k)	-0.055 (0.177)
Manufacturing	-1.128 (0.845)
Mining	-0.427 (0.359)
Timber and Logging	-0.030 (0.344)
Agriculture	-0.316 (0.544)
Construction	1.972 (2.866)
Service	0.389 (2.759)
Road Miles	0.520 (0.609)
Protected Area	-0.001* (0.001)
Observations	1,804
R^2	0.1132

Notes: The table corresponds to a regression that looks at the relationship between the military spending share and sampling site characteristics in base year (1966-1971) as in the military spending IV. Standard errors are clustered at the state level. We use state population of the sampling site, logarithm of sector-specific income from manufacturing, mining, timber and logging, agriculture, construction, and services. Sector income data are from U.S. Bureau of Economic Analysis, categorized based on 2-digit SIC and NAICS codes. Agriculture includes farming, fishing, and hunting. Services includes wholesale, retail, transportation, communications, electric, gas, and sanitary services, finance, and all other service. We use state total public road miles (in logs) and total protected area sizes to capture changes in habitat of the sampling sites.

Table A.21: Overidentification Tests

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
TSLS	-4.509***	-2.531**	-0.434***	-6.724***	-3.575***	-0.551***
	(1.388)	(0.983)	(0.155)	(0.836)	(0.506)	(0.082)
[Overid P-value]	0.967	0.569	0.278	0.250	0.153	0.191
LIML	-4.509***	-2.536**	-0.435***	-6.771***	-3.629***	-0.552***
	(1.388)	(0.986)	(0.155)	(0.823)	(0.494)	(0.082)
[Overid P-value]	0.967	0.569	0.278	0.252	0.157	0.191

Notes: This table reports the implied second-stage biodiversity-GDP elasticity estimates using military spending shocks as the underlying source of policy variation. Overidentification test results are shown for both TSLS and LIML using p-values of Sargan chi-squared test. Standard errors are clustered at the state level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Robustness Tests of the Air Pollution IV

LASSO model selection This analysis demonstrates that our algorithm does not systematically select counties with similar attributes (e.g., industry sectoral income, land use patterns, pollution-generating activities, or population levels).

We assess whether the attributes of LASSO-selected upwind counties are more similar to those of the destination county than those of unselected counties. Specifically, we examine: (1) The shares of sector-specific income relative to total income for industries categorized under 2-digit SIC and NAICS codes (e.g., manufacturing, mining, logging, agriculture, construction, and services). (2) Land use patterns: the percentage of urban land in a county based on MODIS land cover types. (3) Population levels sourced from the Census. (4) Total toxic releases from the Toxic Release Inventory (TRI).

Specifically, we estimate the following cross-sectional regressions:

$$dest_c = \beta \cdot src_c \times I(selected)_c + \gamma \cdot src_c + \alpha \cdot I(selected)_c + \eta_{dest\_src} + \epsilon_c.$$

where $dest_c$ refers to one particular characteristic of destination county c. src_c refers to the corresponding characteristic for potential source counties (all counties). The indicator $I(selected)_c$ equals to 1 if county c is selected by the LASSO algorithm and 0 otherwise. Destination-source state pair fixed effects (η_{dest_src}) are included. Standard errors are clustered at the source county level.

The coefficient of interest, β, measures whether the LASSO-selected counties are more strongly correlated with the destination county than unselected counties. Results, presented in Table A.22 below, indicate no significant difference in these correlations across any of the examined attributes between the LASSO-selected and unselected counties.

Alternative Wind Instrument We attempt to implement the approach used in Deryugina et al. (2019), which follows a more general design that predicts variation in local pollution as a function of changes in wind direction. We show that our main findings are robust to this alternative wind instrument.

We estimate the following first-stage model following Deryugina et al. (2019):

$$Pollution_{ct} = \sum_{b=0}^{7} \beta_b \times WINDDIR_{ct}^{45b} + X'_{ct}\sigma + \eta_{cj} + \eta_t + \epsilon_{cjt},$$

where $Pollution_{ct}$ represents the annual logged pollution level in county c, as defined in the baseline specification. Each variable in $WINDDIR_{ct}^{45b}$ is equal to the number of days that the wind direction in county c falls within the 45-degree interval $[45b, 45b+45)$ in year t. with the interval $[0, 45)$ serving as the omitted category. Weather controls, X'_{ct}, such as county annual average maximum and minimum temperatures, and precipitation, are included. The other variables are defined as in the baseline regression.

To illustrate the first stage results, we replicate Figure 2 in Deryugina et al. (2019), which visualizes the relationship between daily average wind direction and pollution for counties near the Bay Area. Since few studies in our dataset are conducted near the Bay Area, we extend the analysis to all studies in California, yielding 700 observations (location-by-taxa-by-year). Panel (a) in Figure A.13 exhibits a pattern similar to Panel (b), which reproduces Figure 2 from Deryugina et al. (2019). This comparison suggests that while the annual wind direction bin IV behaves similarly to the daily wind direction IV, it has lower precision. The key difference in our context is the coarser temporal variation: Deryugina et al. (2019) leverage changes in daily wind direction at a given location, which introduces significant idiosyncratic variation. However, wind patterns become much less idiosyncratic when analyzed at the annual frequency, which likely reduces the effectiveness of this approach in our setting.

Table A.23 below presents the second-stage estimation results. We find that pollution reduces biodiversity, with estimated effects comparable to those obtained using our preferred LASSO instrument specification. However, the wind direction bin IV is a weaker instrument, as indicated by a Kleibergen-Paap F-statistic of approximately 20 (or 4 for the non-bird sample), compared to values exceeding 200 for the LASSO instrument. These results highlight the loss of first-stage strength when using annual wind direction bins. Although our main findings remain consistent, the reduced precision reinforces our preference for the LASSO instrument as the primary identification strategy.

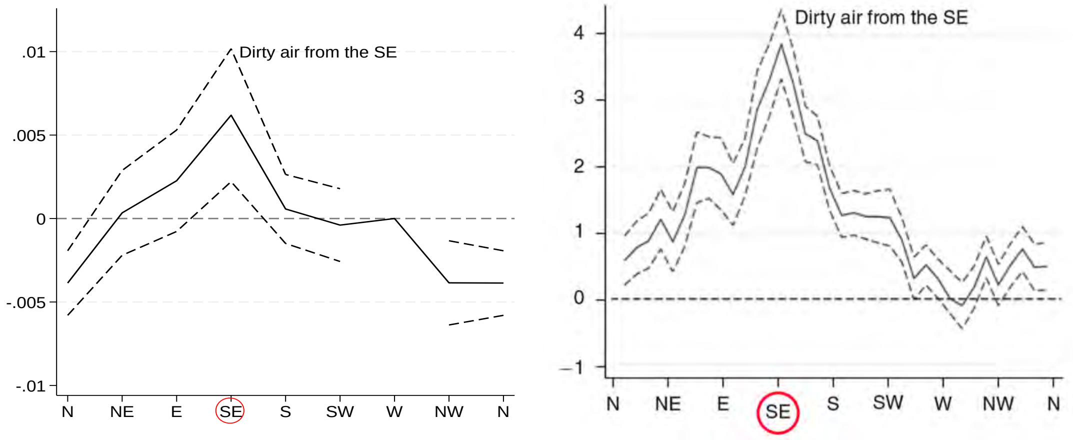
This is in fact a good opportunity to think through how our approach relates to, yet differs from, the strategy used by Deryugina et al. (2019) and why our approach yields more predictive power. First, both strategies inherently exploit pollution transport. The Deryugina et al. (2019) approach is entirely agnostic about the origin of pollution transport—it simply documents that different wind directions are associated with varying pollution levels. In contrast, our approach explicitly models source-to-destination dynamics, using national wind data to identify upwind counties and employing LASSO to pinpoint which of those upwind counties contribute most to local pollution.

Table A.22: Comparison of County Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Industry Sectoral Income						
	Destination share of sectoral income:					
	sector = Manufacturing	Mining	Logging	Agriculture	Construction	Service
source_*s*_share × I(selected)	-1.94e-04	1.16e-04	-7.64e-06	9.44e-06	-1.15e-03	-1.12e-03
	(2.52e-04)	(4.74e-04)	(6.98e-06)	(6.51e-06)	(1.62e-03)	(2.37e-03)
source_*s*_share	-2.31e-08	-2.52e-05	5.91e-08	1.34e-06	1.20e-05	1.40e-05
	(1.25e-05)	(3.28e-05)	(2.80e-07)	(5.81e-07)	(5.33e-05)	(6.11e-05)
I(selected)	5.70e-05*	3.50e-05***	1.43e-08***	7.51e-08	1.20e-04	1.04e-03
	(2.95e-05)	(1.20e-04)	(4.25e-09)	(9.41e-08)	(7.94e-05)	(1.04e-03)
Panel B. Land Use Patterns, Population, Pollution Activities						
	Destination county:					
	x= % of Urban land		Population		Toxic releases	
source_*x* × I(selected)	-.005		-0.006		0.009	
	(.018)		(0.006)		(0.008)	
source_*x*	3.51e-05		6.13e-05		-0.0001	
	(1.8e-04)		(7.95e-05)		(0.0001)	
I(selected)	-8.49e-04		0.04		-0.13	
	(7.05e-04)		(0.06)		(0.10)	

Notes: Each column presents a result from a separate regression incorporating different county characteristics. Panel A compares the share of sector-specific income relative to total income between destination and source counties. The industry sector varies across columns according to s. For instance, Column 1 regresses the share of manufacturing income relative to total income in destination counties on the corresponding share in source counties. Industry classifications follow the 2-digit SIC and NAICS codes. Sector income data are sourced from the U.S. Bureau of Economic Analysis (1969–2016). Panel B examines differences in three county characteristics: the percentage of urban area (calculated using MODIS land cover type data), the log of county population from the Census, and the log of total toxic releases reported under the TRI Program. Specifically, the regression is $dest_c = \beta \cdot src_c \times I(selected)_c + \gamma \cdot src_c + \alpha \cdot I(selected)_c + \eta_{dest-src} + \varepsilon_c$, where $dest_c$ represents a given characteristic of the destination county c. src_c refers to the corresponding characteristics of all source counties, as all counties are considered potential source counties. The indicator $I(selected)_c$ equals to 1 if county c is selected by the LASSO algorithm and 0 otherwise. Destination-source state pair fixed effects are included. Standard errors are clustered at the source county level. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.

Figure A.13: Wind Direction and Pollution



(a) Wind Direction IV

(b) Figure 2 from Deryugina et al. (2019)

Notes: This figure presents a comparative analysis of the relationship between wind direction and air pollution, aligning our study of annual wind direction with the daily wind direction analysis conducted in Deryugina et al. (2019). The left panel focuses on data from our study, covering the entire state of California. It displays regression estimates from the first-stage equation, where the dependent variable is the county's annual log-transformed Aerosol Optical Depth (AOD) pollution level, and the key independent variables represent the number of days per year that a county's wind direction falls within a specific 45-degree bin. The model includes controls for location-by-taxa interactions, year fixed effects, and annual average maximum and minimum temperatures and precipitation. The dashed lines indicate 95% confidence intervals, calculated using robust standard errors. The right panel reproduces Figure 2 from Deryugina et al. (2019), which examines the relationship between daily average wind direction and PM 2.5 concentrations in counties within and around the San Francisco Bay Area, California.

Table A.23: Replication of Biodiversity-Pollution 2SLS Estimates using Deryugina et al. (2019)

	(1) Abundance	(2) Richness	(3) Similarity	(4) Abundance	(5) Richness	(6) Similarity
	Panel A. All species			Panel B. Non-bird species		
$\widehat{\text{Pollution}}$ (IV)	-2.629***	-1.437***	-0.177**	-5.202***	-2.304***	-0.676***
	(0.950)	(0.267)	(0.080)	(1.159)	(0.715)	(0.194)
TMAX	-0.019	-0.001	-0.016***	0.081	0.075	-0.072***
	(0.034)	(0.032)	(0.003)	(0.105)	(0.045)	(0.009)
TMIN	0.104*	0.044*	0.021***	0.213***	0.048**	0.076***
	(0.052)	(0.024)	(0.005)	(0.043)	(0.021)	(0.014)
PRCP	-0.061*	-0.023	-0.023**	-0.065	0.023	-0.052***
	(0.034)	(0.016)	(0.011)	(0.046)	(0.026)	(0.013)
Kleibergen-Paap F-stat.	20.1	20.1	11.5	3.9	3.9	2.952
Observations	52,860	52,860	40,422	12,726	12,726	11,599

Notes: This table reports 2SLS estimates replicating Deryugina et al (2019). 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 instrumental variables are the number of days that the wind direction in a county falls in each of the 45-degree bin in a year. The Kleibergen-Paap F-statistics of the first stages are reported at the bottom of the table. Columns 1-3 report 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. *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$.