National Parks and Economic Development*

Andrea Szabó and Gergely Ujhelyi†

Economics Department, University of Houston

December 10, 2023

Abstract

This paper studies the economic effects of the US National Park System, the largest national conservation entity in the world. We assemble a new dataset on the history of the system, and show that parks increase overall employment and income in the local economy. The data allows us to study several specific mechanisms. Economic effects appear to be driven by visitors, and they cannot be explained by direct government spending on park budgets or by various substitution effects. Our findings provide evidence relevant to conservation policy in the US and elsewhere.

*Partially supersedes "Economic Impacts of the US National Park System." We thank Eyal Frank, Michael Greenstone, John Loomis, Kate Sims, Liyang Sun, Katherine Wagner, our colleagues at the University of Houston, and seminar participants at the University of Chicago Harris School, University of Texas Rio Grande Valley, and the NBER Environmental Economics Summer Institute for useful comments and discussions. Thanks to Daniel Jacobs, Maggie Lam, Alice Phan, Tasneem Sultana, and Dingyi Xu for research assistance.

†E-mail: aszabo2@uh.edu, gujhelyi@uh.edu

"Conservation means development as much as it does protection." *Theodore Roosevelt*

1 Introduction

US government policy on the management and conservation of public lands has experienced several large shifts over the past decade. The Obama administration created a record number of protected areas but deferred maintenance on many existing parks in the National Park System.[1] Under the Trump administration, protections were lifted from millions of acres of federal land in Utah and Alaska but four new national parks were established, and the Great American Outdoors Act substantially increased funding for the park system. In 2021, the Biden administration announced a goal of conserving 30 percent of US lands and waters by 2030.

One common theme in these policy changes, whether moving toward or away from conservation, is an emphasis on economic impacts. For example, the current administration believes that the 30 by 30 directive "will not only protect our lands and waters but also boost our economy and support jobs nationwide."[2] This is remarkable given that, in contrast to other place-based government programs such as infrastructure investments or local employment subsidies, conservation programs have traditionally not been designed to boost economic development. Instead, their primary goal has been the preservation of public lands as a public good. In essence, the more recent conservation policy is based on the premise that investment in these public goods can also deliver private economic benefits, in the form of job creation and increased income. In this paper, we estimate such economic impacts in the context of a particularly important government program.

We study the impact of the US National Park System (NPS) on local employment and income. The NPS is the largest and best known national conservation entity in the world. Established in 1916, its mission is twofold: to conserve certain areas, and to make them accessible to the public in a sustainable manner.[3] The NPS currently includes over 400 parks, has a budget over $4 billion, and receives more than 300 million visitors annually. Over 100 countries have national parks explicitly modeled after the NPS - understanding the local economic impacts of parks may thus be informative to some of these countries as well.

[1] https://www.wsj.com/articles/national-parks-lost-in-the-wilds-of-neglect-1461531553

[2] https://www.doi.gov/sites/doi.gov/files/report-conserving-and-restoring-america-the-beautiful-2021.pdf, p6.

[3] "[Its] fundamental purpose is to conserve the scenery and the natural and historic objects and the wild life therein and to provide for the enjoyment of the same in such manner and by such means as will leave them unimpaired for the enjoyment of future generations." (16 U.S.C. 1, 1916)

We collected and digitized what is, we believe, the most comprehensive dataset on the history of the NPS. The dataset follows parks backward in time, and contains information on administrative histories (e.g., name and boundary changes) as well as annual visitors, budgets, and size (acreage), creating a park-level panel. We combine this with county-level economic variables in order to estimate the NPS's impact on local employment and income using event study specifications.

Our first exercise estimates the impact of National Parks (NP). To do this, we distinguish between NP's and other parks in the system. Often referred to as the "crown jewels," NP's are the best-known and most visible parks within the NPS.[4] During the past 50 years, almost all NP's were established by upgrading an already-existing park. Thus, in this exercise we estimate the impact of conferring the NP designation onto a park that is already part of the NPS. This can be interpreted as a treatment on the "intensive margin."

We find that NP designation increases employment and incomes in the local economy. After a gradual increase, employment rises to 4% above its initial level 4 years after the designation change. The implied increase in jobs in the average county is 2,100 one year after the change, and 6,100 four years after. Similar to employment, income also shows a gradual increase, to approximately 6% above its pre-change level 3-4 years after the designation change.

Our second exercise estimates the opening of a park, i.e., the inclusion of a new area in the NPS. This represents a treatment on the "extensive margin," and we find that it yields qualitatively similar effects to the first exercise. New parks lead to approximately 4% higher employment and 5% higher incomes by year 4 following the treatment. This is particularly noteworthy given that newly opened parks during our period of study are *not* National Parks. They tend to be relatively smaller, and their importance often lies in their historical, as opposed to natural, significance. Our results show that these parks, too, are conducive to local economic development.

The dataset we collected allows us to study several specific mechanisms behind these findings. The evidence shows that direct government spending on or around the parks cannot account for these large income and employment effects. The results also cannot be explained by the diversion of economic activity from neighboring counties or from other parks, nor by changes simply in the size of the conserved areas. Instead, the evidence is consistent with economic impacts being driven by visitors. Comparing different designations suggests that increased visitation is a necessary condition for the increase in local employment and income.

Our main analysis focuses on a five-year period after each treatment. Extending the

[4] We refer to all areas of the NPS as "parks," and reserve the term National Park to parks that carry this designation.

analysis further in time increases the uncertainty of our estimates, and we cannot rule out that the employment impact of parks starts dissipating after about a decade. Effects on income appear more sustained, particularly for the opening of new parks.

Likely due to a lack of data, the existing literature on the NPS is remarkably small. It includes willingness-to-pay surveys (Haefele et al., 2020) and economic contribution estimates from an input-output model published regularly by the National Park Service (e.g., Thomas et al., 2018). Weiler (2006) finds that national park designation increases visitation in a sample of 8 parks but does not control for year fixed effects, and he does not discuss the resulting economic impacts. Kotchen and Wagner (2023) show that larger park budgets increase volunteering.

The broader literature on US conservation programs mostly studies regional policies affecting a limited number of states (e.g., Rasker et al. (2013), Chen et al. (2016), Sims et al. (2019), Jakus and Akhundjanov (2019), Walls et al. (2020)). These policies are often very different from the NPS, which, together with the different empirical methods used, makes it difficult to extrapolate the findings. We compare the magnitude of our estimates to different programs, as well as to the NPS's "official" estimates in Section 4.3 below. A parallel literature studies conservation programs in developing countries (e.g., Sims (2010), Ferraro and Hanauer (2014), Sims and Alix-Garcia (2017)).[5]

Our paper differs from the existing conservation literature in several ways. First, we introduce a new dataset on one of the most important conservation programs in the world, and we provide the first analysis of the economic impacts of this program using quasi-experimental methods.

Second, we analyze several explicit mechanisms behind the impact of parks. This helps illuminate why the NPS has a clear positive effect on employment and income, in contrast to some of the other conservation programs studied in the literature.

Third, we address specific methodological issues that often jeopardize the causal interpretation of estimates in this literature. Our estimation strategy fully accounts for the clustering of treatment, an issue that always arises when a conservation area affects multiple units (counties) simultaneously, but that is ignored in most existing studies; we estimate dynamic treatment effects, which accounts for the fact that the impact of parks is likely to vary over time; finally, we incorporate some of the recent advances addressing the bias of difference-in-differences estimates in the presence of treatment effect heterogeneity.

[5] Mirroring the conservation literature, there is a large literature on the economic effects of natural resource *extraction* and the "resource curse" (see Allcott and Keniston (2018) and the studies cited therein). Taken together, these literatures highlight the fact that natural resources are not generally "good" or "bad" for the economy: the details of exactly what is being extracted or conserved, and how, matters. In this vein, our paper shows the economic benefits of the conservation of public lands *when coupled with visitation*.

It is important to note that there are many dimensions of the NPS that our paper is not designed to study. First, we do not attempt to measure "conservation." This is a concept that the Park Service itself struggles to define in an era when the natural environment is changing rapidly (Schuurman et al., 2020). We simply take it as given that conservation in the NPS is by law (i.e., the limitations on extractive activities). Second, we do not attempt to provide an overall welfare assessment of the NPS as this would require measuring various externalities, general equilibrium effects, as well as distributional impacts (including social justice issues related to the treatment of native populations (Spence, 1999)). We investigate some of these issues in on-going work (Szabó and Ujhelyi, 2023).

2 Background and data

2.1 Background

Today the NPS is a collection of over 400 areas managed by the National Park Service, a federal agency established in 1916. Most areas ("parks" from now on) are large natural areas, but many also have historical significance, containing, e.g., Native American dwellings, Civil War battlefields, or prehistoric fossils. The mission of the Park Service is twofold: first, to conserve the parks, and second, to make them accessible to the public in a way that does not jeopardize the first goal (NPS Organic Act, 1916, 16 U.S.C. 1).

Parks in the NPS have a variety of titles or "designations" (Table A.1). Designations are suggestive of a park's primary purpose and attraction, but they have no clear definition (Rose, 2017). The flagship parks of the system are the National Parks. Lesser natural areas have designations such as National Preserve or National Seashore; parks that are primarily of historic importance carry designations like National Historic Park (typically considered to be the most prestigious designation in this category), National Historic Site, or National Battlefield. All these parks are managed by the same agency, listed in the same publications, and distinguished by the NPS "brand."[6]

Parks in the NPS "are among the most strictly protected federal lands, as compared to those administered by other agencies such as the Bureau of Land Management or the Forest Service" (Congressional Research Service, 2022b, p6). Specific rules are set on a park-by-park basis, but in general activities that may consume or damage resources are limited - this includes both commercial activities (grazing, mining, logging) and recreational activities (e.g., hunting or off-road vehicles). For example, commercial logging is prohibited in all

[6]Compared to areas managed by other agencies, the NPS has a number of well-recognizable features: uniformed park rangers, visitor centers offering interpretative programs, annual passes valid in all NPS parks, etc.

parks in the NPS, while it is typically allowed in national forests administered by the US Forest Service. Importantly, most parks in the NPS restrict vehicle traffic on roads located inside the park, which indirectly creates restrictions on all users (including commuter traffic and transportation).

Among parks in the NPS, National Parks have the most restrictions. For example, hunting and off-road vehicle use are prohibited in all NPs, while they are allowed in other NPS areas such as National Recreation Areas and National Seashores. Vehicle traffic is more restricted in NP's than in other parks.[7] In addition, violating restrictions in NP's carries higher fines than in other units (Congressional Research Service, 2022b).

Although there can be variations, creating a new park generally involves the following steps. 1. Someone makes a proposal to the Park Service or directly to a member of Congress. 2. Congress requests the Park Service to undertake an area study in order to assess the national significance of the proposed area (such as its unique natural or historic features) and the feasibility of creating a park. 3. The Park Service completes the study and makes a recommendation to Congress. 4. Congress passes legislation creating the park. 5. The president signs the legislation. 6. Congress appropriates the funds necessary to create and operate the new park.[8] The process of changing a park's designation to National Park involves similar steps.

In practice, many areas clear the first step because virtually anyone can propose a park, and many US counties have places that could be proposed. For example, out of California's 58 counties at least 46 (79%) had a park proposed for inclusion in the NPS at some point, with proposals coming from "local chambers of commerce, editors, scientists, tourists, and neighbors" (Dilsaver, 2008, p17). However, even among the proposals that clear the first few steps, most ultimately fail. For example, of the area studies undertaken by the Park Service since 2000, between 75-80% have concluded that the area is not recommended for inclusion in the NPS (Congressional Research Service, 2022a).

Even for parks that are ultimately successful, the process typically takes decades. For example, Purcell (2019, pp221-224) describes the history of designating Arches NP. Soon after Arches National Monument was established in 1929, local interests began pushing for redesignation to NP hoping that this would attract development to the town of Moab. After failed attempts in the 1930s, redesignation was revisited in 1948 without success. Bills on NP

[7] For example, local residents have long opposed redesignating Colorado National Monument to NP for fear that this would lead to further traffic restrictions on roads passing through the park (https://www.nytimes.com/2012/06/17/us/debating-if-colorado-national-monument-should-be-a-national-park.html).

[8] One exception to this process is that the president has discretionary power to establish National Monuments (another common designation in the NPS) under the Antiquities Act of 1906.

designation failed in Congress in 1961, 1962, and 1963, reportedly because the Park Service asked for more time to complete the area study. The legislative push that eventually led to NP designation in 1971 began in 1969 but was delayed because of disagreements about the park's boundaries.

One major source of delay is the administrative process itself as the Park Service must deal with the high volume of area studies requested and the limited resources available for their funding. In response to these delays, in 1998 Congress passed legislation requiring the Park Service to complete area studies within three fiscal years (Congressional Research Service, 2022a).

Because park openings and designation changes take time and involve multiple decision makers, they are unlikely to be caused by short-run changes in the local economy. The idea that these events are difficult to predict based on contemporaneous information (on which we offer empirical evidence below) is well illustrated by the seminal book on the NPS by Rettie (1995), a senior career bureaucrat who spent decades at the Park Service and other land-management agencies in the federal government. In the book, Rettie predicts that Death Valley National Monument would not become a National Park "in the foreseeable future" (p44). In fact, the park acquired NP designation as the book was being printed, in 1994.

Relative to the unpredictable process of a park's establishment, once the change takes place, the uncertainty regarding the area's status all but disappears. Once a park becomes part of the NPS, it will almost always remain in the NPS permanently: during our period of study, only one park was moved out of the NPS. NP designation offers a similar long-term guarantee: no National Park has been moved out of the NPS since 1940, and only one National Park was downgraded to a lesser designation (Platt NP, established in 1906, became Chickasaw National Recreation Area in 1976).

2.2 Data and sample

We collected and digitized what is, to our knowledge, the most complete dataset on the history of each unit of the NPS. We followed parks backward in time, gathering information on the number of visitors, annual park budgets allocated by the federal government, acreage, and administrative histories (name changes, park mergers, boundary changes, etc.) in order to create a park-level panel. Assembling the dataset required digitizing information from multiple archival sources, and keeping track of the definitions of different variables over time. The construction of the dataset is documented in the separate Online Data Appendix.

Our level of observation is a park-year. As of 2017, the Park Service reported visitors in 379 parks. We exclude parks in Alaska, Hawaii, and US territories, for which the economic

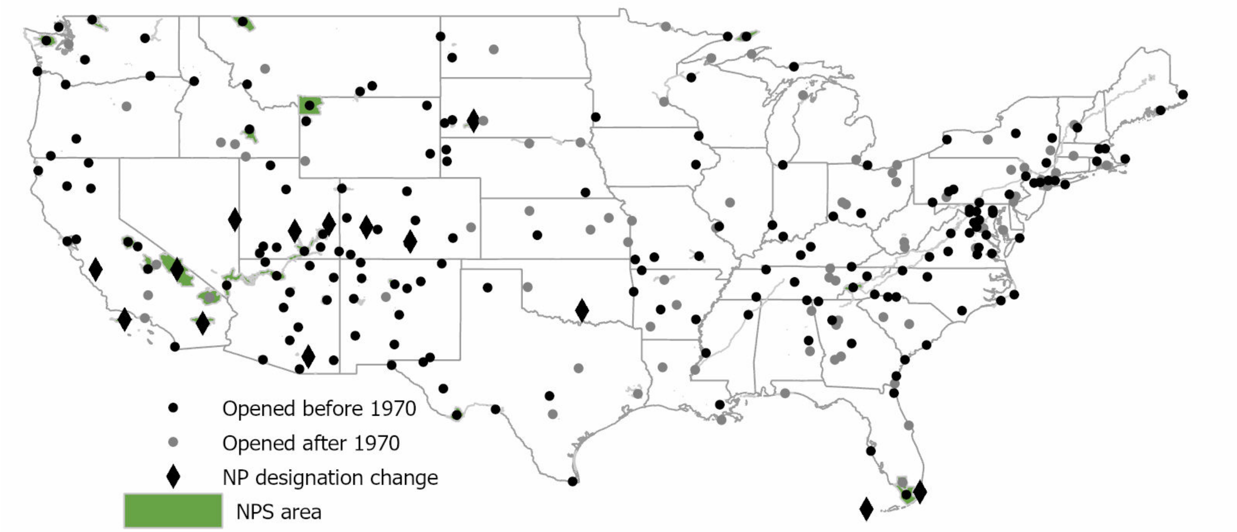

Figure 1: Parks in the sample

Markers indicate the centroid of each park. When a park's geographic footprint is larger than the marker, the park's area is shaded in green. National Parks with a designation change are Arches, Badlands, Biscayne, Black Canyon of the Gunnison, Capitol Reef, Platt (Chickasaw NRA), Channel Islands, Death Valley, Dry Tortugas, Great Basin, Great Sand Dunes, Joshua Tree, Pinnacles, Saguaro.

data is often missing. We also exclude units (mostly museums) located in large metropolitan areas because their contribution to the local economy is likely to be negligible. In some cases, a park is a collection of multiple units (typically close to each-other) that are treated administratively as a group by the Park Service. If any parks were treated as a group for some of our main sample period (1970-2017), we combine their data for the entire period and treat them as one park. We similarly treat as one combined park parks that are located in the exact same counties (in every case, this is a single county).[9] After these changes, we have 269 parks, located throughout the country (Figure 1). Of these, 189 parks were established before 1970, the start date of our economic variables.

Figure 2 shows the evolution of the park system over time. The left panel shows the total number of parks. Our sample period starts after the sharp increase in the number of parks around 1966 (the 50th anniversary of the Park Service) known as the "Mission 66" project.

[9]A combined park is coded as a NP in a given year if at least one of its units is a NP. Only one combined park received NP designation during our period of study.

The right panel shows the number of National Parks over time, separating parks that were created as National Parks, and parks that were redesignated as National Parks. The period after World War II saw a long pause in the establishment of these parks. Subsequently, National Parks were established through redesignation in all cases but one.

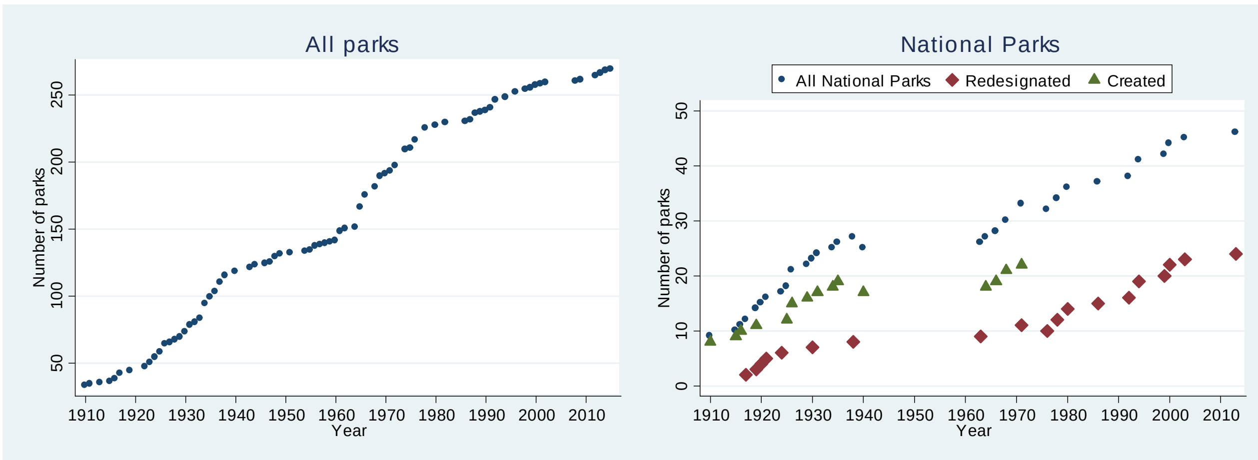

Figure 2: Evolution of the number of parks over time

Cumulative number of parks over time in the sample. The left panel shows all parks in the NPS, the right panel shows National Parks only. Each marker indicates a change in the number of parks; years with no markers had no change. The right panel shows separately the (cumulative) number of National Parks created as a National Park or redesignated from an existing park.

Over the period 1970-2017, the median park had 234,843 annual visitors and a budget of $693,113 per year (Table A.2). National Parks received more visitors and had larger budgets: their median is over 3 times the overall median for both variables. National Parks also tend to be larger and older than others. Between 1975 and 2015, the number of visitors in the average park increased by 6.5% while the number of parks increased by 28%. The average park's budget grew by 38.5% in real dollars over this period.

2.3 Defining the treatments

Our analysis focuses on two types of events: NP designation and the opening of a new park.

NP designation. First, to study the impact of National Parks, we estimate the impact of NP designation for a park that is already part of the NPS. NP designation places a park among the most famous ones in the system but also comes with the most restrictions on permitted use. For these estimates, the comparison group will consist of all parks in the NPS that are not receiving NP designation in the given year. This offers a clear counterfactual:

in the absence of NP designation, the park would still be part of the NPS - for example, it would not undergo commercial development.

There are 46 National Parks in our dataset, and the majority of these were created through the redesignation of an existing park (Figure 2). After 1970, all National Parks but one were created through redesignation (17 parks), and one park lost NP designation. Three of these changes involve parks established after 1970, which we drop from this analysis. We drop an additional park, Theodore Roosevelt NP, where a major oil discovery adjacent to the park in the year of the redesignation would bias our estimates of economic impacts *upward* (Figure A.10). Our identification will thus come from designation changes in 14 National Parks, indicated on Figure 1.

We code designation changes as happening in the year when the president signs the bill redesignating the park. Although this typically occurs in the Fall, Congress will have passed these bills earlier in the year. Economic decisions in response to a redesignation could be made throughout the year of the treatment as redesignation becomes all but guaranteed.

Park opening. Our second treatment is the inclusion of a new park in the NPS (which we refer to simply as "park opening"). This helps quantify the economic impacts of conservation compared to a counterfactual without conservation. In this sense, the opening of a new park represents an increase in conservation on the "extensive margin," while NP designation increases conservation on the "intensive margin."

The park opening event typically involves transferring federal land to the Park Service from another agency, and authorizing the Park Service to begin spending money on developing the park. In practice, openings range from mere formalities to the start of actual construction of park facilities. At the same time, there are a number of elements common to all new parks as they are managed by the same agency and placed among the most protected federal lands. Most importantly, because parks can be expected to remain in the NPS indefinitely, a park's opening represents a commitment on the part of the federal government regarding the future management of the area. To exclude events that were mere formalities, we drop parks that do not begin reporting visitors within 5 years after opening (we vary this threshold to check robustness).

For this treatment, we extend the sample to parks included in the NPS after 1970 (i.e., we consider all parks on Figure 1) and also include counties without any parks (subject to various restrictions - see Section 3.5 in the Online Data Appendix[10]). The comparison group for our estimates are areas that do not experience a park opening in a given period. This

[10] We exclude counties that contain large metropolitan areas. Similarly to parks, we follow counties backward in time. Counties that share a park in 2017 are aggregated into one observation in each year of the sample. Counties that never have a park enter the data individually.

includes areas that will receive a park later in the sample period, areas that received a park earlier, and areas that will not receive a park before 2017.

Recall that our sample starts four years after Mission 66, an intense period of park construction around the 50th anniversary of the Park Service. Of the parks opened between 1966-2017, over 40% opened in the period 1966-74 (Figure 2). Because economic variables start in 1970, these parks do not have all their lags and leads observed in a $[-5, +5]$ event window, resulting in large imbalances in the observations used to identify the different coefficients. To avoid this, we focus on the impact of park openings that occur between 1975 and 2013 (31 opening events). In robustness checks, we expand this set and find similar results.

2.4 Other data

We merge a number of different county-level datasets to our NPS data using GIS boundary files. Employment and income come from the Bureau of Economic Analysis. Our measure of income is earnings by place of work, which consists of compensation of employees and proprietors' income generated in the given county. This is consistent with the employment measure, which is also calculated on a place-of-work basis. Summary statistics are in Table A.2. Population age groups come from the Census Bureau. Weather information is from the National Climatic Data Center of the U.S. Department of Commerce. Finally, one of our robustness checks adds data on local government spending from the Census of Governments database of the Census Bureau. This is collected every 5 years and is available in consistent format for 1972-2012. All monetary values are transformed to real 1982-84 dollars using a consumer price index from the Bureau of Labor Statistics.

2.5 Sample characteristics

Table 1 presents average sample characteristics for 1970, the start of our sample period. Values are shown separately for two groups of units: those that are treated during our sample period ("to-be-treated") and those are not treated before the end of the sample ("never-treated"). Panel A is for the intensive-margin treatment (NP designation), Panel B for the extensive-margin treatment (park opening).

Table 1: Characteristics of never-treated and to-be-treated units

	Never-treated	To-be-treated	p-value of difference	N
Panel A: NP designation sample				
Pop. density (log)	-3.995	-5.054	0.085	158
Kids	0.392	0.393	0.940	158
Old	0.102	0.096	0.592	158
Precipitation	2.775	1.710	0.006	158
Droughts	0.030	-0.114	0.650	158
County land area	5.296	13.033	0.001	158
National Monument	0.379	1.000	0.000	158
Nature park	0.317	1.000	0.000	158
Park age	30.455	37.692	0.238	158
Park acreage (log)	7.012	11.133	0.000	155
Visitors (log)	5.296	5.246	0.914	138
Employment (log)	9.857	10.114	0.571	158
Income (log)	12.660	12.989	0.501	158
Δ employment 1969-70	0.009	0.013	0.763	158
Δ income 1969-70	0.013	0.023	0.616	158
Δ visitors 1969-70	0.107	0.126	0.769	136
Panel B: Park opening sample				
Pop. density (log)	-4.463	-2.893	0.000	2521
Kids	0.383	0.383	0.985	2521
Old	0.122	0.104	0.011	2521
Precipitation	3.050	3.061	0.956	2520
Droughts	0.202	-0.136	0.063	2520
County land area	2.158	3.962	0.000	2521
Employment (log)	8.924	10.935	0.000	2521
Income (log)	11.601	13.825	0.000	2521
Δ employment 1970-74	0.095	0.102	0.975	2521
Δ income 1970-74	0.178	0.130	0.843	2521

Notes: Entries in columns 1 and 2 are mean values and column 3 shows the p-value for a t-test of equality. In Panel A, to-be-treated units are the 13 parks receiving NP designation during our sample period (Platt NP, which loses its NP designation, is excluded from this table for ease of interpretation), never-treated units are all other parks in the sample that are not NP in 1970. In Panel B, to-be-treated units are the 31 areas with a park opening during our sample period, and never-treated units are all other areas in the sample without a park in 1970. In each panel, the last rows show average changes in outcomes before the first treatment in the sample occurs.

In both cases, there are several level differences between never-treated and to-be-treated units. In Panel A, parks that are eventually redesignated to NP are in counties with lower population density and less precipitation, and they are also larger than parks that are never-treated. This is not surprising as we already saw in Section 2.2 that, e.g., National Parks tended to be larger than others. Treated parks are all nature parks, and they are all National Monuments in 1970. In Panel B, areas that eventually see a park opening have higher population density, lower share of elderly, fewer droughts, and higher income and employment than areas that are never-treated. These observations imply that it will be important to control for level differences between different areas, and the fixed effects in our specification will control for *all* such differences. In addition, we will also perform robustness checks where we compare more similar groups, such as restricting the NP designation exercise to nature parks or National Monuments, and using propensity scores to trim the sample in the park opening regressions.

In contrast to these level differences, *changes* in outcomes are very similar, both economically and statistically, between to-be-treated and never treated units before treatment begins. In Panel A, changes in employment, income, and visitors, and in Panel B, changes in employment and income, are similar in magnitude and not statistically different between the two groups. Figure 3 provides a similar comparisons *within* the group of treated units. These figures present scatterplots of the year of treatment and the change in income and employment, with a corresponding linear fit. There is no evidence of a significant association between year of treatment and changes in outcomes before the first treatment in the sample. Note that the associations are slightly positive: if anything, units experiencing faster growth in employment or income are treated later.

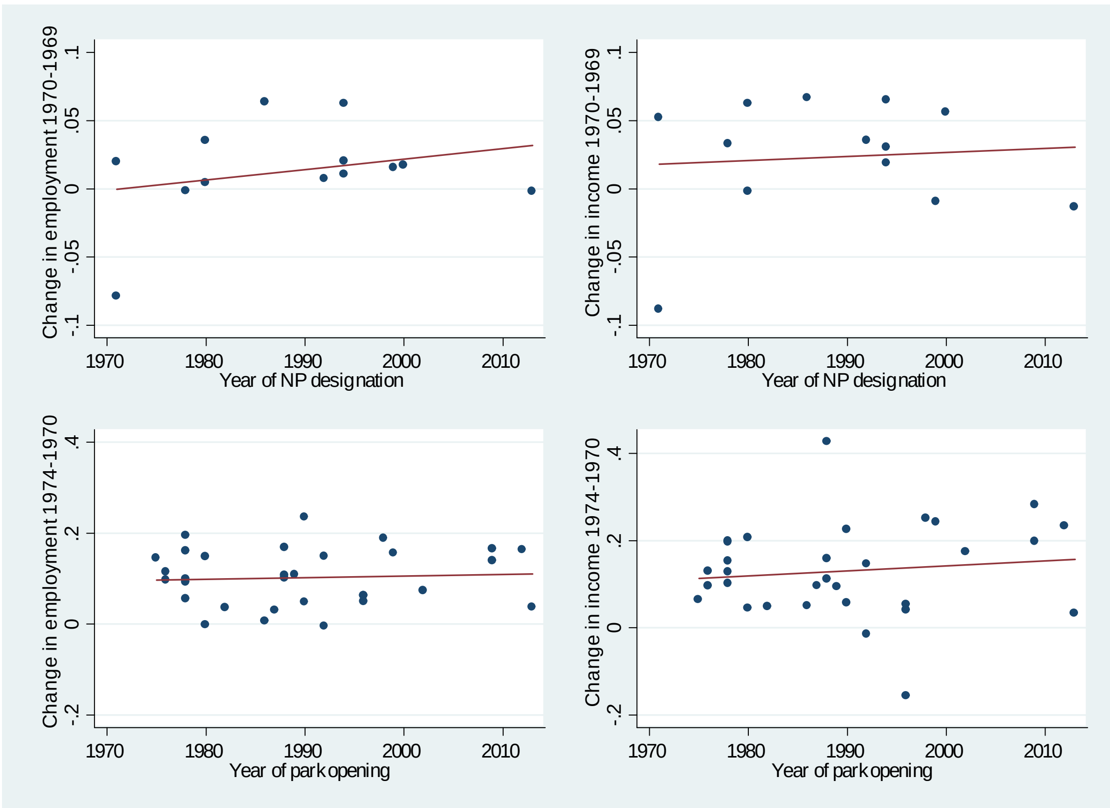

Figure 3: Relationship between treatment year and changes in outcomes prior to treatment

For to-be-treated units, year of treatment is plotted against the change in outcomes prior to the first treatment in the sample (1971 for NP designation, 1975 for park opening). Magnitude of point estimates from linear fit for NP designation: one s.d. higher employment growth means 3.3 (SE = 2.7) years later treatment, one s.d. higher income growth means 1.0 (SE = 4.4) year later treatment. For park opening: one s.d. higher employment growth means 1.3 (SE = 1.95) years later treatment, and one s.d. higher income growth means 0.7 (SE = 1.8) year later treatment.

3 Empirical strategy

Specification. The patterns above provide support for an empirical strategy that estimates the effects of treatment by comparing changes in outcomes around the time of treatment to a comparison group consisting of parks not treated at that time. Because treatment effects can be expected to vary over time (for example, changes in employment or park visitation are likely to occur gradually over several years following the treatment), we use an event study, which is the appropriate specification in this case.[11]

If each park experiences at most one "positive" treatment (e.g., acquiring NP designation), the standard event study specification is

$$Y_{pt} = \sum_{j=-4, j\neq -1}^{4} \beta_j \mathbf{1}(\tau_{pt} = j) + \widetilde{\beta}_5 \mathbf{1}(\tau_{pt} \geq 5) + \widetilde{\beta}_{-5} \mathbf{1}(\tau_{pt} \leq -5) + \boldsymbol{\gamma}\mathbf{X}_{pt} + \delta_p + \lambda_t + \varepsilon_{pt}, \quad (1)$$

where Y_{pt} is an outcome of interest for park p in year t. The variable τ_{pt} denotes time since the treatment, with $\tau_{pt} = 0$ if the park experiences the treatment in year t. Our excluded category is $\tau_{pt} = -1$, the year before the treatment. We estimate the impact of each treatment using a window of +/-5 years, with "binned" indicators for 5 or more years before/after the treatment. We chose 5 as the upper end of the event window because this is the number of lags we can observe for the last park acquiring NP designation. We chose -5 as the lower end for symmetry. To obtain some estimates over a longer time horizon, we also extend the event window to +/-15 years.

To allow for the fact that parks may experience negative events (e.g., a loss of NP designation), we assume that the impact of positive and negative events is symmetric, and include negative events in (1) with coefficients $-\beta_j$. See Schmidheiny and Siegloch (2019) for a review of the different event study specifications used in the literature.

The time-varying controls $\mathbf{X}_{pt}$ include log population density (in order to normalize our dependent variable, which is also in logs) as well as variables that could conceivably affect government policy towards the parks, tourism, and the local economy: the share of the population under age 19, the share above 65, a precipitation and a drought severity index, and, for the NP designation regressions, the park's age squared to control for reputation effects.[12] Adding these controls improves precision but is not crucial for the results. Finally, specification (1) includes time fixed effects λ_t and park fixed effects δ_p. The latter control for all level differences between parks, such as those in Table 1.

[11] A number of recent papers emphasize the limitations and possible bias in "static" difference-in-differences specifications in the presence of time-varying treatment effects (see Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2020), and studies cited therein).

[12] A linear function of the park's age is subsumed in the year and park fixed effects.

Note that equation (1) is specified at the same level as our treatments, i.e., at the park level. This allows us to obtain correct inference for a treatment that affects clusters of counties (Bertrand et al., 2004). This is an issue that often plagues conservation studies that estimate the impact of a few conserved areas on many counties or firms. We are able to address it directly here thanks to the fact that our dataset contains many parks.

For variables observed at the county level, we create Y_{pt} or X_{pt} as the average among the counties that overlap with park p.[13] Unless noted otherwise, our regressions cover the years t from 1970 to 2017 (1972 to 2017 in the case of park budgets).

Identification. Causal interpretation of the β_j coefficients in (1) rests on the standard parallel trends assumption that changes in Y_{pt} between $t-1$ and t in a park treated in period $(t-j)$ would have been similar, in the absence of treatment, to the control group consisting of parks not treated in $(t-j)$. This assumption would fail if some past shock (for example, an influx of tourists) had placed treated and control parks' outcomes on different paths before the treatment. We presented preliminary evidence against this possibility in Section 2.5, and the event study specification allows us to corroborate this directly by looking at the trends in the two groups' outcomes before treatment.

Another reason the identifying assumption could fail is if the treatment responded to contemporaneous shocks correlated with our outcomes: for example, if a park was opened in response to an uptick in local economic activity. Given the long process which leads to either a designation change or the opening of a park (see Section 2.1), this is extremely unlikely. While it is possible that changes in economic conditions could affect the *process* of park openings or redesignation (for example, through local politics, media attention, tourism, etc.), it is unlikely that such changes in a particular year would lead to either treatment occurring in the same year.

The control group used to identify each β_j can be subdivided into parks that are never-treated, parks that are not-yet-treated (but will be after t), parks that are already-treated (treated before t but after 1970), and parks that are always-treated (treated before 1970). The recent literature raises concerns about using the last two groups, already-treated and always-treated parks, for identification in the presence of treatment effect heterogeneity (see footnote 11). To address this issue, we first note that excluding always-treated parks from the set of parks used for identification (by including group-year fixed effects) has no impact on our estimates. Second, we use both a diagnostic from Sun and Abraham (2021) and an estimator from Callaway and Sant'Anna (2021) to show that our results for both treatments

[13] Since our dependent variables are in logs and we control for park fixed effects, taking the total or the average across counties is equivalent (the number of counties being aggregated is captured by the park fixed effects).

are valid even if the treatment effects are heterogenous (see Section 4.4).

4 Effects on local employment and income

4.1 NP designation

We begin by estimating the impact of NP designation on local employment (the average number of people employed in the counties containing a park), in logs. The estimates are in column (1) of Table 2. Column (2) shows the same specification with control variables, which results in very similar estimates. The estimates from column (2) are displayed on the first panel of Figure 4.[14]

We find that NP designation increases employment. Employment begins to rise significantly in the first year after a designation change, reaching around 4% above its initial level 4 years after the designation change (95% CI: [1.5%, 5.9%]). The point estimates imply an increase in employment in the average county from 2,100 jobs in year 1 up to 6,100 jobs in year 4.[15] Coefficient estimates for periods before the change are flat at 0, supporting the interpretation of these effects as being causal. If there was some other change, such as an increase in media attention and tourism in years -1 or -2 that caused both NP designation and higher economic activity, employment should begin to increase *before* the designation change. The estimates clearly show that this is not the case. Extending the pre-period further back in time yields similar conclusions (Figure A.3). We obtain similar statistical inference from p-values computed using alternative clustering procedures (Table A.4).

Column (3) of Table 2 shows that the estimates remain similar when we weight observations by the number of counties that a park overlaps. Weighting is motivated by the fact that economic variables reflect averages across counties, and parks overlapping more counties may therefore have an error term with lower variance in the regression. As suggested by Solon et al. (2015), the similarity of the weighted and unweighted estimates provides a check on the specification.

Columns (4-6) of Table 2 shows the corresponding results for local income. Estimates from our preferred specification (controls, unweighted) are displayed on the second panel of Figure 4. Similarly to employment, income also shows a gradual increase, to approximately 6% above its pre-change level by year 3 following the designation change (95% CI: [2.3%,

[14] Because different parks are treated in different years, the coefficients on the binned indicators ($\widetilde{\beta}_5$ and $\widetilde{\beta}_{-5}$) partly reflect changes in sample composition. As is common, we omit them from the graphs throughout.

[15] Throughout, figures such as these are obtained as $(\exp(\beta_j) - 1)\bar{Y}$ where β_j is the coefficient estimate and $\bar{Y}$ is the average outcome of interest in the counties overlapping the treated parks in the year before the treatment.

10.1%]. The point estimates imply that income in the average county overlapping with the redesignated parks rose by $76 million in year 1 and $193 million in year 4.[16]

Figure 4 clearly highlights the importance of the event study approach, and the limitations of static difference-in-differences in this setting. Economic impacts develop gradually over a period of several years following the treatment. Because static specifications estimate a weighted average of the treatment's impact over the post-treatment period, and *underweigh* long-term impacts, they would bias the estimated impact of NP designation downward (Borusyak and Jaravel, 2017).

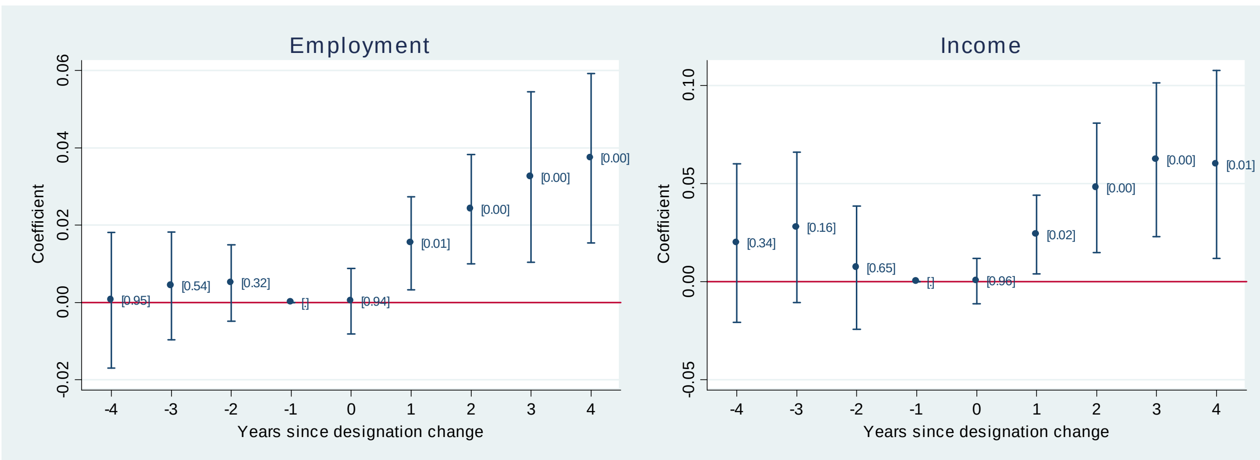

Figure 4: The impact of NP designation on employment and income

Event study coefficient estimates for the impact of NP designation on log employment and income. Estimates are relative to the year before the designation change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 9,024.

[16]Parks about to undergo redesignation are very similar to the comparison group between year -2 and -1. Between year -3 and -2, their income is (statistically insignificantly) higher and converging. Because, if anything, the pre-trend is declining, it cannot account for the increase in income we observe following the treatment.

Table 2: The impact of NP designation on employment and income

Years since change	Employment (1)	(2)	(3)	Income (4)	(5)	(6)
≤-5	0.034	0.030	0.031	0.037	0.036	0.057*
	(0.030)	(0.023)	(0.021)	(0.037)	(0.036)	(0.034)
-4	0.000	0.001	-0.001	0.017	0.020	0.028
	(0.013)	(0.009)	(0.008)	(0.020)	(0.020)	(0.020)
-3	0.004	0.004	0.011	0.028	0.028	0.034*
	(0.012)	(0.007)	(0.007)	(0.019)	(0.019)	(0.020)
-2	0.006	0.005	0.005	0.008	0.007	0.023
	(0.009)	(0.005)	(0.006)	(0.016)	(0.016)	(0.016)
0	0.001	0.000	-0.002	0.000	0.000	0.002
	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)	(0.005)
1	0.015**	0.015**	0.017***	0.024**	0.024**	0.031*
	(0.007)	(0.006)	(0.005)	(0.010)	(0.010)	(0.018)
2	0.022***	0.024***	0.028***	0.046***	0.048***	0.065**
	(0.008)	(0.007)	(0.006)	(0.017)	(0.017)	(0.027)
3	0.032**	0.032***	0.032***	0.058***	0.062***	0.054*
	(0.013)	(0.011)	(0.008)	(0.022)	(0.020)	(0.028)
4	0.036***	0.037***	0.043***	0.057**	0.060**	0.071*
	(0.011)	(0.011)	(0.011)	(0.025)	(0.024)	(0.038)
5≤	0.023	0.031	0.055***	0.020	0.029	0.061*
	(0.022)	(0.022)	(0.019)	(0.028)	(0.030)	(0.032)
Controls	no	yes	yes	no	yes	yes
Weighting	no	no	yes	no	no	yes
Adj. R^2	0.92	0.93	0.95	0.86	0.87	0.90
N obs.	9,024	9,024	9,024	9,024	9,024	9,024
N units	188	188	188	188	188	188

Notes: Event study estimates of the impact of NP designation on log employment and income, 1970-2017. Coefficients represent changes relative to year -1 (the year before the event). All specifications control for park and year fixed effects and log population density. Additional controls are the share of population under 19 and above 65, precipitation and droughts, and the park's age squared. In weighted regressions observations are weighted by the number of counties being aggregated. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

4.2 Park opening

We now turn to our second treatment, the opening of new parks. The dynamic treatment effect estimates are in Table 3, with the coefficients from columns (2) and (5) displayed on Figure 5.

The results show evidence of a gradual increase of both employment and income following the opening of a park. The estimates for employment indicate an increase of 2% by year 2 and 4% by year 4 (95% CI: [0.2%, 3.9%] and [1.0%, 6.5%], respectively). For income, the point estimates indicate an increase of 2% after one year, which rises to around 5% in subsequent years. For the treated areas, the implied increase in income is between $84 million (year 1) and $228 million (year 4). Estimates for a longer pre-period (Figure A.5) or using different standard errors (Table A.6) reinforce our findings.

It is noteworthy that newly opened parks during our period of study are *not* National Parks. The average park opened after 1970 is less than half the size of parks opened earlier; most of the parks opened later are historical, and their modal designation is National Historic Site, a relatively less prestigious designation in this category. According to some observers, the "quality" of the NPS has been diluted over time through the addition of these less prestigious areas (Foresta, 1984, Ch 3). Yet, our results indicate that even this pool of parks has a substantial positive impact on the local economy on average. The economic development impact of parks is clearly not restricted to the "crown jewel" National Parks.

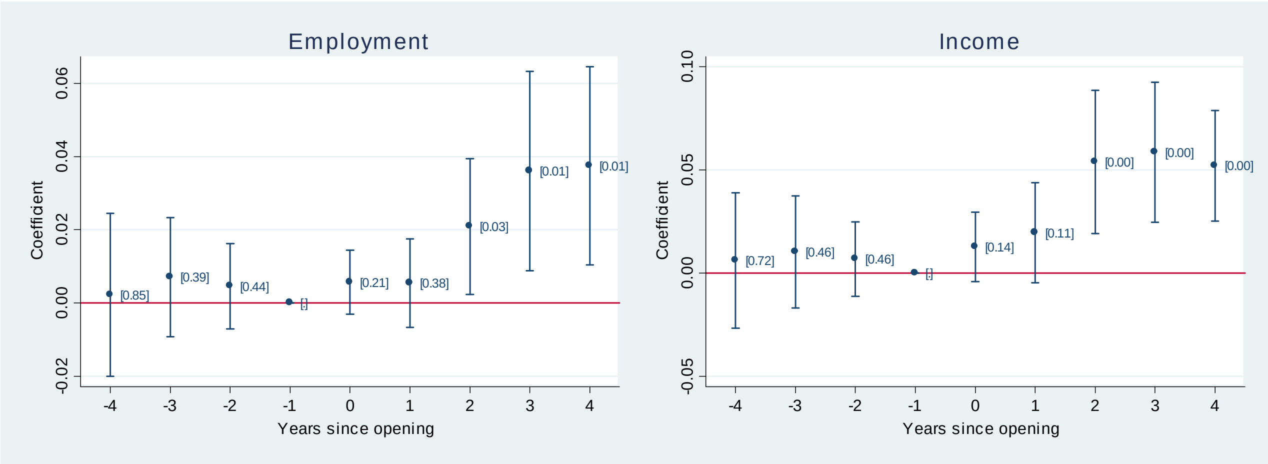

Figure 5: The impact of park opening on employment and income

Event study coefficient estimates for the impact of park opening on log employment and income. Estimates are relative to the year before the park opening. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 129,744 (employment) 129,730 (income).

Table 3: The impact of park opening on employment and income

Years since change	Employment (1)	(2)	(3)	Income (4)	(5)	(6)
≤-5	-0.016	-0.016	-0.004	-0.008	-0.014	0.012
	(0.020)	(0.019)	(0.016)	(0.030)	(0.030)	(0.026)
-4	0.002	0.002	0.011	0.007	0.006	0.014
	(0.011)	(0.011)	(0.010)	(0.017)	(0.017)	(0.013)
-3	0.007	0.007	0.011	0.010	0.010	0.012
	(0.008)	(0.008)	(0.007)	(0.013)	(0.014)	(0.011)
-2	0.004	0.005	0.005	0.006	0.007	0.004
	(0.006)	(0.006)	(0.004)	(0.009)	(0.009)	(0.007)
0	0.006	0.006	0.004	0.012	0.013	0.006
	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.009)
1	0.005	0.005	0.004	0.019	0.020	0.007
	(0.006)	(0.006)	(0.005)	(0.013)	(0.012)	(0.011)
2	0.021**	0.021**	0.018**	0.052***	0.054***	0.046***
	(0.009)	(0.009)	(0.008)	(0.018)	(0.018)	(0.013)
3	0.035**	0.036***	0.029***	0.056***	0.059***	0.047***
	(0.014)	(0.014)	(0.011)	(0.018)	(0.017)	(0.015)
4	0.037***	0.037***	0.032***	0.049***	0.052***	0.047***
	(0.014)	(0.014)	(0.011)	(0.015)	(0.014)	(0.012)
5≤	0.001	0.004	-0.006	0.051*	0.055*	0.039
	(0.018)	(0.017)	(0.018)	(0.029)	(0.030)	(0.026)
Controls	no	yes	yes	no	yes	yes
Weighting	no	no	yes	no	no	yes
Adj. R^2	0.85	0.85	0.86	0.72	0.72	0.74
N obs.	129,072	129,072	129,072	129,058	129,058	129,058
N units	2,689	2,689	2,689	2,689	2,689	2,689

Notes: Event study estimates of the impact of park opening on log employment and income, 1970-2017. Coefficients represent changes relative to year -1 (the year before the event). All specifications control for park and year fixed effects and log population density. Additional controls are the share of population under 19 and above 65, precipitation and droughts. In weighted regressions observations are weighted by the number of counties being aggregated. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

4.3 Comparisons to existing estimates

4.3.1 Comparison to the Park Service's measures

It is interesting to compare our estimates to measures of the parks' *economic contribution* published annually by the Park Service (e.g., Thomas et al., 2018). These official measures inform policy makers and have been used by analysts as a basis for evaluating proposed changes to the NPS (Headwaters Economics, 2018).

To compute the number of jobs and the income created in the local economy by each park in a given year, the Park Service multiplies estimated annual visitor spending with regional economic multipliers derived from an input-output model (Thomas et al., 2018, p2). This is meant to account for direct and indirect economic impacts *attributable to visitor spending in a given year*. The same methodology is used by other agencies to calculate the economic contribution of national forests (USDA, 2019), state parks (Jeong and Crompton, 2019), museums (Oxford Economics, 2017), coral reefs (Wallmo et al., 2021), and tourism in different states (e.g., Rockport Analytics, 2022).

Figures obtained using this procedure cannot be interpreted as the causal effect of parks on the economy.[17] First, the measures are limited to the impact of visitor spending and do not consider other channels through which parks can affect the local economy. Some of these effects could be negative, such as restrictions on resource extraction.

Second, even among the effects that can be linked to visitor spending, the measures only capture the impact of *annual* visitor spending, i.e., they ignore dynamic effects arising from economic activity over multiple years. For example, suppose that a local store invests in a new building, anticipating increased demand from visitors in future years. Or, the owner of a sold-out hotel saves her income and spends it in a future year, when the number of visitors and her revenue is lower. Neither of these examples would be fully captured by the park's measured economic contribution, as this does not take into account the current impact of increased demand in either the future (first example) or the past (second example).

A third reason the Park Service's figures may be different from the parks' causal effects is that some visitor spending around a park could be incidental (e.g., visits could be caused by a local cultural event). The Park Service attempts to estimate the number of incidental visitors and only counts a given fraction of their spending towards the park's economic contribution. However, this fraction may be different from the spending that would not have

[17] The Park Service is explicit about this: "[Economic contribution estimates] should not be confused with an economic impact analysis [...] the economic activity that would likely be lost from the local economy if the National Park was not there." (Thomas et al., 2018, p3) However, this is based on the notion that what would be lost if the park was not there is limited to "the inflow of new money to the economy solely from non-local visitors." (Thomas et al., 2018, p3) As we explain below, there are in fact many other effects that would be lost.

occurred in the absence of the park.

The preceding discussion implies that the Park Service's economic contribution figures could be lower or higher than the parks' causal effect on the economy. To get a sense of the difference, we compare their figures to our estimated causal effects of the extensive margin treatment (the opening of a new park). We consider the 31 newly opened parks in our sample, i.e., the treated parks in the park opening regressions. Based on the Park Service's figures, for 2017, the contribution of the average park in this group is 273 jobs (with a range of $[1, 1723]$ across the 31 parks) and \$25.6 million in economic output (with a range of $[0.155, 164.9]$ million) at current prices.[18] For the same year and the same set of parks, our point estimate for β_0 implies 2,823 new jobs and an extra \$424.3 million in local income for all counties overlapping with the average park.[19] Using reports from different years yields similar results - for example, in 2013 the Park Service's figure imply a contribution of 380 jobs and \$24 million in income, while our estimates imply 2,621 new jobs and \$379.6 million in extra income.

Computing economic impacts relative to a counterfactual without parks yields considerably larger estimates than the economic contributions attributed to visitor spending in a given year.[20] Recall however that our estimate for β_0 was not statistically different from 0, and that we found larger effects a few years after a park opening. For example, using the coefficients for year 2, we estimate an extra 10,568 jobs, and an extra \$1,836.0 million in local income.[21] By ignoring dynamic treatment effects, such as those arising from savings and investment, the Park Service's figures may considerably understate the economic impact of parks in the NPS.

4.3.2 Comparison to other programs

It is also instructive to compare our estimates to the effect of other programs. Because the existing conservation literature often uses different estimation methods and studies programs that are different from the NPS, these comparisons are merely suggestive. In general, a number of papers find that conservation is positively associated with employment but not

[18] These figures are computed from Table 3 in Thomas et al. (2018, p18-30), using the "Jobs" and "Economic Output" columns.

[19] We obtain these figures as $(\exp(\hat{\beta}_0) - 1)\bar{Y}$, where $\hat{\beta}_0$ is the point estimate from the regression, and $\bar{Y}$ is the relevant benchmark. To compute $\bar{Y}$, we take the 2017 total of the dependent variable across all counties overlapping with a park, and then take the average across the 31 parks. In the employment regression, $\hat{\beta}_0 = 0.0056317$ and $\bar{Y} = 499,905$. In the income regression, $\hat{\beta}_0 = 0.0127323$ and $\bar{Y} = 33,113.170$ million.

[20] Interestingly, our estimate of income effects is comparable to the results obtained by Haefele et al. (2020) from a willingness-to-pay survey on the NPS. A back-of-the-envelope calculation using the figures in their Table 2.5 for the average park being considered here yields \$599 million in 2017 dollars.

[21] These are obtained as above, using the point estimates $\hat{\beta}_2 = 0.0209191$ (employment) and $\hat{\beta}_2 = 0.0539644$ (income).

with incomes. For example, Sims et al. (2019) find that a 1% increase in protected land area around New England towns is associated with a 0.03% increase in employment, but has no impact on household income. Jakus and Akhundjanov (2019) and Walls et al. (2020) study the establishment of national monuments that are not part of the NPS in 8 western states. Walls et al. (2020) find an increase in employment, but both papers find no effect on income. As we discuss below, a possible explanation for our different findings is that visitors to the NPS generate economic activity that these other programs do not.

Another form of government spending that is explicitly designed to attract visitors is subsidizing local sports venues (e.g., stadiums). A large literature estimates the impact of these subsidies, and Bradbury et al. (2023) document the "near-universal consensus" that these do not generate large positive impacts on the local economy. Relative to sporting events, parks in the NPS have more classic public goods features (e.g., they are essentially non-excludable as they can be visited any day of the year). In addition, they are often located in areas with few close substitutes and less tourism to begin with. Both of these features is likely to matter for the different results.

We can also compare the magnitude of our effects to that of place-based policies that explicitly target local economic development. The federal Empowerment Zone program, which provides employment tax credits and other investment incentives in specific communities, is estimated to have increased local employment by 12-21 percent and wages by 8-13 percent in the 1990s (Busso et al., 2013). In the early 20th century, the Tennessee Valley Authority, which provided electrification to rural areas, increased local employment growth by an estimated 10 percentage points (Kline and Moretti, 2014). While these effects are larger than our estimates, they arise in the context of government programs which, unlike the NPS, were designed specifically to generate economic development.

4.4 Robustness

Appendix 3 contains a detailed discussion of the robustness checks we performed on the estimates above. This includes changes in the sample, controlling for various possible confounders, and changes in the comparison groups used for identification, either based on a priori considerations or on propensity scores.

We also explicitly address some of the concerns raised in the recent literature regarding the possible bias of difference-in-differences estimates in the presence of treatment effect heterogeneity. First, we follow a method proposed by Sun and Abraham (2021) to assess whether treatment effect heterogeneity would affect our ability to compute meaningful average treatment effects. Computing different treatment cohorts' weights in our estimates indicates that, for both treatments, our estimates would be valid even in the presence of heterogenous effects (Figures A.11 and A.16). Second, we show that our findings are robust to using the Callaway and Sant'Anna (2021) estimator explicitly designed for the case of heterogenous treatment effects (Table A.12). This estimator also reduces the comparison groups used for identification, and therefore establishes the causal interpretation of our results under a weaker parallel trends assumption.

5 Mechanisms

5.1 Tourism, government spending, and park size

Our data allows us to study several specific mechanisms behind the above economic impacts.

Tourism. Local support for parks is typically based on the expectation of increased tourism. Are these expectations justified? The left panel of Figure 6 shows that NP designation puts park visitation on a path of gradual increase, eventually leading to 17% more visitors. The year of NP designation shows a 5% increase, which rises to 7.5% by year 1 and a statistically significant 17% by year 2. Coefficients for subsequent years suggest a sustained long-run increase in visitation as a result of NP designation. This provides support for the idea that increased tourism is a key mechanism behind local economic development resulting from the parks.

These estimates for visitors are interesting in their own right as they can shed light on the difference in visitation between National Parks and other parks in the system. In our sample, visitation in National Parks is higher by a factor of 1.9. A priori, there could be a number of reasons for this: National Parks may have unique natural features that make them inherently more interesting, they may be more easily accessible through major airports, etc. Our estimates indicate that about one fifth of the difference in visitation can be explained simply by the National Park label.

Government spending. Creating or upgrading parks involves government spending on infrastructure, facilities, wages, etc. which directly contribute to the local economy. What fraction of parks' economic impacts can be accounted for by direct government spending on parks?

The right panel of Figure 6 estimates the impact of NP designation on park budgets. We find that the average park budget increases by 8.4% one year after the designation change, and 22-26% in subsequent years. NP designation appears to have a sustained positive effect on annual park budgets. The increase in budgets is sizeable in percentage terms, but its absolute value is small. For the average park experiencing a designation change, a 20%

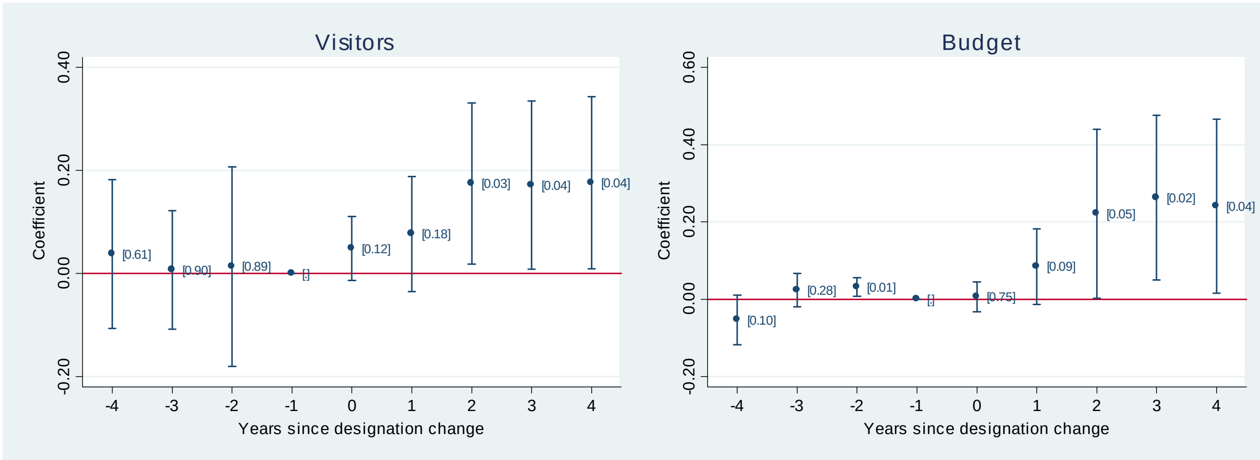

Figure 6: The impact of NP designation on visitors and park budgets
Event study coefficient estimates for the impact of NP designation on the log number of visitors and log park budgets. Estimates are relative to the year before the designation change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 8,880 (visitors), 8,568 (budget).

annual budget increase means an extra $256,652 per year. This is less than 0.5% of the income increase we found above, indicating that economic impacts go well beyond direct government expenditures on local goods and services associated with the parks.

Although it is not possible to provide a corresponding estimate for park openings (as parks have no budget before opening), we note that the average annual budget of newly opened parks in their first 5 years is $481,390. This is again much smaller than the corresponding income effects, suggesting that the economic impact of new parks also cannot be accounted for by direct government spending.

It could be that NP designation or park opening is bundled with other government spending programs. We address this possibility in three ways. First, we show that our findings are robust to dropping one treated park at a time, ruling out that government spending around one specific park is driving the results (Figures A.10 and A.15). Second, we include state-year fixed effects. These control for any state-level policy changes that may occur in a given year in a fully flexible way, and we find that our findings still hold in this case (Tables A.7-A.8 and A.13-A.14).

Based on these results, for any confounding policy to call into question the causal interpretation of our estimates, it would have to be the case that (i) the policy is systematically implemented in the same year as the designation change, and (ii) it affects the counties that contain the redesignated park differentially *within the state*. As a final check to rule out that the effects of parks are due to other government programs, we use data from the Census of Governments and estimate the impact of NP designation and park opening on total spending of all local governments within a county. These regressions show no evidence of an increase in government spending programs around the park - if anything, spending shows a slight decline (Figure A.17). Government spending in or around the parks cannot account for their economic impacts.

Changes in size. We have interpreted the impacts of NP designation as the impacts of intensive-margin changes in the park system. However, NP designation is often accompanied by an expansion of a park's acreage (Figure A.18). To what extent are park expansions responsible for the economic impacts?

To answer this question, we run "horse-race" regressions that include two sets of event study indicators, one for NP designation and one for large park expansions. We find that our results above regarding the impact of NP designation remain unchanged (Figures A.20-A.21). These effects of parks on employment, income, or visitors cannot be explained simply by changes in the size of the conserved area. In other words, these economic impacts seem to require more than merely increasing the acreage under conservation.

5.2 Other designations

It is instructive to compare the impact of NP designation to other park designations. First, we consider other designations within the NPS. During our period of study, the second most common change after NP was to National Historic Park (NHP), with 11 such redesignations in our sample. As the name suggests, these parks are primarily of historical interest, and the NPS considers NHP the most prestigious designation in this category. We find that NHP designation yields an increase in park budgets of around 26%, but it causes at most a temporary increase in visitors: we see a statistically insignificant 14% increase that falls by a half 4 years after the designation change. Estimates for employment and income are flat both before and after the designation change (Figure A.22).

Second, we consider the most prestigious international designation, the UNESCO's World Heritage Site (WHS) designation. This is awarded each year by an international committee based on countries' nominations, and 13 parks in the NPS received it during our sample period. This designation also shows no significant effect on visitors, with point estimates close to zero. Here the budget estimates are also insignificant, and we again do not find any increase in employment or incomes (Figure A.23).

Comparing the three designation changes (NP, NHP, and WHS), we find that it is only where visitation shows a clear, sustained increase that we also find increases in employment and income. This underscores the relevance of visitors as the channel behind increased economic activity. In turn, these findings suggest an explanation for why areas that are protected but do not have extensive visitor infrastructure, such as national monuments which are not part of the NPS, appear to have no impact on local income (Jakus and Akhundjanov, 2019; Walls et al., 2020).

5.3 Geographic spillovers

Some of the positive employment and income effects of NP designation may simply reflect the relocation of economic activity from neighboring areas. In this case, our results may not indicate a net gain in employment and income. Conversely, it is possible that neighboring areas also experience positive effects.

To study possible spillover effects, we look at the *neighbor* counties of the treated parks, i.e., the counties neighboring the county or counties a park is located in. For each park, we take the average employment or income across its neighbors, and run our previous regressions using these variables as outcomes.

We do not find any evidence of a decline in employment and income in neighboring counties for either NP designation or park opening (Figure A.24). In fact, we see the opposite: there are positive spillovers on employment and income. For NP designation, the point estimates are approximately 1/2 of the results we found above (1-2% for employment and 2-3% for income) but most estimates are not statistically significant. For park opening, the estimates are similar in size to the main results and are statistically significant throughout. There is no evidence that our main results are driven simply by the relocation of businesses and employees from neighboring counties. If anything, we see positive economic impacts on surrounding areas.

A related question is whether some of our results could reflect the relocation of tourism from nearby parks. To address this, for each park in the NP designation regressions we check if there are any treated parks within 100 miles. We create a new set of event study indicators based on this, and include both the NP designation event and the "NP designation in a nearby park" event in the regressions. We find no reduction in visitors when a nearby park receives NP designation (Figure A.25). The effect of NP designation on visitors is not simply due to substitution away from nearby parks.

Of course, it is not possible to measure every conceivable place and activity that tourists and economic actors may substitute away from. For example, we do not know where tourists who visit National Parks would have spent their money otherwise. Along with parks' local effects, these general equilibrium considerations are also likely to be important in a full welfare evaluation of the NPS.

6 Longer run effects

To obtain some evidence on longer-run effects, we extend our specifications to include lags up to 15 years. As usual, long-run estimates should be interpreted with caution as many things unrelated to parks can change in the local economy over time.

Figure 7 looks at park visitors and budgets following NP designation and finds that the increases described in the paper are sustained in the longer run as well. By year 10, visitors increase by around 30% and budgets by close to 40% compared to their pre-redesignation levels. The visitor results in particular indicate that the impact of NP designation on tourism is more than just a temporary "novelty" effect.

Figure 8 shows the results for our main economic variables, employment and income. Each figure shows both the baseline (unweighted) estimates and the estimates weighting each park by the number of counties to provide a specification check (Solon et al., 2015). As can be seen, the estimates indicate sustained positive, statistically significant effects on both employment and income for about 10 years after NP designation. However, the weighted and unweighted estimated diverge. While the weighted results indicate sustained positive effects, the unweighted results show a declining pattern. For employment, the unweighted effects become close to 0, while for income the point estimates remain sizeable (around +5%) but become statistically insignificant. Based on both the large standard errors and the difference between the weighted and unweighted results, the conservative interpretation is that compared to its robust effects in the short run, the long-run impact of NP designation, particularly after a decade, is more uncertain.

Figure 9 shows the corresponding estimates for the impact of park opening.[22] Here too, the standard errors of the estimates grow over time, but the weighted and unweighted estimates now remain close to each other. Both sets of estimates show a positive long-run effect on income, while the effects on employment clearly dissipate.

The different patterns for employment and income could be consistent with various interpretations. One possibility is that the opening of parks (and perhaps NP designation) turns towns into popular "gateway" communities, resulting in increased prices and wages, similarly to some theories of the "Dutch disease" effects of local resource booms (e.g., Allcott

[22] Because the sample composition depends on the event window, these estimates are identified from a smaller set of parks than the short run results. Regressions with the [-5,+15] event window include 27 newly opened parks (these are the parks with a full set of lags and leads in the sample period). Comparing the coefficients for years -4 to 4 on Figure 9 to our main estimates provides a further robustness check on the latter.

and Keniston, 2018). Investigating such long-run effects is an interesting avenue for future research.

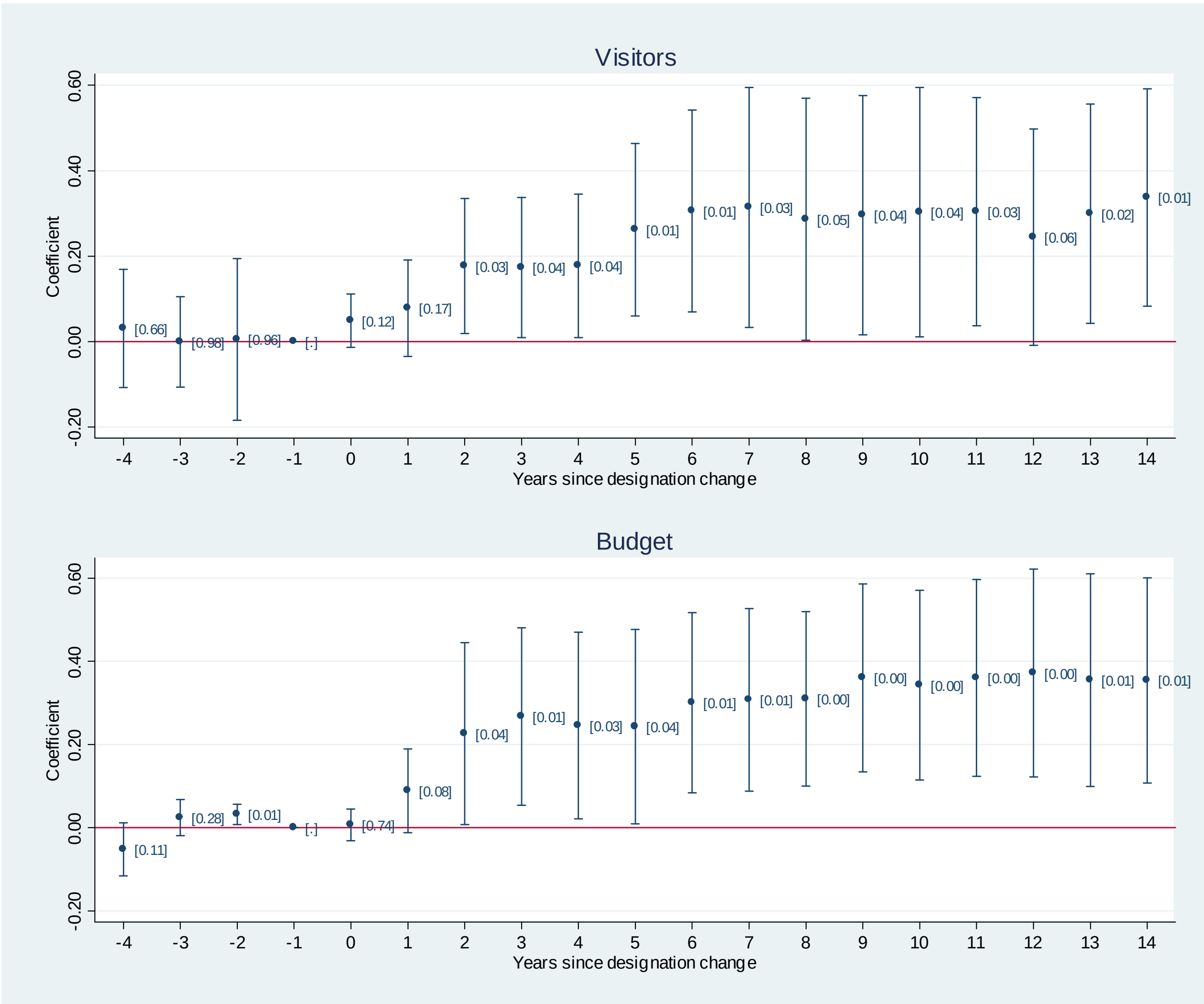

Figure 7: The impact of NP designation on visitors and park budgets in the long run
Event study coefficient estimates for the impact of NP designation on log visitors and budgets. Estimates are relative to the year before the designation change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 8,880 (visitors), 8,568 (budget).

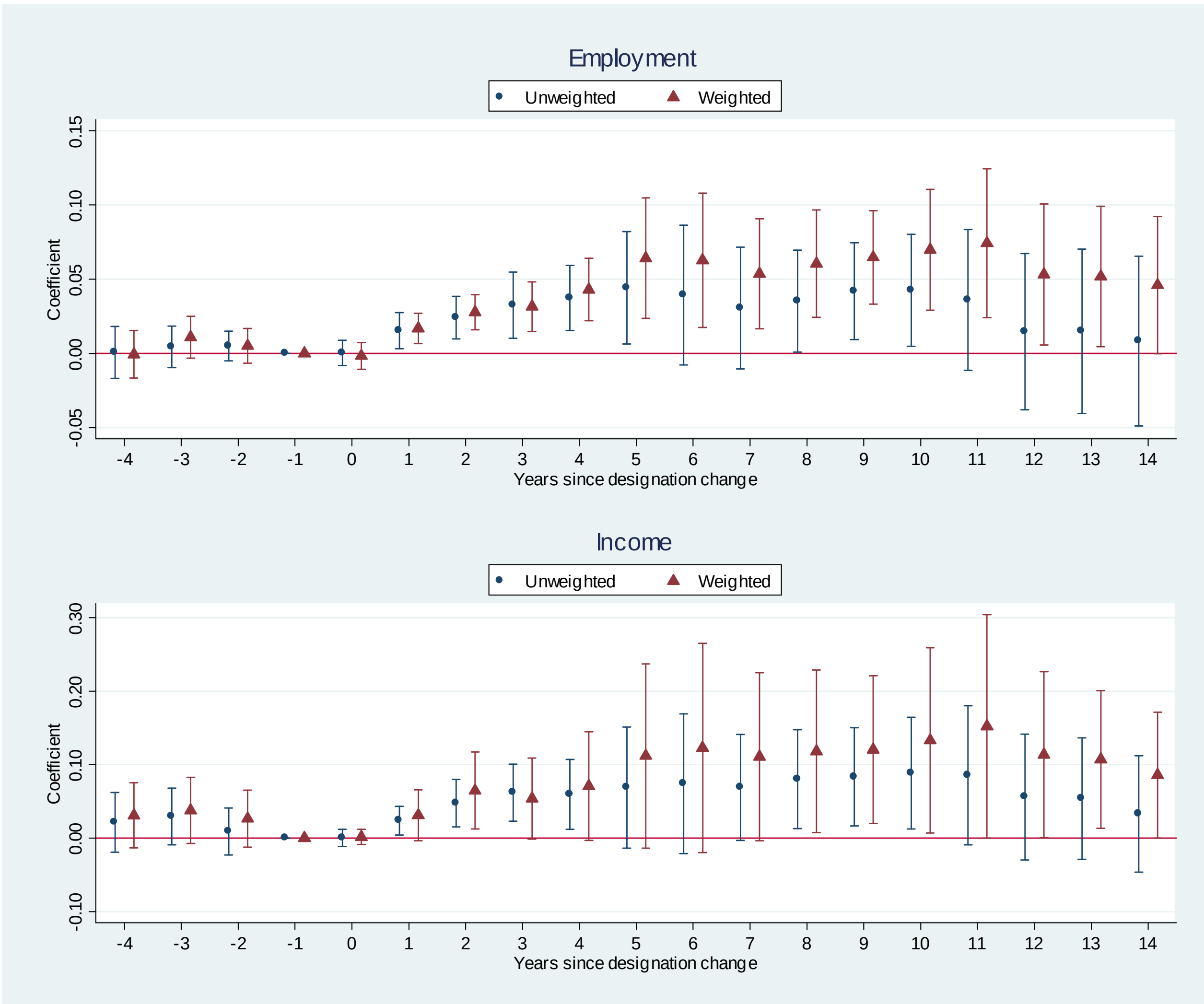

Figure 8: The impact of NP designation on employment and income in the long run

Event study estimates for the impact of NP designation on log employment and income. Each panel shows estimates from two regressions. One is unweighted, the other weights observations by the number of counties aggregated to create the dependent variable. Estimates are relative to the year before the designation change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 9,024.

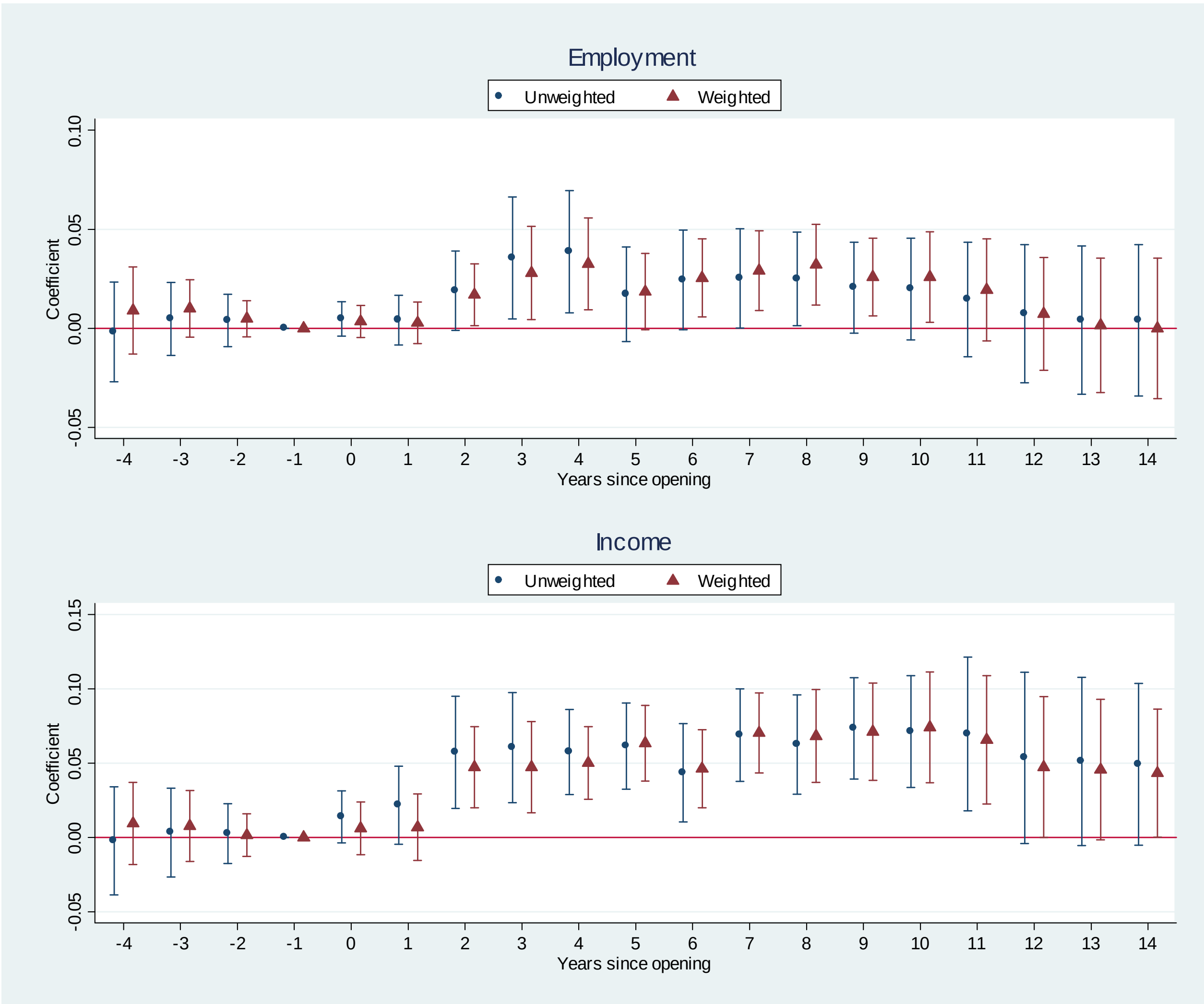

Figure 9: The impact of park opening on employment and income in the long run

Event study estimates for the impact of park opening on log employment and income. Each panel shows estimates from two regressions. One is unweighted, the other weights observations by the number of counties aggregated to create the dependent variable. Estimates are relative to the year before the designation change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 127,200 (employment) 127,186 (income).

7 Conclusion

This paper presents an analysis of the local economic impacts of the US National Park System, the largest and best known national conservation entity in the world. We introduce a detailed park-level dataset and investigate the impact of parks through various channels.

We consistently find large positive effects on local employment and income, and these are not restricted to the "crown jewel" National Parks. These effects are larger than what can be explained by direct government spending on park budgets, and indicate substantial multiplier effects on the local economy from visitors. Comparing different designation changes suggests that increased visitation is a necessary condition for increased employment and incomes.

Conservation policy in the US has undergone several changes over the past decade, putting the spotlight on the economic effects of conservation. The National Park System has also served as a model for conservation efforts around the world, and the tradeoffs between conservation and development are a particularly salient question in developing countries. Our study provides evidence that may help inform some of these policy discussions.

First, our paper has implications for analyses of the impact of protected areas using visitor spending and economic multipliers (such as those derived from input-output models). This is a common methodology for evaluating economic effects not just by the US Park Service, but also in other countries including Canada (Outspan Group, 2011), Zambia (Chidakel et al., 2021), Peru (Vilela et al., 2018) or Iceland (Siltnanen et al., 2023). Without questioning the internal logic of these calculations, our findings highlight some of the additional economic considerations that can be captured using quasi-experimental methods. In particular, analyses that ignore dynamic treatment effects such as those arising from savings and investment, can considerably understate the economic impact of protected areas.

Second, our findings provide direct evidence that the economic benefits of parks are linked to tourism. This is important because several studies on developing country conservation programs have found positive impacts and argue that tourism is the likely mechanism (e.g., Sims, 2010; Ferraro and Hanauer, 2014). However, a lack of data has typically prevented direct tests of this hypothesis. In this sense our findings bolster the economic argument for protected areas based on tourism.

This also has implications for the types of services that governments might invest in to maximize the impact of parks, or the types of data they might collect in order to measure these impacts. For example, there are 372 national parks in 54 countries on the African continent (covering about 4.5% of the continent's area), but many of these do not have facilities or even allow visitor access. Only a few parks appear to have visitor numbers available. Our findings suggest that improving access may be crucial to realize the economic potential of these areas, and collecting systematic visitor data may be important to measure their impacts.

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