Online Appendix to "National Parks and Economic Development"

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December 10, 2023

Contents

1 Data and summary statistics

Table A.1: NPS designations 1970-2017

Designation	Nature park
International Historic Site	0
National Battlefield	0
National Battlefield Park	0
National Battlefield Site	0
National Historic Landmark	0
National Historic Park	0
National Historic Park and Preserve	0
National Historic Site	0
National Lakeshore	1
National Memorial	0
National Memorial Parks	1
National Military Park	0
National Monument	1[a]
National Monument and Historic Site	0
National Monument and National Preserve	1
National Park	1
National Park and Preserve	1
National Park for the Performing Arts	0
National Parkway	1
National Preserve	1
National Recreation Area	1
National Recreational River	1
National Reserve	1
National River	1
National River and National Recreation Area	1
National Scenic River/Riverway	1
National Seashore	1
Scenic and Recreational River	1

[a] National monuments are categorized as nature parks, except the following: Aztec Ruins, Booker T. Washington, Cabrillo, Casa Grande Ruins, Cesar E. Chavez, Charles Young Buffalo Soldiers, Fort Frederica, Fort Pulaski, Fort Stanwix, Fort Sumter, Fort Union, George Washington Birthplace, George Washington Carver, Gila Cliff Dwellings, Homestead of America, Lincoln Boyhood, Little Bighorn Battlefield, Montezuma Castle and Tuzigoot, Navajo, Ocmulgee, Pipe Spring, Pipestone, Salinas Pueblo Missions

Table A.2: Summary statistics, 1970-2017

Variable	Mean	Std. dev.	10%	50%	90%	N
			NPS data			
All parks						
Visitors	862.222	1,910.206	28.273	234.843	2,125.081	10,990
Budget	1,431.003	1,951.221	213.466	693.113	3,430.795	10,714
Acreage	111.966	330.252	0.043	3.393	241.865	11,082
Age	50.290	29.869	12.000	48.000	90.000	11,606
National Park	0.164					11,606
National Monument	0.252					11,606
National Historic Park	0.091					11,606
Nature park	0.477					11,606
National Parks						
Visitors	1,429.670	1,671.875	171.451	857.031	3,216.681	1,893
Budget	3,343.676	2,886.144	865.517	2,280.311	6,889.570	1,833
Acreage	427.615	569.927	35.835	218.200	1,013.572	1,895
Age	72.418	30.160	34.000	73.000	107.000	1,900
			Employment and income			
NP designation sample						
Employment	10.305	1.585	8.368	10.185	12.651	9,024
Income	13.153	1.753	11.012	13.027	15.698	9,024
Park opening sample						
Employment	9.335	1.390	7.726	9.198	11.175	129,072
Income	12.088	1.540	10.313	11.932	14.118	129,058

Notes: Number of parks: 269. Visitors: annual, in 1000. Budget: annual, in 1000 1982-84 dollars. The budget series starts in 1972. Acreage: 1000 acres. National Park, National Monument and National Historic Park are indicators for some of the main designations. Table A.1 gives the list of designations categorized as "nature parks." We aggregate county-level employment and income to the park level as described in the main text. See the Online Data Appendix for sources and further details on the NPS dataset.

2 Further details on main estimates

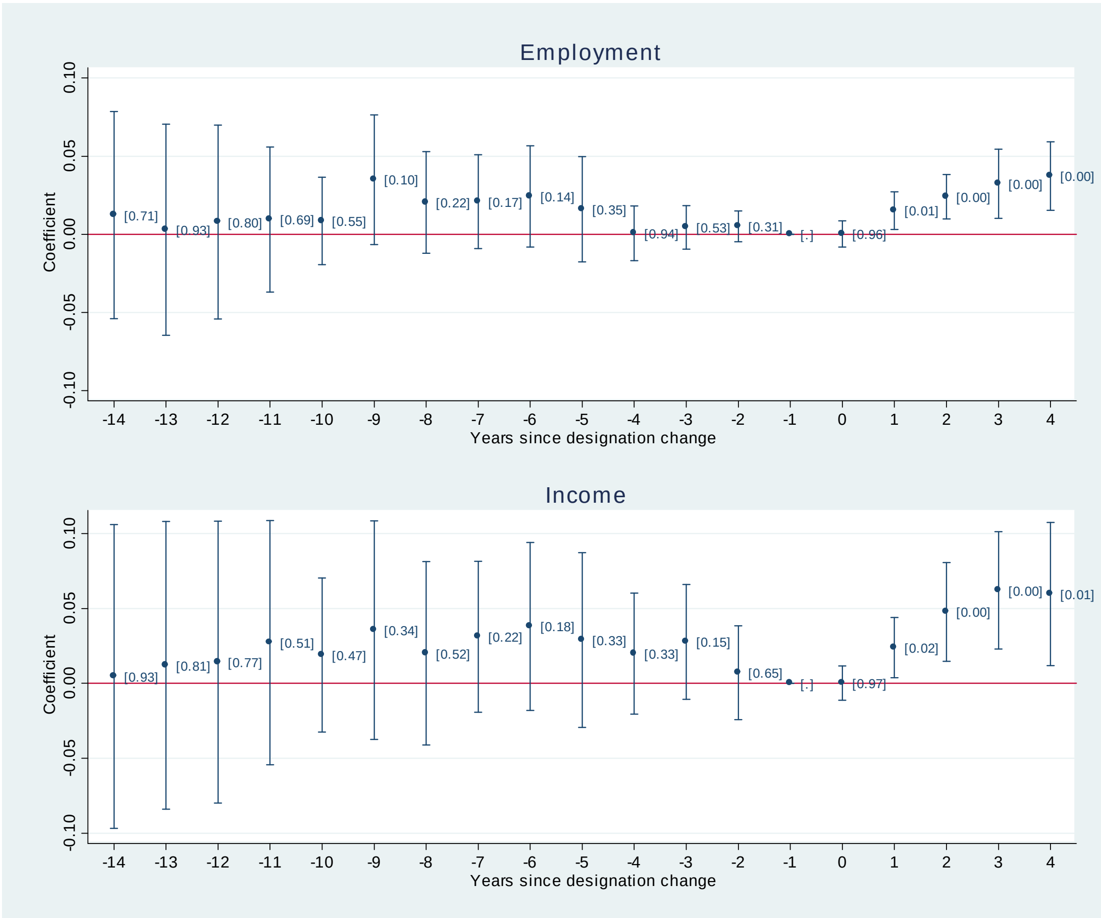

Figure A.3: The impact of NP designation: pretrends

The figure shows estimates on a longer pre-period for the effect 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.

Table A.4: p-values computed using different procedures, NP designation

Dep. var.:	Employment			Income		
Inference:	Asymptotic	Bootstrap	Asymptotic	Asymptotic	Bootstrap	Asymptotic
Clustering:	Park	Park	Park group	Park	Park	Park group
	(1)	(2)	(3)	(4)	(5)	(6)
≤-5	0.21	0.24	0.22	0.31	0.34	0.31
-4	0.95	0.95	0.95	0.34	0.37	0.18
-3	0.54	0.52	0.61	0.16	0.18	0.04
-2	0.32	0.25	0.29	0.65	0.69	0.37
0	0.94	0.95	0.94	0.96	0.96	0.96
1	0.01	0.03	0.05	0.02	0.01	0.04
2	0.00	0.00	0.01	0.01	0.00	0.00
3	0.00	0.01	0.01	0.00	0.00	0.00
4	0.00	0.00	0.01	0.02	0.03	0.02
5≤	0.15	0.18	0.07	0.34	0.36	0.15
N obs.	9,024	9,024	9,024	9,024	9,024	9,024
N clusters	188	188	147	188	188	147

Notes: Columns 1 and 4 report the p-values for the main NP designation specifications (columns 2 and 5 in Table 2 in the paper). Columns 2 and 5 present p-values from the clustered wild bootstrap. Columns 3 and 6 are based on asymptotic standard errors clustered by groups of parks, where parks are partitioned in the smallest sets such that they share a county with at least one park in the same set.

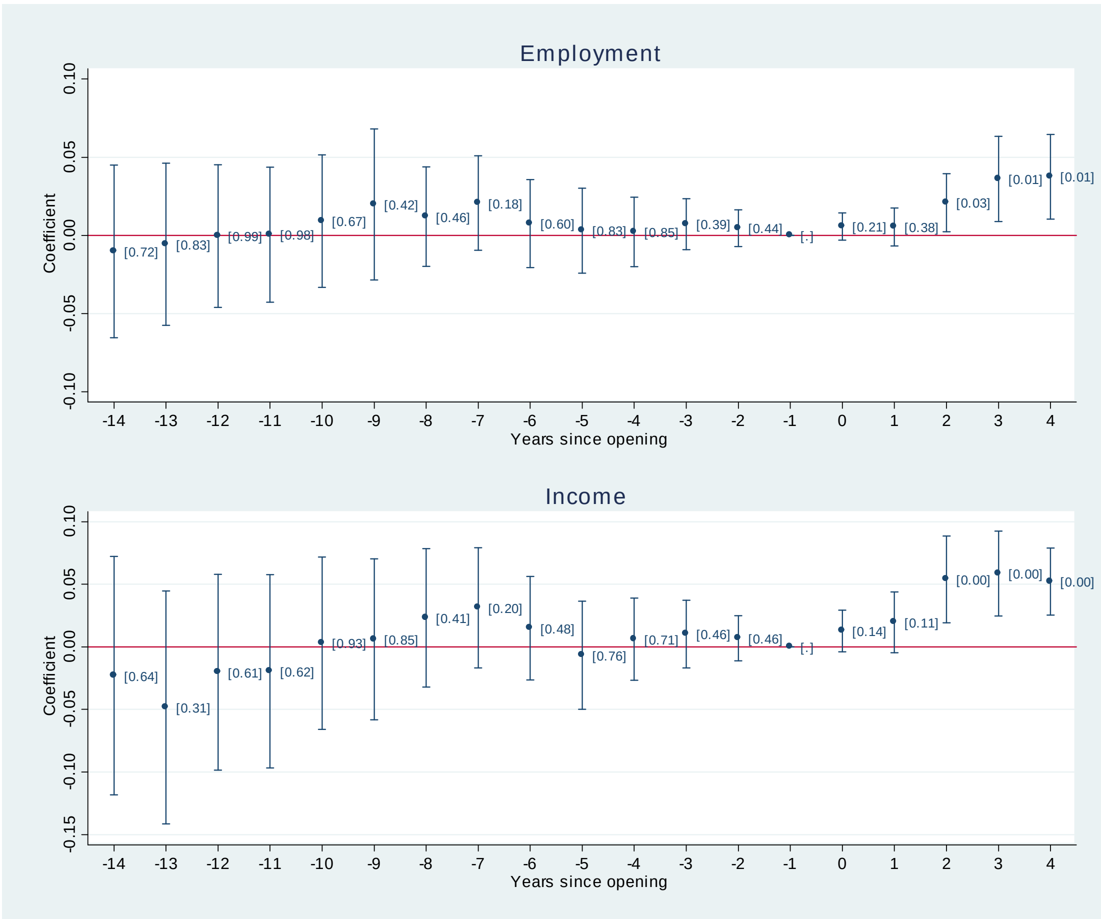

Figure A.5: The impact of park opening: pretrends

The figure shows estimates on a longer pre-period for the effect of park opening 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 = 129,072 (employment) 129,058 (income).

Table A.6: p-values computed using different procedures, park opening

Dep. var.:		Employment			Income	
Inference:	Asymptotic	Bootstrap	Asymptotic	Asymptotic	Bootstrap	Asymptotic
Clustering:	Park	Park	Park group	Park	Park	Park group
	(1)	(2)	(3)	(4)	(5)	(6)
≤-5	0.41	0.43	0.41	0.63	0.65	0.63
-4	0.85	0.85	0.85	0.72	0.71	0.72
-3	0.39	0.40	0.40	0.46	0.45	0.46
-2	0.44	0.46	0.44	0.46	0.46	0.45
0	0.21	0.21	0.21	0.14	0.13	0.13
1	0.38	0.41	0.39	0.11	0.14	0.11
2	0.03	0.04	0.03	0.00	0.00	0.00
3	0.01	0.01	0.01	0.00	0.00	0.00
4	0.01	0.00	0.01	0.00	0.00	0.00
5≤	0.81	0.81	0.81	0.07	0.08	0.06
N obs.	129,072	129,072	129,072	129,058	129,058	129,058
N clusters	2,689	2,689	2,643	2,689	2,689	2,643

Notes: Columns 1 and 4 report the p-values for the main park opening specification (columns 2 and 5 in Table 3 in the paper). Columns 2 and 5 present p-values from the clustered wild bootstrap. Columns 3 and 6 are based on asymptotic standard errors clustered by groups of parks, where parks are partitioned in the smallest sets such that they share a county with at least one park in the same set.

3 Detailed robustness checks

3.1 NP designation

Unless stated otherwise, all estimates described in this section are in Tables A.7 (employment) and A.8 (income).

Sample composition. In the NP designation regressions, two parks which acquired NP status in 1971 are not observed before year -1 relative to the designation. We checked whether this lack of balance in the sample affected our estimates in two ways. First, at the cost of dropping some of the control variables, we can extend the data by one year, to 1969. This allows us to estimate an event window beginning in year -2 on a balanced sample (column 2). Second, we simply exclude these two parks from the estimation (column 3). In both cases the estimates are similar to those in our main specification.

Different comparison groups and identification. The treatment effects above are identified by comparing changes relative to comparison groups that include all parks that do not receive NP designation in a given year.[1] While the lack of clear pre-trends suggests that these comparison groups offer a reasonable counterfactual for the treated parks, we now explore limiting them to parks that may offer even better comparisons.

First, we restricted the sample to "nature" (as opposed to historical) parks (see Table A.1). All parks acquiring NP designation in our sample period are nature parks - thus, restricting attention to nature parks might make the treatment and comparison groups more similar. We also pursued the same objective in a different way, by restricting the sample to parks that were national monuments in 1969. The rationale for this is that all parks that acquired NP designation in our sample period were initially designated national monuments. This restriction leads to our smallest sample, dropping over 60% of observations. In both of these specifications we found very similar results to those presented earlier (columns 4 and 5).

As an additional robustness check, we omitted from the regressions parks with a Parkway designation. These NPS units extend over 20 counties and may have a substantial amount of non-recreational traffic. We also repeated the regression without the 26 combined parks described in Section 2 of the paper. These again yielded similar estimates to our original findings for both employment and income (columns 6 and 7).

Finally, we limited the sample to the 26 states that had a NP before the end of our sample period (2017). These states may have different natural attributes from others, and could

[1] Note that because the year of treatment varies by park, the comparison group varies as well: for a designation change in year t, the comparison group is the set of parks not experiencing a designation change in year t.

therefore have their economic outcomes follow different trends.[2] Dropping these states also changes the values for parks located partially in these states as these values now aggregate a different set of counties. Despite these changes, the results, shown in column 8, remain very similar.

In addition to these changes to the sample, we also estimated treatment effects controlling for park-specific linear trends. Here treatment effects are identified off of departures in employment or income relative to their park-specific trends in treated parks vs. the comparison parks. The estimates, shown in column 9, remain robust.

Park openings as potential confounders. Focusing on parks that already existed at the start of our sample period allows us to ignore confounds that may impact the establishment (as opposed to the redesignation) of parks. There is a remaining concern, however, because parks opening during our sample period could impact some of the counties which also contain parks that we do include in the analysis.[3] This would be of particular concern if the new park opening happened in the same year as the NP designation of the existing park, but this is never the case in the data. As an additional robustness check, we repeat the analysis dropping all parks that share a county with a park established after the start of our sample period. We find that our findings above for employment and income are robust (column 10). We also estimated a specification that used both the NP designation event and the park opening event in the same event study. The estimates for both treatments are similar to those shown individually on Figures 4 and 5 in the paper (Figure A.9).

World Heritage Sites as potential confounders. The UNESCO World Heritage Site (WHS) designation, which we study in Section 5.2 of the paper, could in principle have a confounding effect on NP designations. However, none of the parks experiencing an NP designation change in our regressions received WHS designation during our period of study. Therefore, controlling the WHS designation event (and its lags and leads) does not affect our results (column 11).

Changing the sample by one park at a time. We checked that our main results were not driven by a designation change in any one park by dropping one treated park at a time (Figure A.10).

Recall that we have excluded Theodore Roosevelt NP (North Dakota) from the estimation. Figure A.10 shows that *including* this park would double the magnitude of our

[2] Note however that, as of 2023, two states that did not have a NP in 2017 (Indiana and West Virginia) have since acquired one. Not having a park by the end of our sample period does not imply that a state could not have one in the future.

[3] For example, Death Valley NP (established in 1933, designated NP in 1994) overlaps 4 counties. One of these counties experiences a new park opening in 1992: Manzanar NHS. Although Manzanar is not included in our sample, its opening could impact the outcomes we measure for Death Valley.

estimates. It turns out that in the same year that this park acquired NP designation (1978), a series of oil discoveries occurred in the two counties where the park is located.[4] Between 1977 and 1978, mining employment in these two counties almost doubled, and continued to increase steeply over the next five years. This shock likely leads us to *overestimate* the impact of NP designation when this park is included in the sample.

[4]Roosevelt NP is comprised of two units, a North unit located in McKenzie county and a South unit in Billings county. Exploratory drilling had been taking place between the two units along a formation known as Billings Nose. In 1978, three fields along this formation struck oil for the first time: Bull Moose, close to the North unit, and TR Billings and Four Eyes, close to the South unit (Gerhard and Anderson, 1979).

Table A.7: The effect of NP designation on employment, robustness

Years since change	(1) main	(2) balanced1	(3) balanced2	(4) nature only	(5) nm only	(6) no pway	(7) no combined	(8) states w/NP only	(9) park trends	(10) drop all openings	(11) control for WHS	(12) state-year FE
≤-5	0.030		0.027	0.030	0.015	0.030	0.041	0.030	0.016	0.028	0.031	0.009
	(0.023)		(0.024)	(0.025)	(0.028)	(0.023)	(0.027)	(0.023)	(0.030)	(0.038)	(0.024)	(0.025)
-4	0.001		-0.002	-0.003	-0.001	0.001	0.003	0.000	-0.002	0.014	0.001	-0.016
	(0.009)		(0.009)	(0.011)	(0.011)	(0.009)	(0.010)	(0.009)	(0.015)	(0.016)	(0.009)	(0.011)
-3	0.004		0.001	-0.000	0.003	0.004	0.006	0.003	0.003	0.013	0.004	-0.007
	(0.007)		(0.007)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.013)	(0.011)	(0.007)	(0.010)
-2	0.005	0.005	0.002	0.002	0.005	0.005	0.008	0.003	0.004	0.015	0.005	0.004
	(0.005)	(0.005)	(0.003)	(0.007)	(0.006)	(0.005)	(0.007)	(0.005)	(0.010)	(0.011)	(0.005)	(0.010)
0	0.000	-0.000	0.004	-0.002	-0.002	0.000	0.001	-0.000	-0.001	0.002	0.000	0.004
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)	(0.007)
1	0.015**	0.014**	0.010*	0.013*	0.012	0.015**	0.017**	0.015**	0.013**	0.027***	0.015**	0.012
	(0.006)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)	(0.006)	(0.010)
2	0.024***	0.021**	0.020***	0.020**	0.019*	0.024***	0.027***	0.024***	0.020**	0.039***	0.024***	0.021*
	(0.007)	(0.008)	(0.007)	(0.008)	(0.010)	(0.007)	(0.008)	(0.007)	(0.008)	(0.009)	(0.007)	(0.012)
3	0.032***	0.031**	0.027**	0.027**	0.025*	0.032***	0.037***	0.032***	0.029**	0.054***	0.032***	0.028*
	(0.011)	(0.013)	(0.012)	(0.012)	(0.013)	(0.011)	(0.011)	(0.011)	(0.014)	(0.015)	(0.011)	(0.014)
4	0.037***	0.035***	0.026***	0.034***	0.041***	0.037***	0.040***	0.037***	0.036***	0.051***	0.037***	0.030**
	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.012)	(0.011)	(0.013)	(0.016)	(0.011)	(0.013)
5≤	0.031	0.022	0.029	0.030	0.046*	0.031	0.032	0.029	0.024	0.017	0.031	0.021
	(0.022)	(0.022)	(0.025)	(0.022)	(0.025)	(0.022)	(0.025)	(0.022)	(0.025)	(0.022)	(0.021)	(0.023)
Adj. R^2	0.93	0.92	0.93	0.92	0.90	0.93	0.95	0.93	0.98	0.93	0.93	0.93
N obs.	9,024	9,212	8,928	4,272	3,312	8,928	7,776	6,864	9,024	8,112	9,024	9,024
N parks	188	188	186	89	69	186	162	143	188	169	188	188

Notes: Event study estimates of the impact of NP designation on log employment. Coefficients represent changes relative to year -1 (the year before the designation change). Controls include park and year fixed effects, population density, the share of population under 19 and above 65, precipitation and droughts, and the park's age squared. Column 1 repeats the main specification (column 2 in Table 2 in the paper). Column 2 balances the sample by including the year 1969 (which requires dropping the population share controls) and binning the event study coefficients at -3 (and +5). Column 3 balances the sample by excluding Arches NP and Capitol Reef NP. Column 4 restricts the sample to nature parks and column 5 to parks that were national monuments in 1969. Column 6 excludes parkways and 7 excludes all combined parks. Column 8 drops all states that do not have a NP before 2017. Column 9 includes park-specific linear trends. Column 10 drops parks that share a county with a park opened after 1970. Column 11 controls for the World Heritage Site designation event (and its lags and leads). Column 12 includes state-year fixed effects. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively

Table A.8: The effect of NP designation on income, robustness

Years since change	(1) main	(2) balanced1	(3) balanced2	(4) nature only	(5) nm only	(6) no pway	(7) no combined	(8) states w/NP only	(9) park trends	(10) drop all openings	(11) control for WHS	(12) state-year FE
≤-5	0.036		0.041	0.017	0.000	0.036	0.047	0.031	0.035	0.017	0.039	0.034
	(0.036)		(0.033)	(0.037)	(0.040)	(0.036)	(0.040)	(0.036)	(0.040)	(0.048)	(0.036)	(0.043)
-4	0.020		0.025*	0.020	0.017	0.020	0.028	0.022	0.039	0.029	0.020	0.006
	(0.020)		(0.014)	(0.022)	(0.024)	(0.020)	(0.020)	(0.020)	(0.033)	(0.033)	(0.021)	(0.026)
-3	0.028		0.033**	0.030	0.026	0.028	0.033	0.028	0.050	0.035	0.028	0.009
	(0.019)		(0.013)	(0.022)	(0.024)	(0.019)	(0.020)	(0.019)	(0.033)	(0.033)	(0.020)	(0.023)
-2	0.007	0.012	0.012**	0.009	0.011	0.007	0.011	0.008	0.031	0.003	0.007	0.008
	(0.016)	(0.007)	(0.005)	(0.019)	(0.020)	(0.016)	(0.016)	(0.016)	(0.031)	(0.027)	(0.016)	(0.020)
0	0.000	0.001	0.006	-0.001	-0.002	0.000	-0.000	-0.001	0.001	-0.002	0.000	0.007
	(0.006)	(0.007)	(0.005)	(0.007)	(0.008)	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)	(0.006)	(0.010)
1	0.024**	0.025**	0.013**	0.024**	0.023*	0.024**	0.025**	0.023**	0.025*	0.036**	0.024**	0.029
	(0.010)	(0.010)	(0.005)	(0.011)	(0.013)	(0.010)	(0.011)	(0.010)	(0.014)	(0.016)	(0.010)	(0.018)
2	0.048***	0.047***	0.036***	0.047**	0.043**	0.048***	0.051***	0.048***	0.052**	0.071***	0.047***	0.051**
	(0.017)	(0.017)	(0.011)	(0.018)	(0.021)	(0.017)	(0.017)	(0.017)	(0.023)	(0.024)	(0.017)	(0.023)
3	0.062***	0.061***	0.050**	0.061***	0.059**	0.062***	0.067***	0.061***	0.066**	0.089***	0.062***	0.052**
	(0.020)	(0.021)	(0.019)	(0.023)	(0.027)	(0.020)	(0.020)	(0.020)	(0.026)	(0.029)	(0.020)	(0.024)
4	0.060**	0.057**	0.038**	0.064**	0.076***	0.060**	0.063**	0.061**	0.070**	0.079**	0.059**	0.064**
	(0.024)	(0.025)	(0.019)	(0.026)	(0.028)	(0.024)	(0.025)	(0.024)	(0.034)	(0.039)	(0.024)	(0.026)
5≤	0.029	0.017	0.037	0.039	0.038	0.029	0.027	0.031	0.087	0.029	0.027	0.032
	(0.030)	(0.029)	(0.027)	(0.032)	(0.037)	(0.030)	(0.032)	(0.031)	(0.060)	(0.037)	(0.030)	(0.040)
Adj. R^2	0.87	0.86	0.87	0.86	0.82	0.86	0.88	0.88	0.93	0.86	0.87	0.88
N obs.	9,024	9,212	8,928	4,272	3,312	8,928	7,776	6,864	9,024	8,112	9,024	9,024
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Notes: Event study estimates of the impact of NP designation on log income. Coefficients represent changes relative to year -1 (the year before the designation change). Controls include park and year fixed effects, population density, the share of population under 19 and above 65, precipitation and droughts, and the park's age squared. Column 1 repeats the main specification (column 5 in Table 2 in the paper). Column 2 balances the sample by including the year 1969 (which requires dropping the population share controls) and binning the event study coefficients at -3 (and +5). Column 3 balances the sample by excluding Arches NP and Capitol Reef NP. Column 4 restricts the sample to nature parks and column 5 to parks that were national monuments in 1969. Column 6 excludes parkways and 7 excludes all combined parks. Column 8 drops all states that do not have a NP before 2017. Column 9 includes park-specific linear trends. Column 10 drops parks that share a county with a park opened after 1970. Column 11 controls for the World Heritage Site designation event (and its lags and leads). Column 12 includes state-year fixed effects. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively

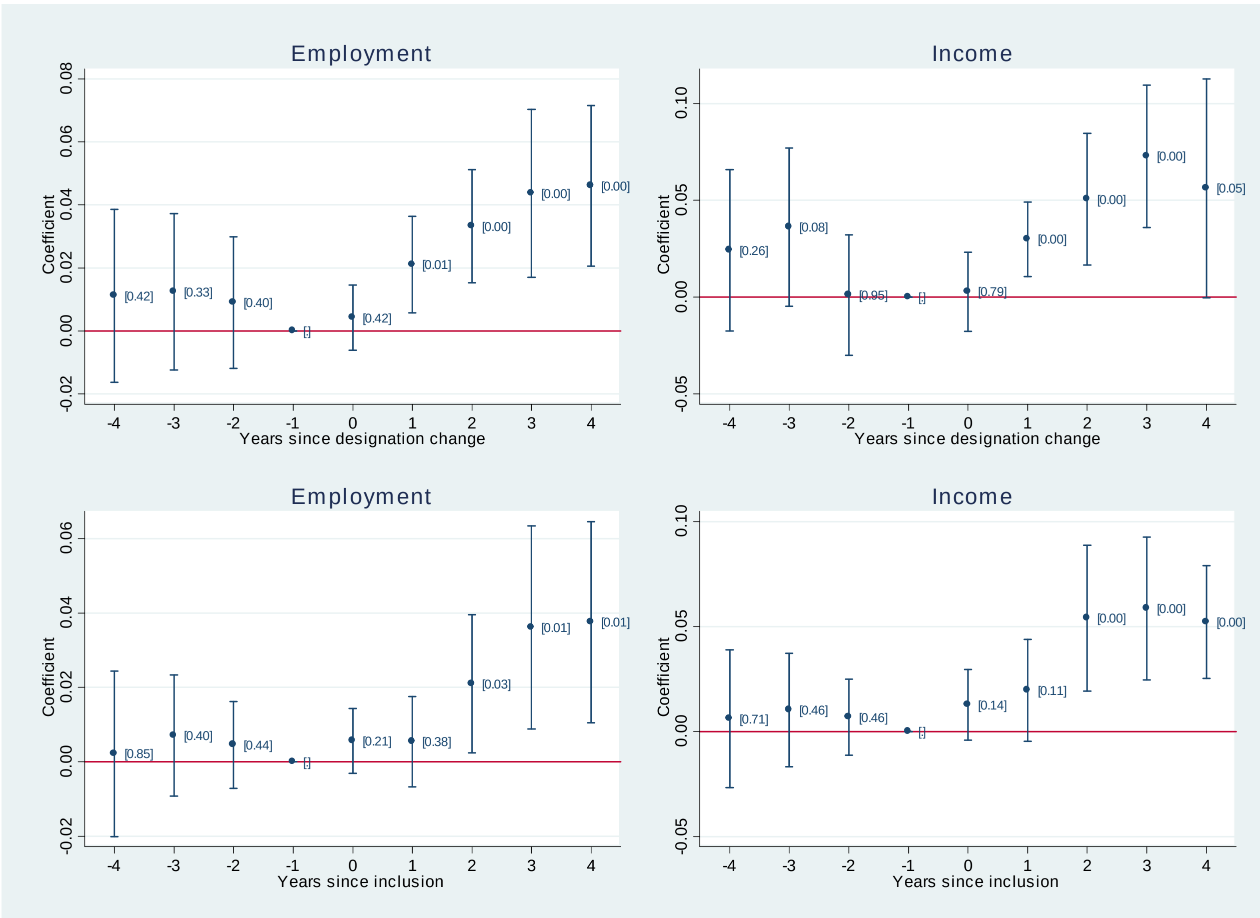

Figure A.9: The impact of NP designation and park opening, joint estimates

Event study estimates for the impact of NP designation and park opening. The left panels show estimates from the employment regression, the right panels show estimates from the income regression. Both regressions include leads and lags for both treatments. The top panels show the designation change coefficients, the bottom panels the park opening coefficients. Estimates are relative to the year before the change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 129,024 (employment) 129,010 (income).

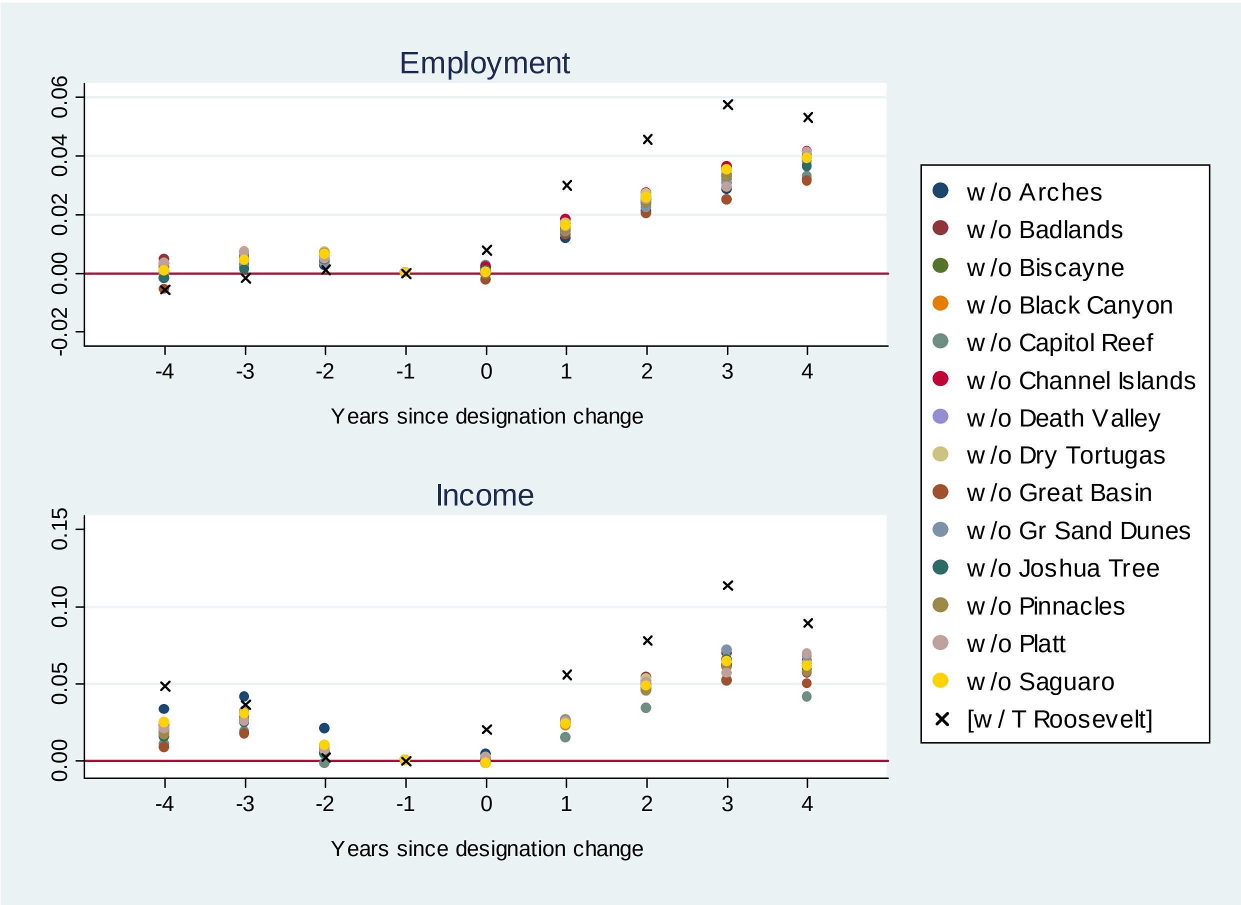

Figure A.10: NP designation, employment and income, changing the sample 1 park at a time

Event study coefficient estimates for the impact of NP designation on log employment and log income. Specifications drop one NP designation change at a time. The last specification adds T. Roosevelt NP, which is excluded from the main sample. Estimates are relative to the year before the designation change.

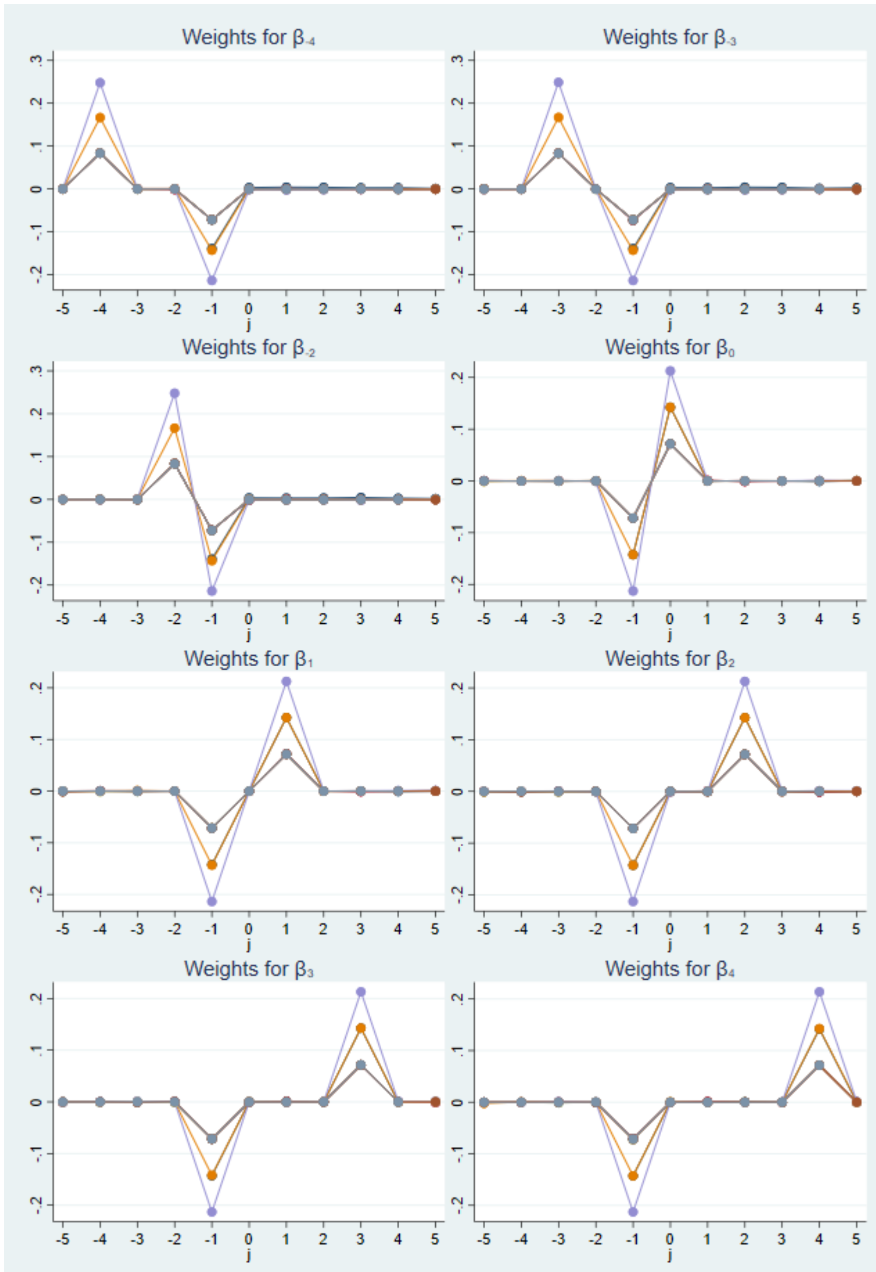

Figure A.11: Weights of different treatment cohorts' event windows in the NP designation estimates

Each panel shows the weights of different treatment cohorts' event windows in the estimates of one of the event study parameters (β_j) based on Sun and Abraham (2021).

Treatment effect heterogeneity. By estimating an event study, we are explicitly allowing for heterogeneity of the treatment effects across time periods (i.e., we are not assuming that a unit treated 3 periods ago has its outcomes on a path that is parallel to those of a unit treated 4 periods ago). As with most policies in other contexts, the treatment effect is also likely to be heterogenous across units, both because the affected units are heterogenous (counties are different from each other) and because the policy itself is heterogenous (parks are different entities). While conceptually this does not preclude the estimation of meaningful average treatment effects (ATEs), a recent literature shows that in the presence of heterogeneity across units, whether the estimates are meaningful must not be taken for granted (Borusyak and Jaravel (2017), Goodman-Bacon (2018), Sun and Abraham (2021), de Chaisemartin and D'Haultfoeuille (2020)). In short, these papers show that, when units (or cohorts of units) are treated at different points in time, difference-in-differences estimates of ATEs, which are weighted averages of the treatment effects across cohorts and time periods, may not be meaningful. For example, weights can be negative, resulting in an overall ATE that has the opposite sign of the individual treatment effects.

In the specific context of event studies, Sun and Abraham (2021) show that problematic ATE estimates can arise when some cohort's treatment effect in period j' relative to treatment has a large weight in the estimate of the period-j ATE (β_j in Eqn (1) in the paper). For example, the problem could arise if some park-observations 3 periods after NP designation had a large weight in the estimate for β_1, the ATE of NP designation 1 year after the designation change. Sun and Abraham (2021) propose a method to evaluate how serious this concern is by calculating the weights of different cohorts in each treatment effect β_j. Ideally, for each treatment effect β_j and each treatment cohort, the weights should only be nonzero for relative period j, and the excluded category (relative period -1 in our case). Remarkably, in each case this is exactly what we find.

Figure A.11 presents the weights in our main specifications. Each panel shows the weights of every treatment cohort in a different coefficient estimate β_j. We have 10 treatment cohorts: one with 3 treated units, two with 2 treated units and seven with 1 treated unit. Each panel shows 10 lines corresponding to the weights of each of these cohorts' observations from relative periods $j \in \{-5, ..., 5\}$. The line with the largest spike is the 3-unit cohort, the middle spikes are the 2-unit cohorts, and the lowest spikes are the 1-unit cohorts.[5] As can be seen, on each panel only observations from relative period j receive sizeable positive weights, and only relative period -1 receives a sizeable negative weight. This indicates that our event study estimates provide meaningful ATEs (the average impact of NP designation

[5] Recall that one of the units, Platt NP, experienced a negative treatment (loss of NP designation). On the figure, we multiply the weights for this park by -1 for ease of comparison with the rest.

in relative period j) even if treatment effects are heterogeneous across parks.

Intuitively, treatment effect heterogeneity is likely to lead to estimation problems when many cohorts have overlapping event windows. In our case, because (i) we have a large number of parks in the comparison groups, and (ii) designation changes are spread out over time, overlapping event windows arise infrequently in the data. Our estimates can be interpreted as meaningful averages of potentially heterogenous treatment effects.

As a second way to check that our results are valid in the presence of treatment effect heterogeneity, we use the estimator proposed by Callaway and Sant'Anna (2021). Here we use only never-treated and not-yet-treated parks for comparison, i.e., we do not impose a parallel trends assumption with either already-treated or always-treated parks (the latter group is excluded from the regressions). The method does not allow for time-varying control variables, we therefore use as dependent variable the log of per-capita income or employment, and drop the demographic, weather, and park-age controls. The results are in Table A.12. The treatment effect estimates are similar in magnitude to our corresponding earlier findings (columns 1 and 4 in Table 2 in the paper).

3.2 Park opening

Unless stated otherwise, all estimates described in this section are in Tables A.13 (employment) and A.14 (income).

Different comparison groups and identification. We investigate limiting the sample to units that may be more comparable to each other. Unlike for NP designation, we do not have a story that would allow us limit the comparison group based on a priori considerations. Instead, we attempt to limit the sample to more comparable units based on propensity scores. Specifically, we estimate the propensity score for ever having a park compared to never having a park. We then reestimate our regressions trimming the sample to exclude units with propensity scores in the tails of the distribution (in general, propensity scores close to 0 in the control group and propensity scores close to 1 in the treatment group).[6]

We estimate propensity scores using a probit regression of ever having a park on the 1970 values of: log population density and its square, water area divided by land area, all available weather indicators (precipitation, temperature, minimum temperature, maximum temperature, drought severity, cooling degree days, heating degree days), the fraction of population aged 0-19, the fraction aged 65+, and state fixed effects. We then exclude from our regressions units with propensity scores below P_{min} and above P_{max}, and we follow

[6]Using the propensity scores to trim the sample rather than to weight observations follows the recommendation of Imbens and Rubin (2015) and makes our results less sensitive to the exact specification of the propensity score regression.

Table A.12: Treatment effect estimates using Callaway and Sant'Anna (2021)

Years since	NP designation		Park opening	
change	Employment	Income	Employment	Income
Pre				
-4	-0.022**	-0.023*	0.000	0.014
	(0.011)	(0.012)	(0.005)	(0.011)
-3	0.003	0.006	0.005	0.003
	(0.006)	(0.006)	(0.005)	(0.009)
-2	-0.002	-0.019	-0.002	-0.004
	(0.005)	(0.013)	(0.004)	(0.008)
-1	-0.005	-0.015**	-0.004	-0.006
	(0.004)	(0.006)	(0.006)	(0.009)
Post				
0	0.000	0.000	0.006	0.012
	(0.006)	(0.007)	(0.004)	(0.008)
1	0.014*	0.022**	0.006	0.019
	(0.008)	(0.01)	(0.006)	(0.013)
2	0.021**	0.044**	0.021**	0.053***
	(0.010)	(0.018)	(0.009)	(0.018)
3	0.027*	0.051**	0.036**	0.056***
	(0.014)	(0.022)	(0.014)	(0.018)
4	0.036***	0.064***	0.038***	0.050***
	(0.013)	(0.024)	(0.014)	(0.015)
N. obs	7,584	7,584	121,024	121,024
N. units	158	158	2,522	2,522

Notes: Callaway and Sant'Anna (2021) treatment effect estimates. Post coefficients represent changes relative to period -1. Pre coefficients represent changes relative to the previous period. The dependent variable is log per-capita employment or income. No control variables are included (other than park and year fixed effects). The comparison groups include never-treated and not-yet-treated parks. Robust standard errors clustered by park in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

two alternative procedures to determine these thresholds. First, we set $P_{\min}$ to be the lowest propensity score in the treatment group, and $P_{\max}$ the highest propensity score in the control group ("common support" trimming). This leaves 2378 units, 29 of which are treated during our sample period. Second, we follow a procedure proposed by Crump et al. (2009) which computes $P_{\min}$ and $P_{\max} = 1 - P_{\min}$ based on properties of the sampling variance of the average treatment effect (see Imbens and Rubin, 2015, Ch 16). This trims over 50% of our sample, leaving 1274 units, 24 of which are treated. In both of these cases we obtain similar estimates to those in our main specification (columnns 2 and 3).

As above, we also estimated specifications that control for unit-specific linear trends. These are shown in column 4 and yield similar results to the main estimates.

Changing the threshold for reporting visitors. In order to exclude openings that were mere formalities, in the main regressions we exclude newly opened parks that did not begin reporting visitors within 5 years. In column 5 we increase this threshold to 10 years, and in column 6 we remove it completely. While most coefficients remain significant, their magnitude declines progressively as the threshold is relaxed. This is consistent with parks that do not report visitors for 5+ years experiencing little actual change in their status and therefore having no significant impact on the local economy.

Extending the sample. As explained in the text, our park opening estimates reflect the impact of parks opened between 1975-2013 to ensure that our sample is not subject to imbalances caused especially by the many parks opening in the late 60-s and early 70-s following Project 66. We checked whether our estimates change by including more treated parks. Specifically, we include 4 parks opened in 1974, 5 in 1972, and 2 after 2013, raising the number of treated parks in the regressions from 31 to 42. To retain balance in the estimation of leads, we bin the event study indicators at -3 instead of -5. The results shown in column 7 are similar to those obtained earlier.

Changing the sample by one park at a time. We checked that our main results were not driven by any one park opening by dropping one treated park at a time (Figure A.15).

Treatment effect heterogeneity. To evaluate if treatment effect heterogeneity would pose a problem for interpreting our estimates, we again compute the Sun and Abraham (2021) weights of the different treatment cohorts' weights in the estimates (see the previous section for details).

We now have 18 treatment cohorts, ranging from 1 to 5 treated units. Figure A.16 shows the Sun and Abraham (2021) weights for the different cohorts' relative periods $j \in \{-5, ..., 5\}$ in the event study coefficient estimates. We again see that for each coefficient β_j, only observations from relative period j have large positive weights, and only relative period -1 has a large negative weight. We conclude that our estimates are meaningful averages even if the treatment effects are heterogenous across units.

Here too, the Callaway and Sant'Anna (2021) estimates are similar in magnitude to the earlier findings (Table A.12 vs. columns 1 and 4 in Table 3 in the paper).

Table A.13: The effect of park opening on employment, robustness

Years since change	Main (1)	Pscore trimmed 1 (2)	Pscore trimmed 2 (3)	Unit trends (4)	10 year visitor threshold (5)	No visitor threshold (6)	More treated parks (7)	State-year FE (8)
$\leq$-5	-0.016	-0.021	-0.013	0.002	-0.018	-0.023*		-0.018
	(0.019)	(0.020)	(0.023)	(0.018)	(0.015)	(0.013)		(0.018)
-4	0.002	-0.001	0.001	0.007	0.000	-0.005		-0.003
	(0.011)	(0.012)	(0.010)	(0.012)	(0.009)	(0.007)		(0.010)
-3	0.007	0.005	0.005	0.010	0.010	0.006		0.002
	(0.008)	(0.009)	(0.007)	(0.009)	(0.007)	(0.006)		(0.008)
-2	0.005	0.003	0.001	0.006	0.006	0.003	0.000	0.004
	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
0	0.006	0.007	0.006	0.004	0.003	0.003	0.001	0.005
	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.005)	(0.004)
1	0.005	0.007	0.004	0.002	0.007	0.006	0.001	0.003
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.004)	(0.005)	(0.006)
2	0.021**	0.023**	0.023**	0.016	0.017**	0.013**	0.014*	0.015*
	(0.009)	(0.010)	(0.011)	(0.010)	(0.008)	(0.006)	(0.008)	(0.008)
3	0.036***	0.040***	0.031**	0.029**	0.025**	0.020**	0.028**	0.026**
	(0.014)	(0.014)	(0.012)	(0.013)	(0.011)	(0.008)	(0.011)	(0.013)
4	0.037***	0.040***	0.033***	0.030**	0.028***	0.022***	0.025**	0.027**
	(0.014)	(0.015)	(0.012)	(0.012)	(0.010)	(0.009)	(0.012)	(0.012)
5$\leq$	0.004	0.009	0.023	-0.003	0.009	0.017	0.014	-0.001
	(0.017)	(0.018)	(0.019)	(0.015)	(0.015)	(0.013)	(0.016)	(0.020)
Adj. R^2	0.85	0.85	0.88	0.93	0.85	0.85	0.85	0.87
N obs.	129,072	114,144	61,152	129,072	129,744	130,368	129,600	129,072
N units	2,689	2,378	1,274	2,689	2,703	2,716	2,700	2,689

Notes: Event study estimates of the impact of park opening on log employment. Coefficients represent changes relative to year -1 (the year before the change). All specifications control for park and year fixed effects, population density, the share of population under 19 and above 65, precipitation and droughts. Column 1 repeats the main specification (column 2 in Table 3 in the paper). Columns 2 and 3 trim the sample based on estimated propensity scores as described in the text. Column 2 trims to a common support, and column 3 trims to thresholds computed using the Crump et al. (2009) procedure. Column 4 includes unit-specific time trends. Column 5 raises the visitor reporting threshold to 10 years after opening and column 6 removes the threshold. Column 7 adds parks opened in 1972, 1974, or after 2013 and bins the event study coefficients at -3 (and +5). Column 8 includes state-year fixed effects. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Table A.14: The effect of park opening on income, robustness

Years since change	Main (1)	Pscore trimmed 1 (2)	Pscore trimmed 2 (3)	Unit trends (4)	10 year visitor threshold (5)	No visitor threshold (6)	More treated parks (7)	State-year FE (8)
≤-5	-0.014	-0.021	-0.010	-0.028	0.000	-0.019		-0.020
	(0.030)	(0.031)	(0.034)	(0.028)	(0.025)	(0.022)		(0.031)
-4	0.006	0.002	0.011	0.008	-0.006	-0.010		-0.005
	(0.017)	(0.018)	(0.017)	(0.017)	(0.016)	(0.013)		(0.020)
-3	0.010	0.009	0.007	0.012	0.009	0.002		-0.001
	(0.014)	(0.015)	(0.013)	(0.014)	(0.012)	(0.010)		(0.014)
-2	0.007	0.006	0.004	0.008	0.013	0.003	0.009	0.006
	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.010)
0	0.013	0.015*	0.017**	0.012	0.006	0.001	0.006	0.014*
	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)
1	0.020	0.023*	0.021*	0.018	0.021*	0.010	0.006	0.016
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.013)
2	0.054***	0.056***	0.055***	0.051***	0.036*	0.021	0.044***	0.046***
	(0.018)	(0.019)	(0.019)	(0.018)	(0.019)	(0.016)	(0.014)	(0.016)
3	0.059***	0.061***	0.061***	0.054***	0.040**	0.027*	0.050***	0.046**
	(0.017)	(0.018)	(0.017)	(0.017)	(0.016)	(0.015)	(0.015)	(0.018)
4	0.052***	0.054***	0.063***	0.047***	0.036**	0.022	0.043***	0.029**
	(0.014)	(0.014)	(0.013)	(0.015)	(0.017)	(0.016)	(0.012)	(0.014)
5≤	0.055*	0.061**	0.090***	0.044**	0.057**	0.055**	0.066**	0.035
	(0.030)	(0.031)	(0.032)	(0.021)	(0.026)	(0.022)	(0.028)	(0.034)
Adj. R^2	0.72	0.72	0.77	0.82	0.72	0.72	0.72	0.77
N obs.	129,058	114,130	61,141	129,058	129,730	130,354	129,586	129,058
N units	2,689	2,378	1,274	2,689	2,703	2,716	2,700	2,689

Notes: Event study estimates of the impact of park opening on log income. Coefficients represent changes relative to year -1 (the year before the change). All specifications control for park and year fixed effects, population density, the share of population under 19 and above 65, precipitation and droughts. Column 1 repeats the main specification (column 5 in Table 3 in the paper). Columns 2 and 3 trim the sample based on estimated propensity scores as described in the text. Column 2 trims to a common support, and column 3 trims to thresholds computed using the Crump et al. (2009) procedure. Column 4 includes unit-specific time trends. Column 5 raises the visitor reporting threshold to 10 years after opening and column 6 removes the threshold. Column 7 adds parks opened in 1972, 1974, or after 2013 and bins the event study coefficients at -3 (and +5). Column 8 includes state-year fixed effects. Robust standard errors clustered by park in parantheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

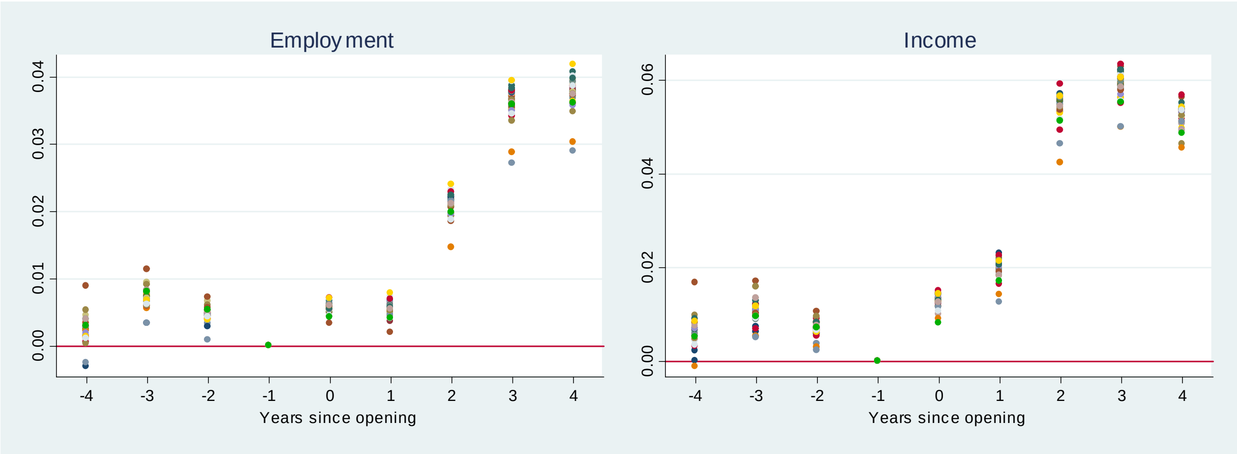

Figure A.15: Park opening, employment and income, changing the sample 1 park at time

Event study coefficient estimates for the impact of NP designation on log employment and log income. Specifications drop one park opening at a time.

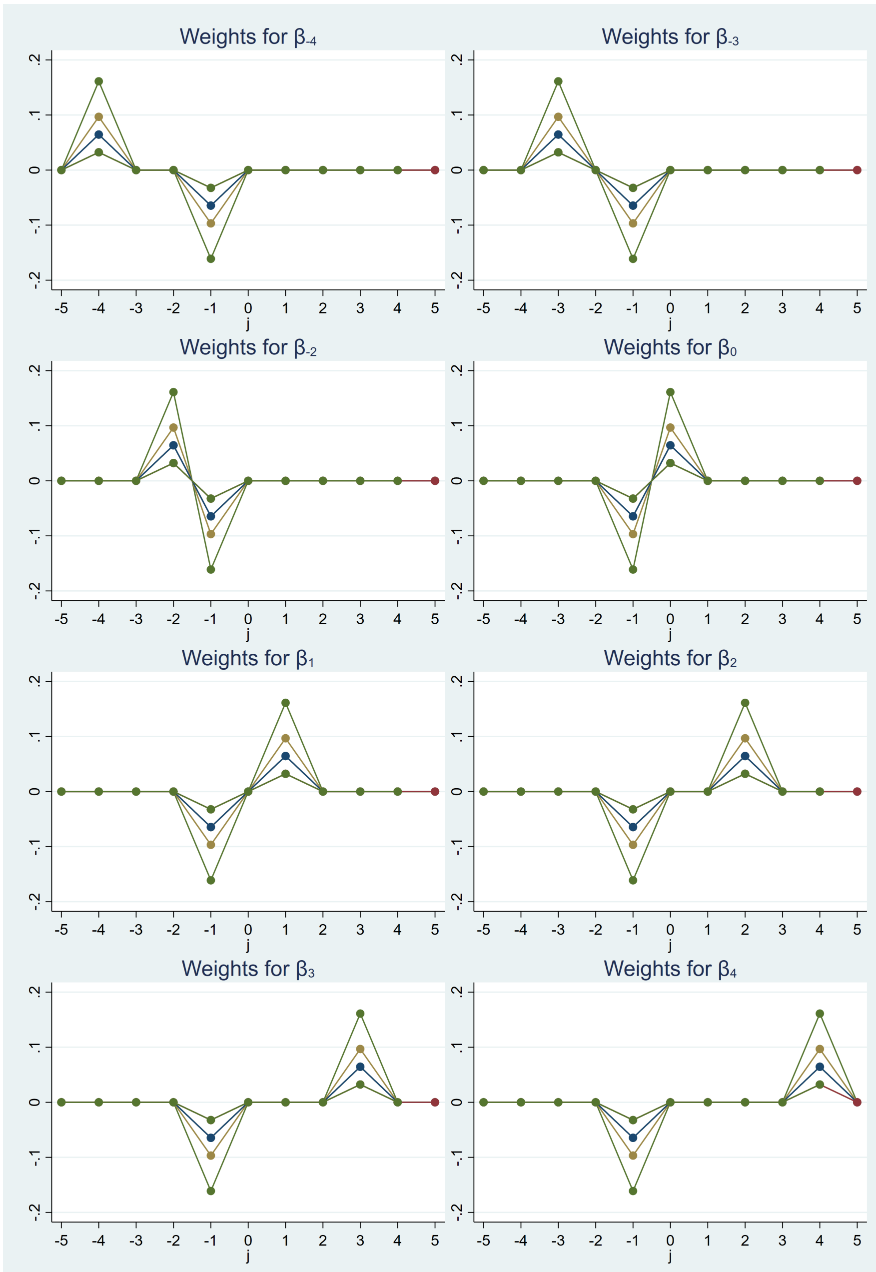

Figure A.16: Weights of different treatment cohorts' event windows in the park opening estimates

Each panel shows the weights of different treatment cohorts' event windows in the estimates of one of the event study parameters (β_j) based on Sun and Abraham (2021).

4 Detailed estimates on mechanisms

4.1 Government spending

The Census of Governments county-level series is published every five years, resulting in 9 data points per county during our period of study. To obtain meaningful event study coefficients, we combine the event indicators into groups: we estimate one parameter for years relative to the event $\tau \in [-10, -6]$, one for $\tau \in [0, 4]$, and one for $\tau \in [5, 9]$, with years $\tau \in [-5, -1]$ serving as the excluded category.

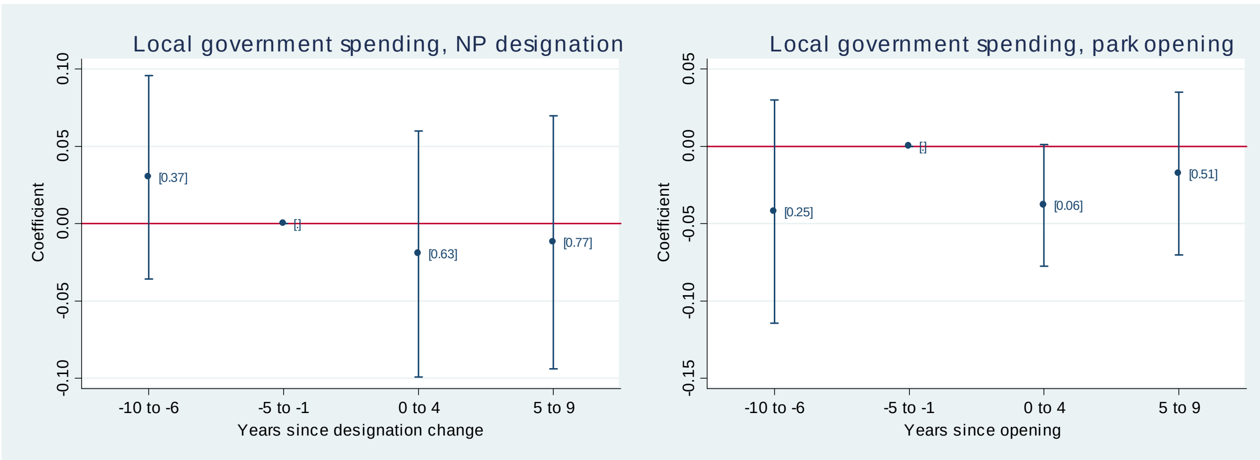

Figure A.17: No evidence of increased government spending around either treatment
Event study coefficient estimates for the impact of NP designation and park opening on local government spending (in logs). Estimates are relative to the period 1-5 years before the event. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. Years: 1972-2012. N = 1,692, 23,967.

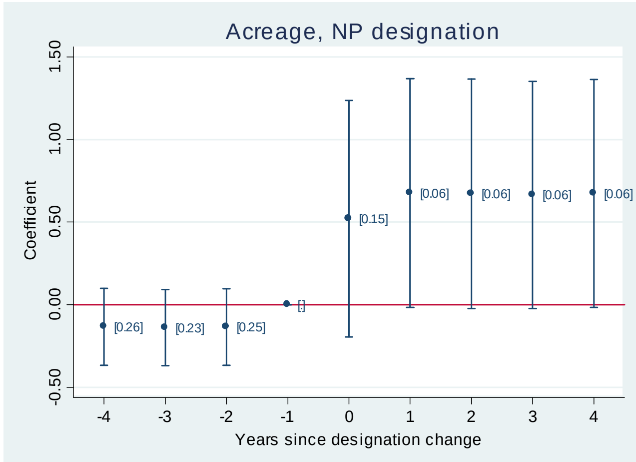

Figure A.18: The impact of NP designation on park size

Event study coefficient estimates for the impact of NP designation on log acreage. 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 = 8,973.

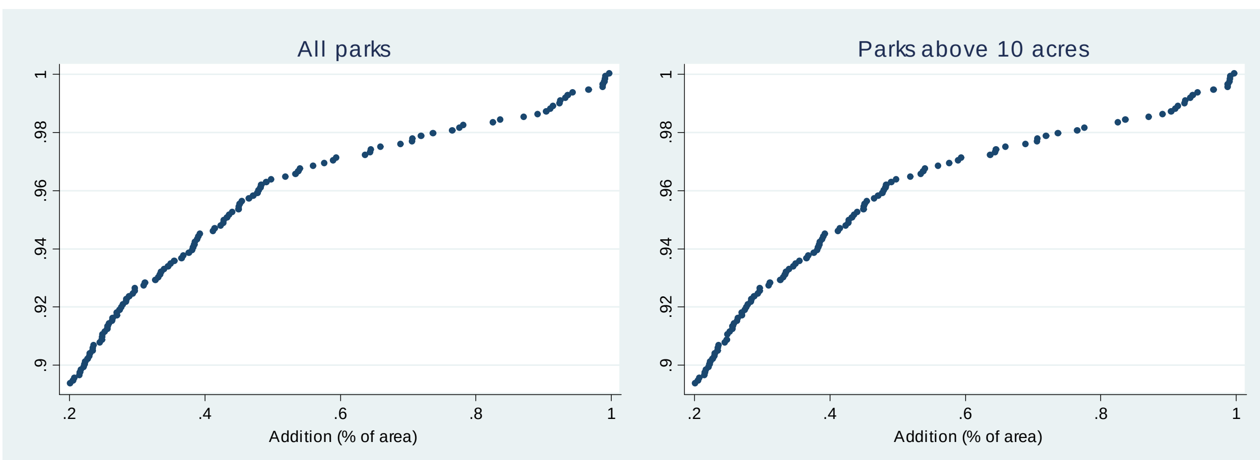

Figure A.19: Cumulative distribution of area additions

CDF of area additions in the sample for all parks (left) and parks with an average size of at least 10 acres over time (right). Additions are measured as the fraction of the park's area that was added since the previous year. 173 of the 188 parks (169 of the 183 parks larger than 10 acres) experienced some addition. Only additions above 20 percent are shown on the graph.

4.2 Changes in size

Figure A.18 shows that, on average, NP designation is accompanied by an increase in a park's acreage. To separately identify the impact of NP designation and park expansions, we need to define a meaningful park expansion event. To let the data speak to this, on Figure A.19 we plot the cumulative distribution of year-to-year area additions in our sample, measured as the fraction of the current park's area that was added since the previous year (for example, 0.5 on the horizontal axis indicates a doubling in size since the previous year). The figure shows all parks as well as excluding the smallest parks (under 10 acres) - the two distributions are nearly identical. As can be seen, approximately 10% of all additions account for a fifth or more of the park's current area. A natural break in the distribution is visible at 60%, we therefore use this threshold to define expansion events. This yields 31 expansion events in 27 parks during our period of study.

Figures A.20 and A.21 show the results of including both NP designation and park expansions as two separate events in our regressions.[7] The estimates for the NP designation event are similar to those in our main regressions. Interestingly, we find that park expansions also raise employment and income, although these effects are relatively small and short-lived.

4.3 Other designations

Figure A.22 shows the estimates for the NHP designation change, and Figure A.23 the results for World Heritage Sites. Because the WHS title is *in addition* to National Park Service designations, these regressions control for the NP designation event and its lags and leads (column 11 in Table A.7 and A.8 contains the NP designation coefficients from these regressions). WHS designation does not appear to increase visitation. It is also associated with a significant *decline* in employment and income, though the clear pre-trend in income suggests that this effect is unlikely to be causal.[8]

[7] To allow for the fact that some parks experience multiple expansions, the park-expansion event study indicator for period j relative to the event is computed as $\sum_k \mathbf{1}(\tau_{pt}^k = j)$, where τ_{pt}^k denotes time since the kth occurrence of the event. Assuming that the path of the effects is the same for a given park over time allows us to sum the $\mathbf{1}(\tau_{pt}^k = j)$ indicators and estimate a single coefficient β_j for each j. See Schmidheiny and Siegloch (2019) for a review of the different event study specifications used in the literature.

[8] On the top right panel of Figure A.23, the 10% increase in park budgets just *before* WHS designation is due to one observation, Redwood NP, which underwent a major expansion accompanied by a doubling of its budget between 1978 and 1979, just before receiving its WHS designation in 1980. Excluding that park from the sample yields flat budget estimates.

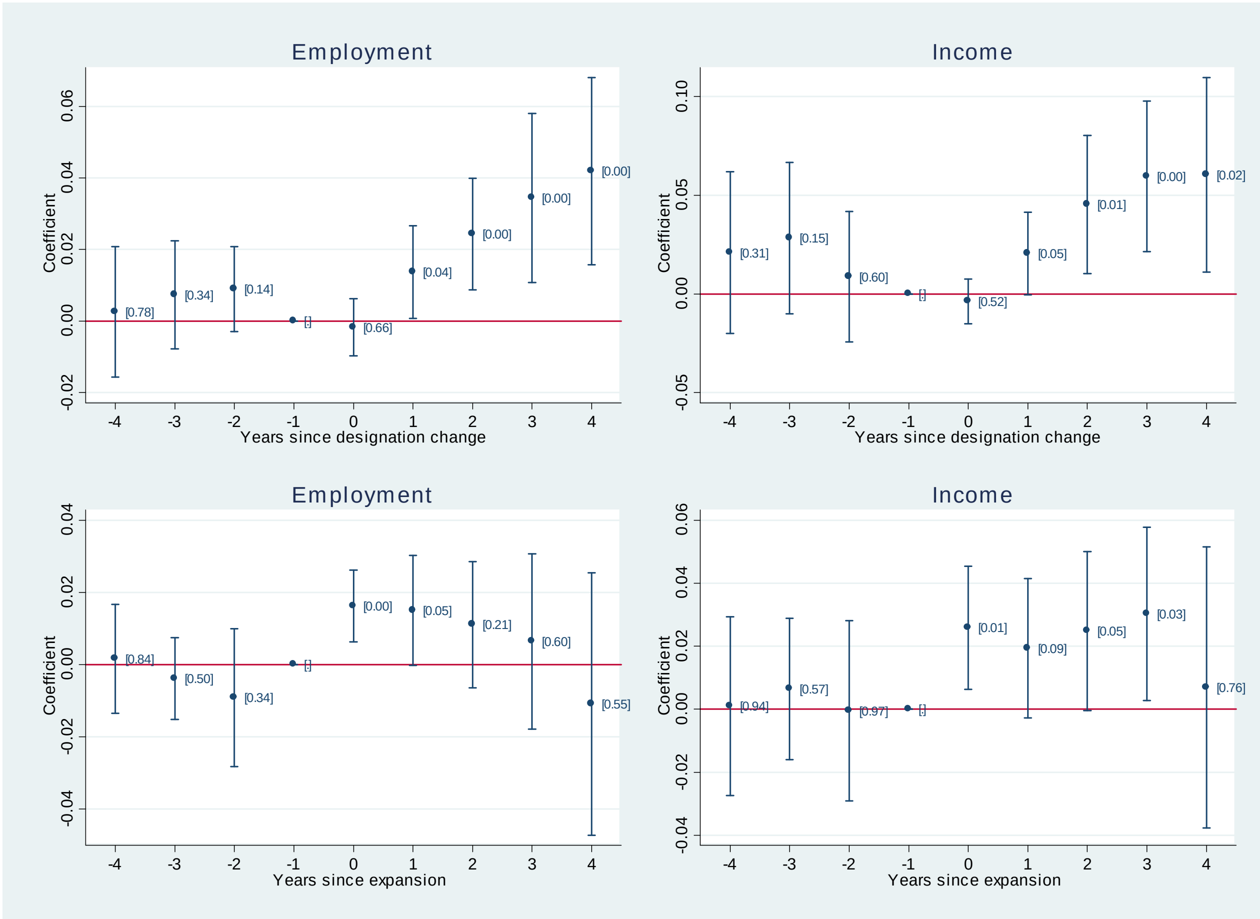

Figure A.20: The impact of NP designation and large expansions, joint estimates

Event study coefficient estimates for the impact of NP designation and park expansions on employment and income. Both regressions include leads and lags for both events. The top panels show the designation change coefficients, the bottom panels the park expansion coefficients. Estimates are relative to the year before the change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 8,969.

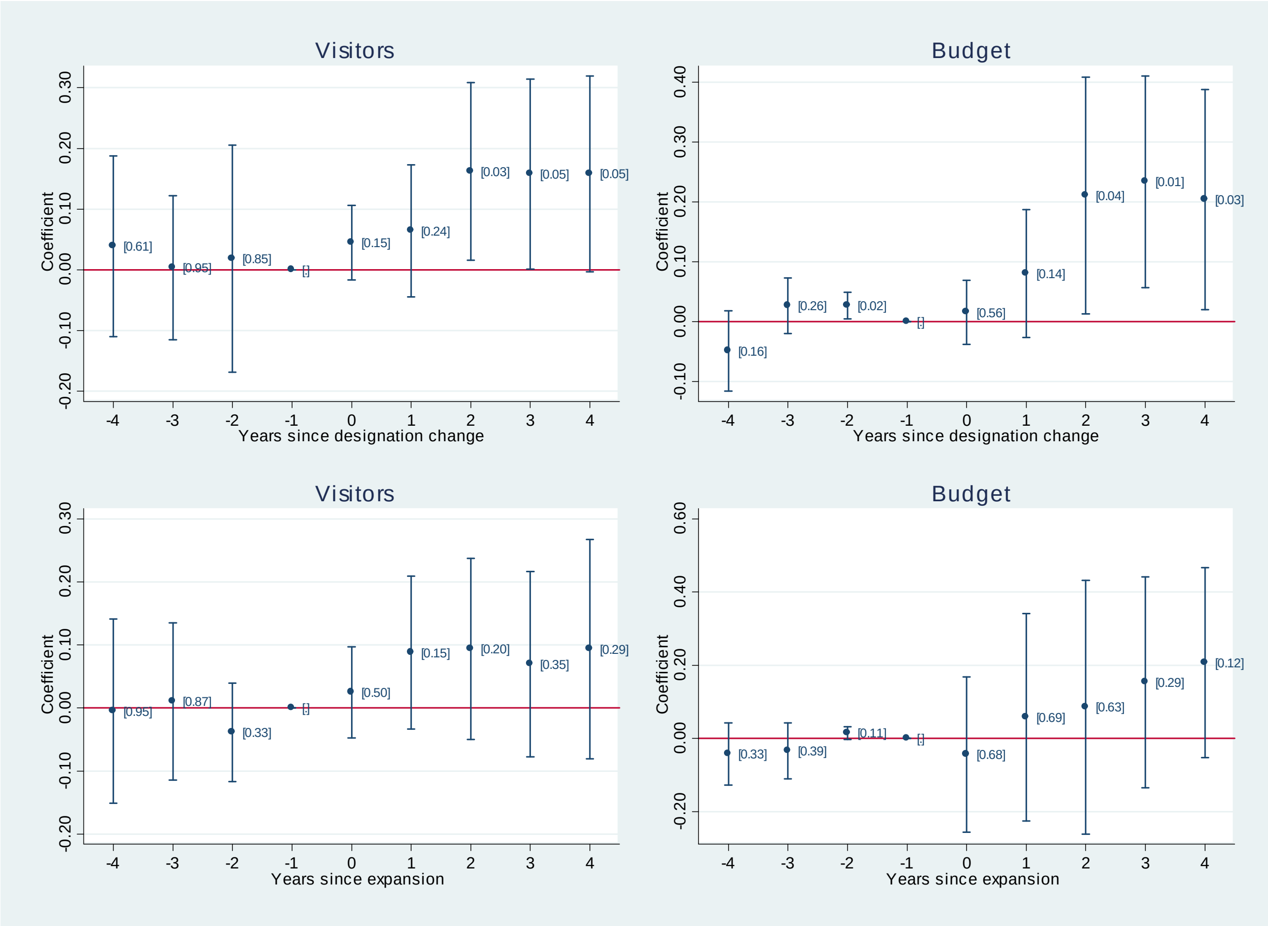

Figure A.21: The impact of NP designation and large expansions, joint estimates

Event study coefficient estimates for the impact of NP designation and park expansions on visitors and park budgets. Both regressions include leads and lags for both events. The top panels show the designation change coefficients, the bottom panels the park expansion coefficients. Estimates are relative to the year before the change. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 8,877 (visitors), 8,566 (budget).

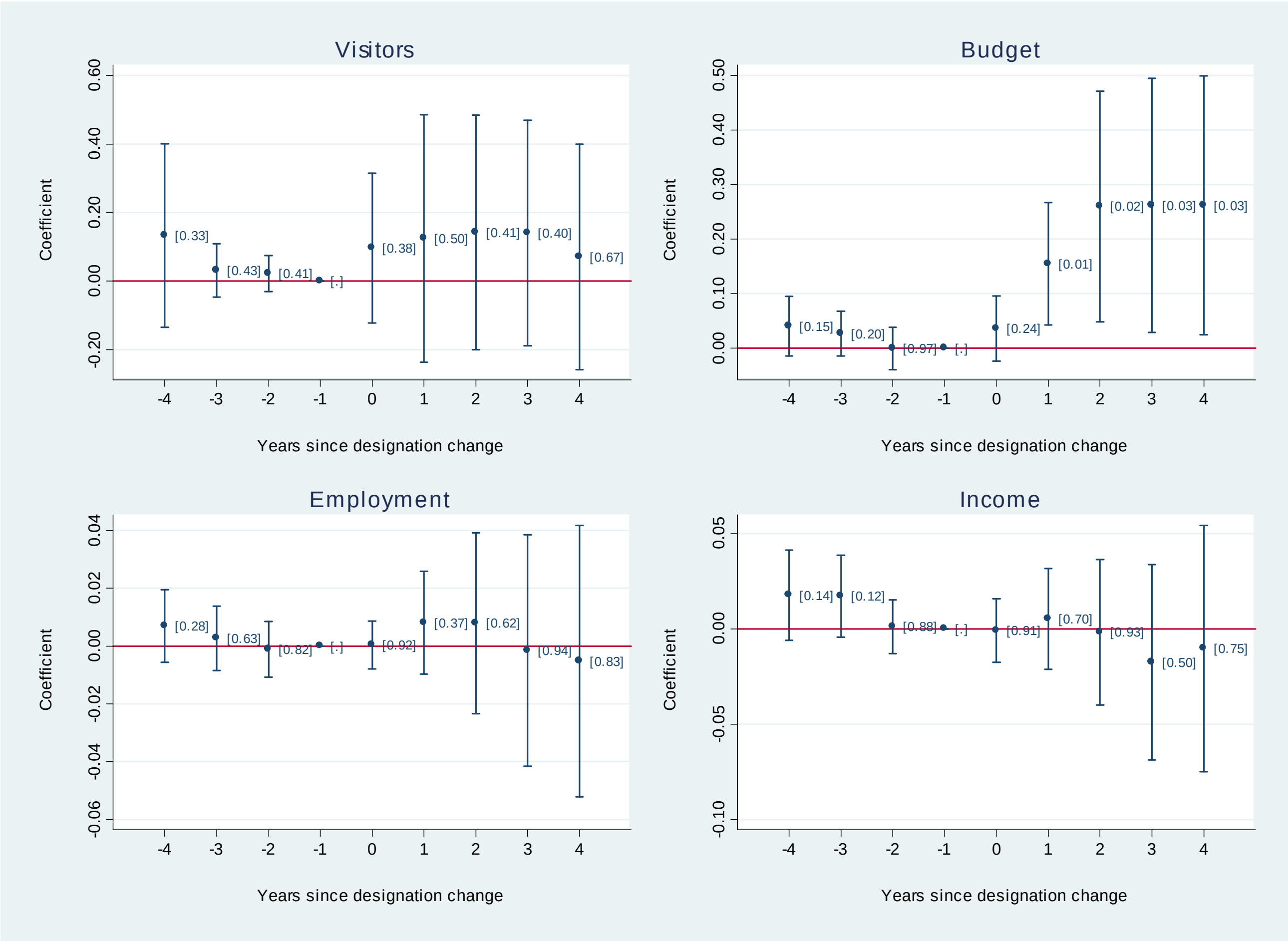

Figure A.22: No evidence that NHP designation raises employment or income

Event study coefficient estimates for the impact of NHP designation. 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 = 8,928, 8,614, 9,072, 9,072.

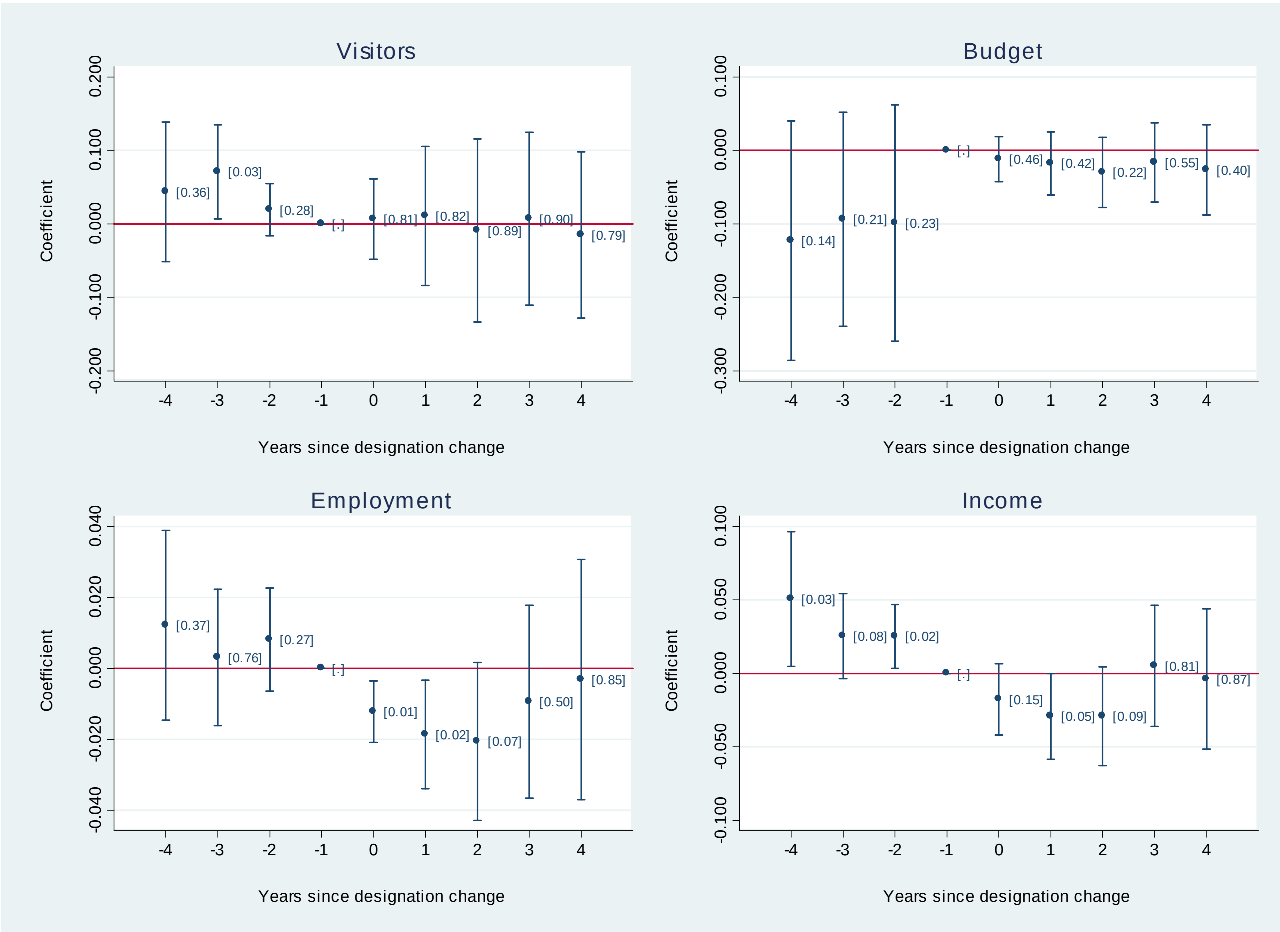

Figure A.23: No evidence that WHS designation raises employment or income

Event study coefficient estimates for the impact of World Heritage Site designation, controlling for NP designation and its leads and lags. Estimates are relative to the year before the WHS designation. Standard errors are clustered at the park level. Bars indicate 95 percent confidence intervals, p-values are in brackets. N = 8,882, 8,568, 9,024, 9,024.

4.4 Spillovers

Neighboring counties.

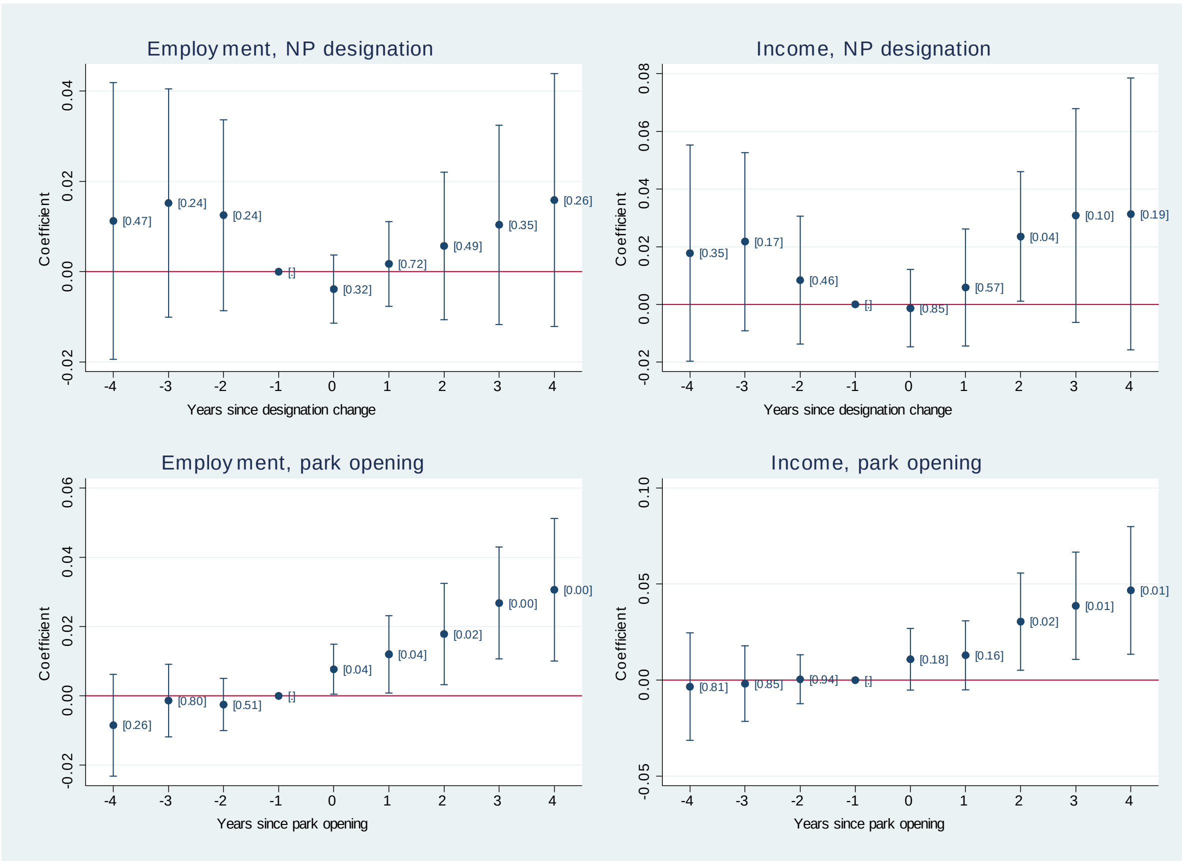

Figure A.24: No evidence of negative spillovers to neighboring counties

Event study coefficient estimates for the impact of NP designation or park opening on log employment and income of neighboring counties. 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, 9,024, 122,352, 122,338.

Nearby parks. Figure A.25 shows estimates from a visitor regression that includes two sets of event study coefficients: one for NP designation, and one for NP designation occurring in a park in a 100 mile radius. There are a total of 20 parks receiving the second treatment, but 9 of these are treated in 1971 (and are thus unobserved before year -1 relative to treatment) so there is a large change in sample composition from -2 to -1 year relative to treatment. To eliminate this source of error, we focus on the balanced specification corresponding to Column (2) of Table A.7 (years 1969-2017, no population share controls, event indicators -2 and above).

The left panel shows the NP designation coefficients, which are similar to those seen earlier (Figure 4 in the paper). The right panel shows the coefficients for a nearby park receiving NP designation. These estimates show that there are no significant declines in visitors in response to a nearby park receiving NP designation.

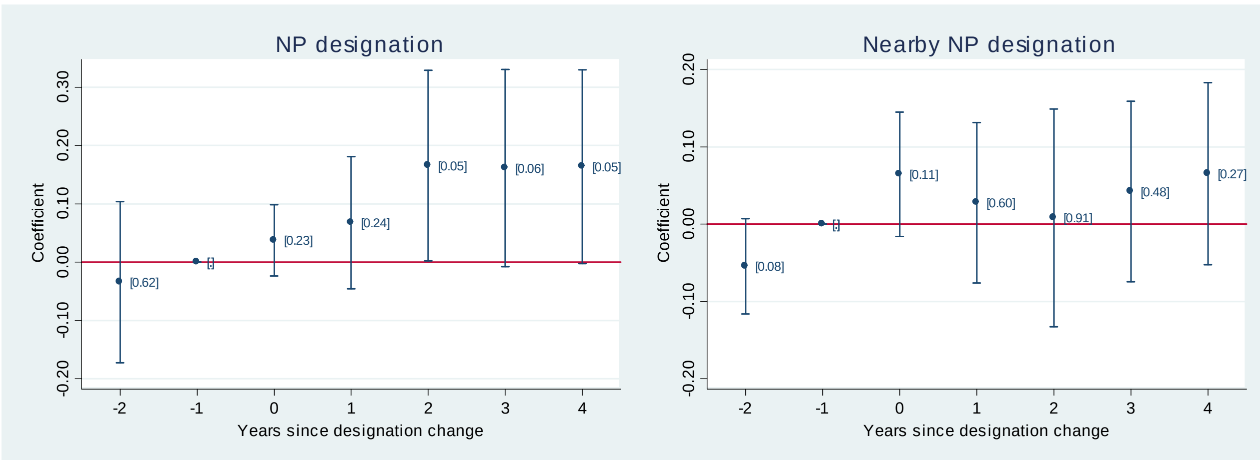

Figure A.25: No evidence that NP designation of nearby parks reduces visitation

Event study coefficient estimates for the impact of NP designation, as well as NP designation of parks located within 100 miles, on log visitors. Years 1969-2017, balanced sample. 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 = 9,043.

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