Power Without Capacity? Local Self-Governance and Economic Development on American Indian Reservations

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Abstract

American politics has always stood out, and is often celebrated, for the large role local governments play. Yet the study of local politics is replete with examples of local power gone wrong, where government bodies are too weak or too captured to act for the better good of the community. This paper asks whether the distributed nature of power in the U.S. is an economic boon or burden. To answer this question, I look at one of the most significant expansions of local power in the modern U.S.: American Indian tribal governments. Constructing the largest public dataset on reservation economies, combining nearly forty years of annual remote sensing data to estimate economic development on over 300 reservations, I analyze how the expansion of tribal self-governance power impacted reservation economies. I find that tribal self-governance slowed economic growth on average, although the effect fades over time. Using different measures of tribal governance capacity and political institutions, I find tentative evidence for a governance capacity mechanism.

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1 Introduction

Over the past 50 years, tribal governments in the United States have rapidly expanded their policy authority across Native areas (Kalt 2022). Moving power away from the federal government, tribal self-governance can be seen as a modern instantiation of the decentralized, local governance celebrated for its effectiveness both in American political history (e.g. Tocqueville 1835; Ostrom, Tiebout and Warren 1961) and globally (Ostrom 1990). It also carries normative appeal as part of broader demands for indigenous recognition and self-determination, as perhaps best epitomized by the UN Declaration on the Rights of Indigenous Peoples (United Nations General Assembly 2007). However, local governments are not perfect. They can produce highly inequitable outcomes (e.g. Trounstine 2016; Einstein, Glick and Palmer 2019; Hankinson and Magazinnik 2023; Sahn 2025) and struggle with capacity (Gargan 1981; Wallis and Dollery 2002), particularly in rural contexts (Helpap 2023; Smith et al. 2026). This raises the question, has tribal self-governance produced better outcomes for Native area communities? In particular, what are the economic consequences of self-governance?

I argue that the economic effects of self-governance result from the tension between two countervailing forces. Local autonomy mitigates agency loss by improving electoral control and ideological congruence between voters and policymakers. However, it may lower governance capacity through reductions in key factors like governing experience, institutional efficiency, fiscal resources, or skilled labor access. Consequently, this model predicts that the overall economic impact will become increasingly positive as local governance capacity grows.

To test this model, I look at tribal Self-Governance Compacting, a policy which enabled tribal governments to take policymaking control across a number of municipal domains from the federal government starting in the early 1990s. Today, roughly 50% of all federally recognized tribes have a compact, governing just under 30% of the reservations and other Native areas in the contiguous United States.

American Indian tribal governments are some of the most poorly understood and under-studied governments in the American federalist system (Ferguson 2016). This knowledge deficit partially stems from low data availability preventing cross-tribe comparisons or time-series analysis. To address these constraints, I leverage remote sensing data on nighttime lights and land use to construct a panel dataset measuring economic development in almost all federally recognized tribal communities in the contiguous United States for over three decades. The end result is the largest public dataset ever constructed on American Indian economies, covering 328 Native areas and 39 years of economic development.

Applying traditional and modern innovations in difference-in-differences designs, I find that entering into a self-governance compact resulted in *slower* economic development over time, although the effect fades after approximately 15 years. I test for a governance capacity mechanism by constructing a number of plausible proxies with existing administrative data and novel coding of tribal constitutional institutions. I find tentative evidence pointing towards the expected positive relationship between governance capacity and economic growth under expanded self-governance. This suggest that maximizing the value of self-governance requires greater investment in initial capacity-building, such as higher federal funding or more bureaucratic buildup.

These findings contribute to a broader understanding of local politics in the United States. Because tribal governments face many of the same policy issues (e.g. land use, business development, public safety) as municipal governments, but have a wider arrangement of institutions and more dramatic changes in historic power, tribal governments make for a useful case study for American local politics more generally. Additionally, contemporary research on American local political institutions often focuses on questions of representation and distribution (Trounstine 2020). This work highlights the often-neglected study of local governance capacity, which has largely been left to public administration scholars.[1] Undoubtedly, how the economic pie is sliced is important to understand, but the impact political institutions have on the size of the pie should also be studied. It also speaks to growing arguments for "abundance"-centered politics focusing on how government can expand the size of public goods offered (Klein and Thompson 2025).

Furthermore, this paper contributes to the growing literature on American Indian political

[1] See Hall 2008; Wang et al. 2012, and Terman and Feiock 2015 for contemporary examples.

economy and the institutional determinants to reservation economies (Dippel 2014; Dippel, Frye and Leonard 2020; Leonard, Parker and Anderson 2020; Leonard and Parker 2021). In particular, this work adds to burgeoning literature examining the effects of tribal institutions (Cornell and Kalt 1990; Evans 2011*b*; Akee, Jorgensen and Sunde 2015; Crepelle, Mahdavi and Parker 2024, Stratmann 2024). Additionally, this study adds to our understanding of the consequences of federal oversight of American Indian tribes (Corntassel and Witmer 2008; Frye and Parker 2016). Finally, the findings re-contextualize the sovereignty-development trade-off discussed in prior work (Anderson and Parker 2008; Wellhausen et al. 2017). Instead of being an inherent cost to tribal power, this work positions tribal governments as key actors which can themselves mitigate negative outcomes through developing capacity (Evans 2011*a*).

2 Self-Governance and Tribes

2.1 Benefits of Self-Governance

Classic theories of local governance focus on heterogeneity in preferences for public goods across different communities to explain the value of local governance (Tiebout 1956; Oates 1972). When such preference diversity exists, a uniform level of public goods provided by a central government will likely under-supply goods in some areas and oversupply goods in others. When the provision of public goods is instead determined locally, such inefficiencies are eliminated.[2] Other arguments in favor of decentralization put forward that it solves a critical principal-agent issue (Seabright 1996). Under a centralized system, local officials and bureaucrats are accountable not to the population they serve, but to the center which employs them. When power is decentralized, these officials are directly accountable to the local population (Faguet 2014). Additionally, when local populations have control over who governs them, they have the opportunity to select officials whose personal preferences are closer to the local population (Fischer 2016).

In application to the tribal context, there is significant reason to believe that these positive arguments to self-governance could have explanatory power. First, prior work in indigenous politics

[2] Oates (1999) argues that while a central government could theoretically provide different levels of public goods to match the specific preferences of each community, this is often politically infeasible or unsustainable.

has found positive returns to indigenous institutional empowerment. In comparative indigenous politics, for example, Díaz-Cayeros, Magaloni and Ruiz-Euler (2014) and Magaloni, Díaz-Cayeros and Ruiz Euler (2019) find that Mexican cities where traditional governance was formalized saw increased provision of electricity and sewerage. McMurry (2022) finds that recognition of indigenous self-governance in the Philippines increased birth registrations, suggesting that autonomy improved demographic record-keeping. In the US context, the era of self-governance has coincided with significant reservation economic growth (Kalt 2022). However, a number of other innovations in tribal economies have occurred in this period, such as tribal gaming, making it hard to disentangle the direct effects of self-governance. Evidence pointing to self-governance as the driver of this growth is scant, limited to a few early case studies on the productivity of tribal forestry programs (Krepps and Caves 1994; Harris, Blomstrom and Nakamura 1995).

Second, we should expect preferences regarding public goods provision to vary significantly across different tribe populations because Native nations face a wide variety of issues. For example, tribes face differing levels of threats due to climate change (Provins 2024). Some tribes, like those along the Pacific coast in Washington state, face existential threats from climate change and may prioritize investing in climate resilience programs. Other tribes in less environmentally fragile areas may prefer to allocate scarce resources toward other pressing issues. Such localized flexibility, however, is impossible when reservation policies are set at the national level.[3] The heterogeneity in preferences across each tribal community suggests that there may be efficiency gains to passing control of federal Indian programs to tribal governments.

Third, under an SGC, the chain of accountability is shortened, suggesting greater agency control for reservation community principals. When policies are under the authority of the federal government, Native communities have little ability to reward good policy and punish bad policy. Tribal electoral power federally is poor, largely limited to a very small set of highly competitive races (McCool 1985). And while many tribes have embraced lobbying as a way to have their interests

[3] As tribal self-governance advocates themselves argued, "Most of the BIA guidelines, policies and regulations are prepared for national application and are not tailored to specific Tribes, Reservations, or local conditions" (Lummi Nation et al. 1991).

represented in Washington, their influence is dwarfed in size by other organized interests (Carlson 2023). When tribal governments control policy, tribe members can make their preferences heard as their vote is worth considerably more. As some tribal leaders have remarked, "Self-governance is a two-edged sword. We get more control, but [...] don't get to blame the feds when my people complain about failure" (Henson 2008, 127). Those administering the program may also be aligned with the preferences of the service population because they themselves are part of the service population. This fits with findings that tribe members trust their tribal government more than any other government body (Schroedel et al. 2020).

Because reservations should have different policy preferences and struggle to hold the federal government accountable, they seem to be an ideal context to observe positive returns to self-governance. We can summarize this as the naive self-governance hypothesis.

> **H1a:** Reservations with greater self-governance will have higher economic growth than reservations with less self-governance.

2.2 Costs of Self-Governance

While the conditions for a positive effect from self-governance seem plausible, this view ignores the downsides of local self-governance. Empirical studies have found mixed results to the economic returns of decentralization (Martinez-Vazquez and McNab 2003; Treisman 2007; Faguet and Sánchez 2008). Work on fiscal federalism argues that decentralized systems can create incentives for poor fiscal behavior by local governments (Cai and Treisman 2004; Rodden 2006) or are poorly designed structurally (Weingast 2009, 2014).

Perhaps more concerning than inducing poor spending constraint, lower levels of government may lack governing capacity to effectively create and implement public policy.[4] Local governments may lack the fiscal and personnel resources to meet demand for public goods (Gargan 1981;

[4]In this framework, I utilize a relatively wide conceptualization of governing capacity. While discussions of capacity often focus narrowly on the ability to implement and enforce policy, such outcomes are endogenous to the underlying conditions and institutions that produce them (Suryanarayan 2024). Earlier public administration scholarship delineated various forms of capacity along this causal chain, considering aspects of political and institutional capacity to create policy (Grindle 1996; Polidano 2000). This broader definition is necessary to accurately capture the structural realities of self-governance.

Prud'Homme 1995; Wallis and Dollery 2002; Lobao and Kelly 2019; Carter 2022; Helpap 2023; Smith et al. 2026). Furthermore, their institutions may be particularly ineffective (Brown and Potoski 2003; Sahn 2023) or lack governing experience and the bureaucratic learning that comes from said experience (Gailmard and Patty 2012; Foa 2022). Finally, the smaller scale of politics can lead to lower information availability[5] and greater ability for capture by select interests (Gerber and Phillips 2004; Oliver and Ha 2007; Lubell, Feiock and De La Cruz 2009; Einstein, Glick and Palmer 2019; Schaffner, Rhodes and La Raja 2020; Anzia 2022; Simonovits and Payson 2023).

These concerns regarding capacity loss translate to the tribal context. In terms of resources, tribal governments can be severely lacking, despite often serving disadvantaged communities and large land areas.[6] Private investment on reservations is rare, so instead of taxation, reservation economies are typically driven by tribal government-owned industry, such as gaming or natural resource extraction, as well as federal spending (Ratté and Anderson 2022). However, federal funding for Indian programs has long been underfunded (see Lummi Nation et al. 1991; Kunitz 1996; U.S. Commission on Civil Rights 2003) and difficult to access (U.S. Government Accountability Office 2024). Tribal gaming revenue is large, but heavily concentrated in a small number of tribes (National Indian Gaming Commission 2023). And reservations that do have access to natural resources often face systematic barriers to extraction (Leonard and Parker 2021).

Modern tribal governments, in the grand scheme of American politics, are also young and inexperienced. Tribal governments were only formally constituted under modern federal recognition starting in the 1930s, with some not acknowledged until much later. Tribes have had to invest significant resources to build bureaucratic expertise, a process many others may have neglected (Evans 2011*b*). Additionally, many of the constitutions adopted by tribes, often at the behest of federal bureaucrats, did not resemble their traditional forms of governance and existing social structures (Cornell and Kalt 1992; Lemont 2009). This disconnect can lead to a mis-match between *de jure* rules and *de facto* power, resulting in poor government performance.

[5]For example, see the decline of local newspapers and subsequent lack of coverage on television (Rubado and Jennings 2020; Peterson 2021; Hopkins 2022)

[6]For example, there are twelve reservations larger than Rhode Island.

Finally, the smaller scale of tribal politics that supports accountability gains can also create conditions for narrow interest capture. Sub-groups within a tribe whose preferences diverge from broader economic development goals may exert outsized influence on tribal policy, producing slower growth even where tribal governments are otherwise well-equipped to deliver it.

This potential loss in governance capacity gives us the opposite prediction regarding the effect of self-governance compacting:

> **H1b:** Reservations with greater self-governance will have lower economic growth than reservations with less self-governance.

Taken together, the countervailing nature of these mechanisms mean that the observed effect of tribal self-governance depends on which competing effect is greater. If the gains from reducing agency loss and increasing policy alignment are greater than the costs from lower governance capacity on average, then we should observe a positive effect for tribes that enter an SGC. If the reverse is true, then we should observe a negative effect. This also implies that as tribal governance capacity increases, the costs to self-governance are reduced, and therefore we should observe that the effect of entering an SGC becomes more positive.

> **H2:** As tribal governance capacity increases, the economic effect of self-governance should be more positive.

3 Tribe Context

By the early 1900s, Indigenous communities in the United States had largely been removed from their homelands and moved to constricted, economically disadvantageous reservations (Wilkins and Stark 2017). Shifting sentiment in Washington led to interest in uplifting these communities in order to facilitate assimilation into US culture and society. As part of this push, the Snyder Act of 1921 was signed into law, empowering the federal agency in charge of overseeing reservations, the Bureau of Indian Affairs (BIA), to carry out various local programs on reservations for the ostensible benefit of Native communities.[7] This power ranged across a variety of policy areas, including health

[7] At the time, the BIA was named the Office of Indian Affairs.

care, agriculture, housing, welfare, employment, and natural resource management. Even as tribal governments were formally reconstituted as recognized governing bodies starting in the 1930s, BIA officials held significant power over public affairs on reservations, often compared to autocrats or dictators (Cohen 1953). Over time, the BIA's mandate to run these local programs has continued. However, federal policy innovation has allowed tribes to take greater control over BIA programs on their reservation, in part due to tribes pushing for more autonomy from agencies like the BIA (Castile 1998).

Today, native communities have three options for how almost any BIA program is administered on their reservation. The first option is to have the BIA administer the program, otherwise known as *direct service*. This functionally resembles what these programs were originally envisioned as in 1921. Programs administered under direct service are centrally controlled. The tribal government has no say in how the programs are run and plays no part in executing them.

Under the Johnson administration, the BIA began experimenting with passing funding to tribes to allow them to run programs themselves. By 1975, this policy was officially instituted with the passage of the Indian Self-Determination and Education Assistance Act (ISDEAA) as self-determination contracting, or simply *contracting*. When a tribe signs a self-determination contract agreement with the BIA, any program included is now executed by the tribe, but based on the BIA's expectations. The tribal government takes on the administration and funding for the program, but is restricted in how it can run the program.[8] Most importantly, the funds for the program cannot be combined with other funding sources, nor can they be transferred from one program to another or carried over to a new year, and the operating procedures and goals must match those of the BIA (Stuart 1990). Functionally, contracting allows the tribe to get experience in executing public policy and generate some local economic benefits through hiring tribe members, but doesn't give tribes the flexibility to properly adjust the programs to their preferences. Ultimately, under a contract, a tribe is primarily responsible to a BIA contracting officer, not the tribe members (Lummi Nation et al. 1991). Any program that is not deemed an "inherent federal function" can be included in a

[8] Additional funding for indirect costs tribes incur for overhead would eventually be added as well. Most notably, 105(l) leases reimbursing tribes for facilities used in the execution of contracted programs.

contract and the BIA is required to allow the program to be contracted if requested by a tribe. As of 2024, 92% of tribes are contracting with the BIA (Newland 2024).

While ISDEAA was seen as a major policy win for tribal governments, the continued influence of the BIA meant frustrations continued, compounded by perceived BIA feet-dragging in signing contracts (Strommer and Osborne 2014). Tribal governments continued to push for greater control over these federal programs and eventually won when Congress amended ISDEAA in 1988 to pilot allowing tribes to take full control over BIA programs, a policy eventually referred to as self-governance compacting (SGC) when made permanent in 1994. Under an SGC, tribes administer programs like they can under a contract, but they now have much greater control over how the program is run and more flexibility in financing with reduced federal oversight (Murray, Dortch and Heisler 2025). Compacts allow tribes to redesign program goals and operating procedures and to reallocate federal funds within their budgets. For example, a tribe with a compact including law enforcement and housing development programs could decide it didn't need as much funding for housing and transfer a portion of the housing funds to its law enforcement program. Or the tribe might prefer to have their law enforcement program focus on public outreach campaigns instead of street patrolling and shift their operating procedures and goals accordingly.

To enter a compact, a tribe must demonstrate fiscal stability by running programs under a contract without auditing errors. The tribe must also complete a planning phase that includes legal and budgetary research as well as organizational preparation for governance (Murray, Dortch and Heisler 2025). Tribes are able to mix and match direct service, contracting, and compacting. So a tribe could have some programs administered through direct service with the BIA, some under a contract, and the rest in a compact. Alternatively, it could have all BIA programs included under one of the three categories.

Early experiences with compacting were filled with optimism, but also met struggles with implementation on both the tribe and federal sides. The BIA was slow to implement the initial compacts, as they were with contracting before. Early adoption tribes, who also were among those originally pushing for control, made significant investments in reorganizing tribal government structures (Lummi Nation et al. 1991). Some tribes, eager to provide for their communities, would greatly expand services under a compact, only to realize that more funding from the BIA was not possible, forcing them to scale back their plans (Henson 2008).

Today, approximately 50% of tribes have a compact. In the contiguous United States, only 29.5% of recognized tribes have an SGC. Uptake is higher in Alaska where 84.6% of recognized tribes have an SGC. What programs are being contracted/compacted versus left for direct service by each tribe, however, is not public knowledge. Discussion of self-governance compacting still largely revolves around planning and implementation to bring new tribes into compacts and successfully manage programs. For example, at the annual Tribal Self-Governance Conference, panels and discussions tend to focus on things like the logistics of entering a compact, changes to federal funding and regulations which impact SGC programs, and learning about policy innovations from other tribes on issues like health, public safety, land management, and job training.

This project focuses on examining the impact of SGCs. There are two primary reasons for this. First, from an empirics practicality standpoint, tracking contracting is very challenging. SGCs, conversely, are much easier to observe because the BIA maintains a list of all tribes with an SGC and the year they entered one. Second, SGCs have greater theoretic interest. Given that the majority of tribes have a contract, most of the empirical comparisons in this paper will be estimating the difference between contracting and compacting, where the only difference is the tribe's assumption of program design and added flexibility. This presents a rare opportunity to isolate the importance of tribal policy design and administrative discretion from funding or execution.

4 Data and Methods

4.1 Unit of Analysis

This paper uses American Indian federal reservations and other Native areas as the unit of analysis.[9] This excludes state-recognized reservations and Alaskan Native communities. Reser-

[9]For brevity, I simply refer to these areas as reservations for the rest of the paper. Other included types of Native areas, like Oklahoma Statistical Tribal Areas (OTSAs) and Tribal Designated Statistical Areas (TDSAs), make up only a small percentage of the included units.

vation boundaries occasionally change, which presents potential issues for estimating changes in development using remote sensing. To avoid this problem, I use boundaries from 2000, the earliest digitized boundaries available for all reservations.[10] Most tribal governments possess one reservation, but there are a handful of tribes with more than one reservation. In total, my dataset includes 328 reservations belonging to 305 tribes.

4.2 Self-Governance Compacts

The key treatment variable is adoption of a self-governance compact, drawn from a BIA Office of Self-Governance list identifying compacting tribes and their year of adoption.[11] Tribes can also enter into separate compacts with the Indian Health Service (IHS) or, more recently, the Department of Transportation (DOT). I focus on BIA SGCs over IHS compacts because BIA policies are more clearly relevant to economic growth. DOT compacting began only in 2020 and therefore provides too few cases and too little time under treatment for meaningful analysis.

In total, 102 federally recognized tribes in the contiguous United States have an SGC as of 2024. This translates to a total of 92 reservations, approximately 28% of reservations included in the study. There is no treatment reversal as no tribe has ever left an SGC. In theory, tribes may have moved previously compacted programs back to direct service or a contract, but that granularity is not available for this study. This means that treatment intensity will have unobserved variation. A tribe with an SGC that covers every compact-able BIA program and a tribe with an SGC that covers just one BIA program will have the same treatment status.

Figure 1 shows the spatial distribution of SGC adoption by reservation, along with the cumulative number of SGC reservations over time. Two patterns stand out. First, adoption was fastest in the first decade of self-governance: just over half of treated reservations adopted an SGC before 2000. Second, adoption and its timing are geographically clustered. Early adoption was common among tribes in western Washington, the different constituent bands of the Minnesota Chippewa tribe in

[10] Reservation boundaries were taken from U.S. Census Bureau's TIGER/Line files, as collected by IPUMS NHGIS (Schroeder et al. 2025). For reservations created between 1985 (the earliest year in my outcome variables) and 2000, I rely on Tiller (2015) and other historical news, tribe, and federal sources to estimate the creation year.

[11] An archive of the online list can be found at https://web.archive.org/web/20250704122056/https://www.bia.gov/sites/default/files/media_document/2024_self_governance_tribes_alphabetically_as_of_07.24.24.pdf.

northern Minnesota, and some of the large Oklahoma tribes. Many California tribes, meanwhile, adopted SGCs in later periods. By contrast, adoption is rare in the Eastern United States and the Great Plains.

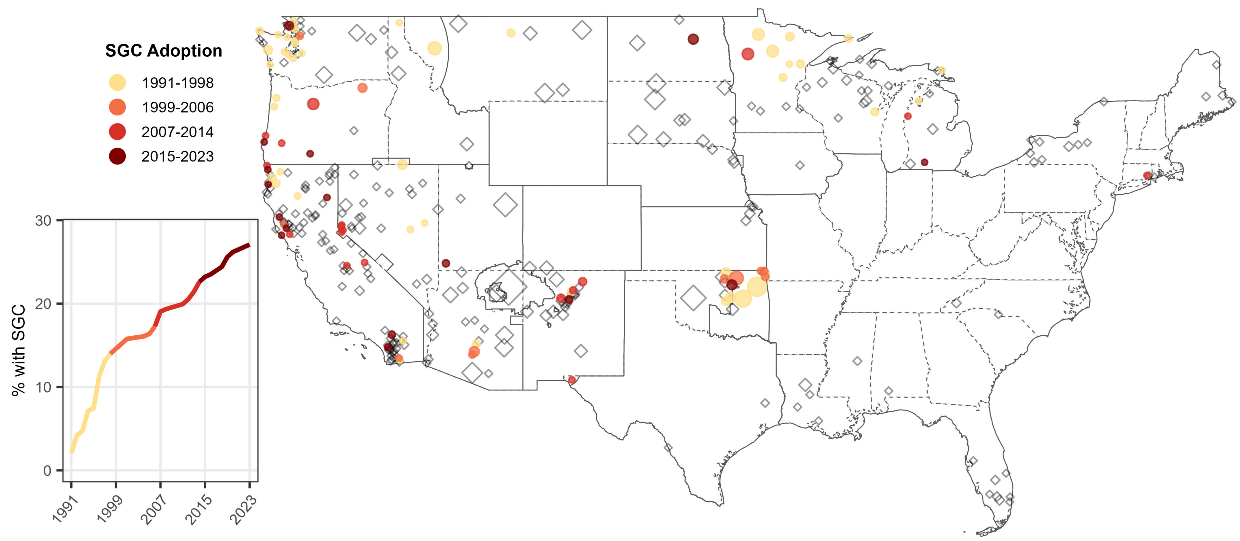

Figure 1: Map depicting SGC adoption timing by reservation. Untreated reservations are marked with diamonds, treated reservations are marked as circles and colored by timing of SGC adoption. Size of all points based on relative geographic size of the reservation. Line graph in the bottom-left corner displays the cumulative number of SGC reservations over time.

4.3 Measures of Economic Development

One of the most difficult challenges to studying American Indian politics is the paucity of quality data available. Measures of economic development are no different in this regard. Census income data reported at the reservation level extends back to 1970. However, Census data can only give once-a-decade snapshots of economic performance, which makes estimating pre-treatment trends difficult to analyze. Given that many panel data methods rely on pre-treatment trends to justify identification assumptions, the low number of time periods is not ideal. The American Community Survey (ACS) also reports reservation-level data, but these data have issues with sampling small reservation populations and only go back to the mid-2000s (Connolly and Jacobs 2020).

The lack of high quality, frequent economic measures makes constructing large panel datasets of reservation economies challenging. An increasingly popular answer to this lack of data has been to rely on remote sensing measures, although American politics scholars have been slower to apply these tools. Nighttime light (NTL) intensity has become a particularly popular measure of economic activity at the local level across a number of different contexts (e.g. Lü and Landry 2014; Min 2015; Kroth, Larcinese and Wehner 2016; Jiang 2018; Jensenius and Suryanarayan 2022; Hong, Park and Yang 2023; Sexton and Zürcher 2024; Pantaleao 2025). Data on land use-land cover (LULC) has also become increasingly common for analyzing phenomena like natural resource management (Baragwanath and Bayi 2020; Sanford 2023; Gulzar, Lal and Pasquale 2024) and urban development (Burchfield et al. 2006; Saiz 2010; Keola, Andersson and Hall 2015; Hu et al. 2024; Wittberg, Tavares and Szmigiel-Rawska 2025). Early work to incorporate remote sensing data into American Indian studies has also started using LULC data. Dippel, Frye and Leonard (2020) use LULC data to measure long-run development of reservation land in 5 snapshots spanning almost 50 years. I expand on these applications of remote sensing by constructing two yearly indicators of reservation economic development using nightlight density and land cover data extending almost 40 years.

I use land use-land cover data from the U.S. Geological Survey's National Land Cover Database (NLCD) (U.S. Geological Survey (USGS) 2024). The NLCD integrates legacy land cover maps with deep learning methods to generate nationwide land use–land cover maps at a 30×30 meter resolution for each year from 1985 to 2023. Each pixel is classified into one of 16 categories. Four categories capture different levels of developed land, ranging from low-intensity uses like parks and large-lot homes to high-intensity uses like apartment complexes and industrial sites.

From these data, I calculate each reservation's yearly share of land classified as developed.[12] Specifically, I sum all pixels intersecting the reservation in the developed categories and divide by the total number of non-water pixels in the reservation to obtain the share of developed land. This measure can theoretically range from 0, meaning no land is classified as developed, to 1, meaning

[12] Only pixels which were at least 25% covered by the reservation were included in these calculations. The same rule was used for nighttime light pixels.

every non-water pixel in the reservation is classified as developed.

For nighttime light intensity, I rely on harmonized data from Chen et al. (2024) which spans from 1992 to 2023. This dataset uses a deep learning U-NET model to increase the resolution of NTL data from 1992 to 2011 to higher quality NTL data available starting in 2012. This process is ideal because not only does it give a harmonized measure over most of the time period of interest in this study, but also gives the highest resolution pixels possible for NTL intensity, 500×500-meter cells.[13] After removing pixels covering water, I calculate the average intensity across all pixels on the reservation.[14]

As a brief, stylized example to give a sense of what the underlying data looks like and what these estimates are capturing, I present the Lac du Flambeau reservation in Wisconsin as observed through these data in Figure 2. In panel A, we see a direct satellite image of the entire reservation, showing us what this reservation looks like in reality. In panel B, the NLCD data over the exact same area and year is plotted. We see clearly many of the features from the satellite imagery transferring over to the land use categories, most notably the various lakes and woodlands, as well as the city of Lac du Flambeau in the center of the reservation. Panel C presents the same NLCD data, except now all water pixels have been removed and the pixels have been recoded to either developed/cultivated or undeveloped. This helps us see exactly what the NLCD is picking up as developed, which in this case seems to be mostly the urban areas around Lac du Flambeau city and the various roads on the reservation. Finally, panel D presents the NTL data for the same area and year, with the water areas already removed. Fairly consistent with panel C, we see a relatively high luminosity in the urban part of the reservation, and almost no luminosity in the rural areas.

[13] Previous harmonization strategies reduce the resolution of the higher quality VIIRS post-2012 imagery to match the lower resolution DMSP-OLS imagery (Li et al. 2020). However, this roughly halves the resolution quality of the data. Studies comparing DMSP-OLS and VIIRS data as proxies for economic activity have found DMSP-OLS to be largely insufficient for local areas (Gibson et al. 2021; Gibson and Boe-Gibson 2021).

[14] For many reservations, the NTL data is very noisy, jumping up and down year-to-year in a way that seems unlikely to represent the true development this measure is meant to capture. To avoid issues related to this noise, I use an imputation approach to replace the more extreme outliers for each reservation. Specifically, I fit a loess line for each reservation's NTL estimate and replace any value further than one standard deviation away from the line.

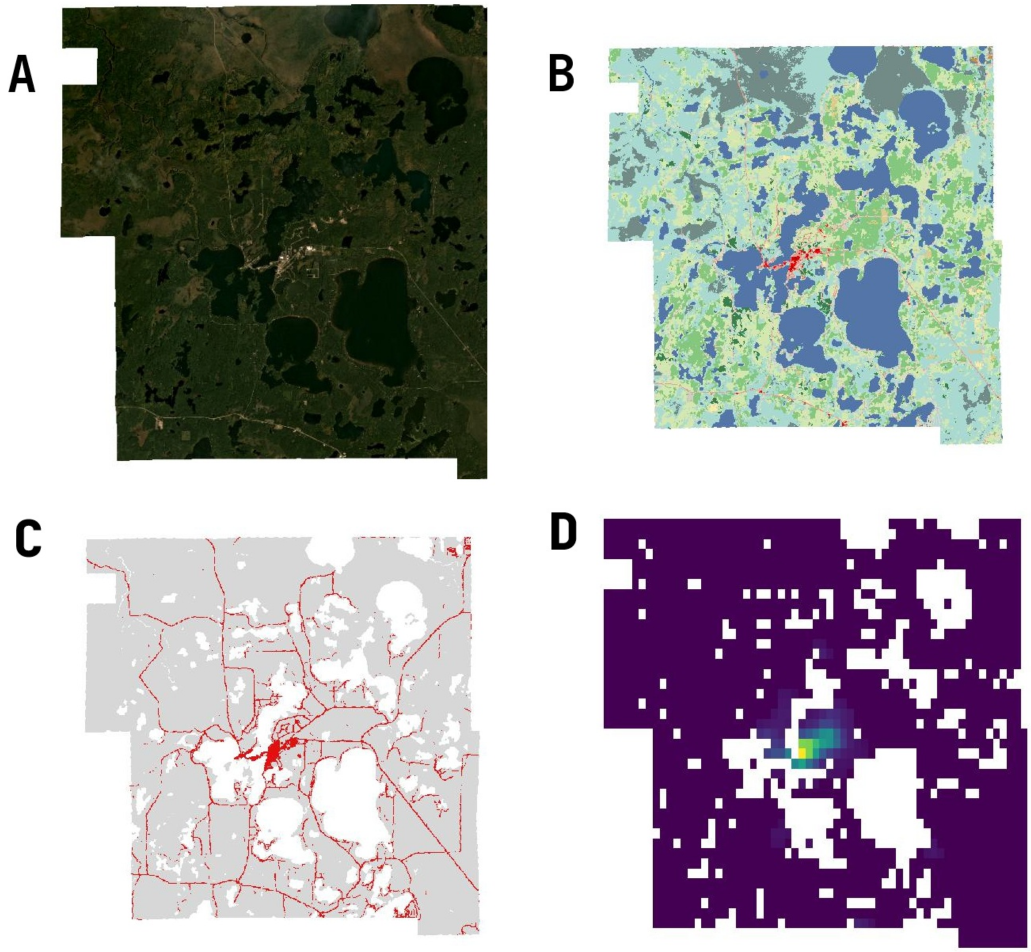

Figure 2: Example of NLCD and NTL data using the Lac du Flambeau reservation. Panel A depicts the reservation through a basic satellite composite image to use as a quality reference for the other data. Panel B presents the NLCD data from the same year as map A with all categories presented. Panel C depicts the same NLCD data, but with water pixels removed and the categories flattened to only developed/cultivated or undeveloped. Panel D presents the NTL data for the same year, with water removed.

Overall, change in both the land and NTL measures vary greatly across reservations. Figure 3 presents the change in both outcomes for each reservation. Some reservations experienced astronomical growth. For example, the Chehalis Reservation, a small reservation in Washington, saw an increase in average NTL intensity from 0.127 in 1992 to 1.14 by 2023, an almost 800% increase. Conversely, 5.8% and 2.1% of reservations had a change in their share of developed land and average NTL intensity change by less than 10% by 2023, respectively. More broadly, while some reservations show clear, significant growth, others have essentially flat growth. Over the study period, on average, the share of developed land grew by 7.65 percentage points (median = 1.88) and NTL intensity grew by 2.7 (median = 0.23).

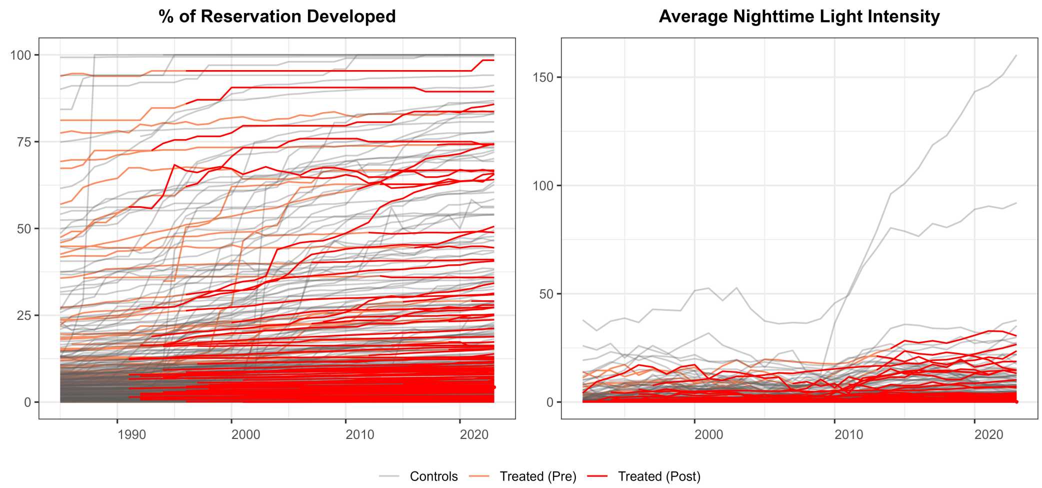

Figure 3: Change in economic development indicators. The left panel depicts the share of developed land and the right panel depicts average nighttime light intensity for all reservations in the sample.

4.4 Measures of Governance Capacity

In order to test whether tribal governance capacity drives the relationship between SGCs and economic growth, I need measures that can serve as meaningful proxies. This is very challenging, given both the lack of data availability and uniqueness of tribes.[15] As a first attempt to explore measuring tribal governance capacity, I collect eight measures which I argue proxy for resource sufficiency, policymaking effectiveness, and interest capture.

To capture whether a tribal government has sufficient resources to meet demand for public goods, I test four measures: reservation geographic size, American Indian and Alaska Native (AIAN) population, tribal enrollment (membership in the tribe), and distance from a large city. In most local contexts, larger land bases or populations may be indicators of greater resources, as they represent tax bases. However, as previously mentioned, tribal governments are not primarily

[15]Some common measures of governance capacity are the size of a government's bureaucracy or its efficiency in tax collection. In the tribal context, data on the former would be theoretically meaningful, but is not available. The latter measure, meanwhile, is not applicable to the tribal context because most tribal governments do not rely on taxation to generate revenue and most tribes do not make their budget publicly available.

funded from taxes, especially on their own population (Ratté and Anderson 2022; Wilkins 2024). Instead, more land and more people represent greater demand for public goods. Distance from a large city, however, serves as a proxy for market access. Being closer to a city allows a reservation to draw on that population for employees with significant training and expertise, as well as a customer base for potential business on the reservation.

Reservation size is calculated as the geographic area of the reservation in 2000. AIAN population is taken from reservation Decennial Census data from 1990 to 2020. Tribal enrollment counts, essentially the number of citizens in a tribe, are taken from the Department of Housing and Urban Development's Indian Housing Block Grant (IHBG) Formula data.[16] For city distance, I calculate the shortest distance between a reservation centroid and a city with a population of at least 100,000 (as of 2020). These four measures are each averaged across all available years and binarized at the median.[17]

I collect three proxies for the efficiency and effectiveness of tribal policymaking. First, I consider ethnic factionalization. When reservations were created in the 19th century, some brought together previously unrelated bands that had governed themselves separately, while others were organized around a single, already-unified tribe. As Dippel (2014) argues, the former experienced weaker long-run economic growth due to persistent social divisions that fostered conflict within tribal government. Members of factionalized reservations are less likely to see themselves as a single community, making disagreement and government inaction more likely. I use data collected by Dippel (2014), which used a binary measure of whether a reservation was created from unified or previously unrelated groups.

My other two indicators for tribal policymaking quality are collected from tribal constitutions, which scholars have increasingly examined to identify Native institutions (e.g. Tatum et al. 2014,

[16]Data available at this archive: https://web.archive.org/web/20250831195217/https://www.hud.gov/helping-americans/public-indian-housing-ihbgformula.

[17]Averaging across years addresses several concerns. Most of these measures do not cover the entire period analyzed in this article, so a time-varying measure would reduce the sample size. Additionally, enrollment figures update infrequently for most tribes (Akee et al. 2020) and yearly measures raise post-treatment bias concerns since economic development likely affects population and enrollment. Categorizing reservations as roughly above or below the median captures stable relative differences that are unlikely to shift significantly over the study period.

Cordell et al. 2020, Piano and Rouanet 2024). From tribal constitutions, I code two relevant variables for policymaking: the executive selection process and direct democracy-style assemblies.[18] Most tribes are governed by a small council with an executive elected either directly by voters or indirectly by council members. Previous work has found that direct executive elections increased long-run reservation incomes (Akee, Jorgensen and Sunde 2015). Because directly elected executives may have greater discretion to act, I assume policymaking will be more efficient for these tribal governments. Direct democracy-style assemblies allow all adult members to participate in government decisions. These are an important part of some tribes' traditional governance, but may have the unfortunate consequence of slowing down decision-making, leaving the tribe less able to respond to issues in a timely manner.

Finally, I also use tribal constitutions to collect a proxy for interest capture in the form of on-vs-off reservation voters. Many tribes have a large share, sometimes a majority, of enrolled members living off-reservation. However, some tribes allow off-reservation members to vote while others restrict voting rights to those on or near the reservation. When off-reservation members are allowed to vote, this shifts government responsiveness away from on-reservation members. In theory, these off-reservation voters may be less interested in resource expenditure on the reservation and instead prefer investment into policies which more directly benefit them, reducing investment on the reservation itself.

Given the large impact tribal gaming has on reservation economies, I also collect data on tribal gaming to use as a control. Specifically, I use a binary measure of tribal gaming to represent whether a reservation's tribal government operated a gaming establishment in time t.[19]

[18] I collected and coded 183 tribal constitutions obtained from tribal nation websites and online repositories including the National Indian Law Library, the Library of Congress, and the University of Arizona. Two research assistants and I first coded 89 constitutions by hand. I then used ChatGPT to code all constitutions using the hand-coding as an accuracy reference.

[19] I use Casino City Press' online Gaming Business Directory to identify all casinos owned by a tribal government as early as 2002. For early years, I rely on listed opening dates in the directory, as well as outside sources, to trace when casinos in the directory opened.

4.5 Empirical Strategy

The primary empirical strategy I employ in this study is a series of counterfactual estimators proposed by Liu, Wang and Xu (2024). With this approach, a counterfactual is estimated for each treated reservation-year observation to predict what the outcome *would have been* in the absence of treatment. Then, the difference between the observed and counterfactual outcome is the treatment effect. This gives a specific treatment effect for every reservation-year observation, as well as an overall average treatment effect on the treated across all treated observations. The model used to generate these counterfactuals is fit using only untreated observations — all treated reservation-years are held out — which avoids the negative weighting problem that afflicts conventional two-way fixed effect (TWFE) models.

All three estimators share a common structure for the untreated potential outcome:

$$Y_{it}(0) = f(\mathbf{X}_{it}) + h(\mathbf{U}_{it}) + \varepsilon_{it} \tag{1}$$

where $\mathbf{X}_{it}$ is a vector of observed covariates, $\mathbf{U}_{it}$ captures unobserved attributes, and ε_{it} is an error term. The estimators differ in how they model the unobserved component $h(\mathbf{U}_{it})$.

The simplest, the fixed effects counterfactual estimator (FEct), uses standard additive unit and time fixed effects:

$$\hat{Y}_{it}(0) = \alpha_i + \xi_t + \varepsilon_{it} \tag{2}$$

where α_i is a reservation fixed effect and ξ_t is a year fixed effect. This is equivalent to a standard TWFE model, but fit only on untreated observations. I will also estimate the counterfactual adding a time-varying binary measure of tribal gaming.

$$\hat{Y}_{it}(0) = \beta_1 Gaming_{it} + \alpha_i + \xi_t + \varepsilon_{it} \tag{3}$$

FEct assumes that unit and time fixed effects, as well as controlling for observable time-varying covariates, are sufficient to capture all systematic variation in untreated outcomes. If unobserved time-varying confounders exist, factors that affect reservations differently over time, this assumption fails and the estimates will be biased. The interactive fixed effects counterfactual estimator (IFEct) and matrix completion estimator (MC) address this in two different ways. IFEct augments the FEct model with r latent factors that interact with unit-specific loadings, allowing the model to capture unobserved confounders that evolve over time and affect reservations heterogeneously. The number of factors r is selected via cross-validation. MC, building on Athey et al. (2021), takes a complementary approach: rather than estimating explicit factors, it treats the counterfactual estimation as a matrix completion problem and uses regularization (controlled by a parameter λ, also cross-validated) to recover the low-rank structure of the data. Using all three estimators not only serves as a robustness check to any effect estimated, but also allows selecting the model which most believably meets the identification assumptions.

The estimand across all three methods is the average treatment effect on the treated (ATT):

$$ATT = \mathbf{E}[Y_{it}(1) - Y_{it}(0) | D_{it} = 1, C_i = 1] \tag{4}$$

where $C_i = 1$ indicates a reservation whose treatment status changed during the observed time window. The ATT is estimated by averaging the difference between observed and counterfactual outcomes across all treated observations. Importantly, these estimators can also recover the ATT at each post-treatment period separately, tracing the dynamic path of the treatment effect over time. This is a key advantage over many alternative estimators, which typically assume a constant treatment effect and cannot accommodate the possibility that effects emerge gradually or intensify over time. Uncertainty estimates are obtained via a block bootstrap clustered at the reservation level with 1,000 replications.

The key identification assumption is that treatment assignment is orthogonal to changes in untreated potential outcomes:

$$Y_{it}(0) - Y_{is}(0) \perp\!\!\!\perp D_{it}, \forall s, t \tag{5}$$

This may seem difficult to justify when treatment, adopting a self-governance compact, involves tribal self-selection. However, I argue that this assumption is reasonable in my setting. Decisions around entering a self-governance compact largely revolve around a tribal community's beliefs about its relationship to the federal government. Very few, if any, tribal government officials I spoke with viewed compacting as an economic development strategy; it was almost universally seen as an expression of tribal sovereignty and a rejection of perceived BIA incompetence. These preferences may partially correlate with economic development, but they are also deeply rooted in the historical experience of each tribe and the network of other tribes it exists in, neither of which necessarily correlates with changes in untreated outcomes.

A practical advantage of this framework is that it provides built-in diagnostic tests to assess the plausibility of this assumption. A placebo test shifts treatment timing backward and checks whether the model detects a false effect in the pre-treatment period; if so, the counterfactual model may be mis-specified. An F-test jointly tests whether residuals across pre-treatment periods are zero, providing a direct check on parallel pre-trends. I also report a TOST equivalence test, which complements the F-test by asking whether pre-treatment residuals are substantively negligible rather than merely statistically insignificant. For robustness, I also report estimates from the traditional TWFE estimator, stacked DID (Cengiz et al. 2019), Callaway and Sant'Anna's (2021) group-time estimator, and the interaction-weighted estimator of Sun and Abraham (2021) in Appendix F.

5 Effect of SGCs

I now estimate the effect of self-governance compacting on the remote sensing measures. I estimate ATTs using all three counterfactual estimators. For each outcome and estimator, I estimate two models, one using no observed covariates and one controlling for gaming.

In Table 1, I report the estimated overall ATTs for each model, as well as the ATTs in specific post-treatment years to give a sense of how the effect develops over time. IFEct produces the most believable lack-of-pretreatment-effect based on the results of the F-test and TOST equivalence test. Consistent across all models, adoption of an SGC led to a reduction in the expected share of developed land and NTL intensity, although only the land-use outcome was statistically significant.

Based on my theory, these results reject hypothesis H1a that increasing self-governance would increase economic growth in this context.

Inspecting how the effect evolves over time, however, reveals a more interesting pattern. Focusing on the top portion of Figure 4 plotting the effect of an SGC on developed land share over time, the effect is not a consistent downward trend. Instead, an SGC seems to have little impact in the first few years after signing. Then, roughly 5 to 15 years after entering an SGC, there is a precipitous drop in the expected growth on the reservation, which levels out in later years. This largely fits with an explanation of bureaucratic learning. If this effect is due to a lack of governance capacity outweighing the representation gains, the later trend could be indicative of the tribal government improving its ability to govern. Instead of being a cost to sovereignty, it's a a cost of capacity-building.

Table 1: Effect of Self-Governance Compacting on Economic Development

	FEct		IFEct		MC	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Developed Land Share						
ATT	-1.805* (0.735)	-1.897* (0.746)	-1.613* (0.708)	-1.564* (0.720)	-1.722* (0.683)	-1.784** (0.684)
$\text{ATT}_{t=1}$	-0.507 (0.372)	-0.536 (0.367)	-0.071 (0.156)	-0.060 (0.162)	-0.337* (0.171)	-0.353* (0.172)
$\text{ATT}_{t=6}$	-0.888 (0.557)	-0.955 (0.558)	-0.332 (0.545)	-0.282 (0.558)	-0.794 (0.425)	-0.833* (0.421)
$\text{ATT}_{t=12}$	-1.983** (0.767)	-2.111** (0.790)	-2.026** (0.742)	-1.987** (0.743)	-1.812** (0.681)	-1.914** (0.680)
$\text{ATT}_{t=18}$	-2.802** (0.921)	-2.913** (0.937)	-2.919** (0.927)	-2.850** (0.933)	-2.688** (0.887)	-2.761** (0.889)
Tuning (r / lambda)	0	0	4	4	0.0026	0.0026
F-test p-value	0.006	0.015	0.106	0.100	0.000	0.000
TOST Equiv. p-value	0.728	0.747	0.034	0.044	0.672	0.686
Observations	12792	12792	12792	12792	12792	12792
Years	39	39	39	39	39	39
Treated Reservations	91	91	91	91	91	91
Panel B: NTL Radiance						
ATT	-0.550 (0.386)	-0.538 (0.367)	-0.580 (0.505)	-0.718 (0.569)	-0.444 (0.471)	-0.493 (0.488)
$\text{ATT}_{t=1}$	-0.177 (0.297)	-0.176 (0.295)	-0.291 (0.153)	-0.298 (0.152)	-0.110 (0.239)	-0.126 (0.242)
$\text{ATT}_{t=6}$	-0.263 (0.448)	-0.255 (0.439)	-0.507 (0.367)	-0.562 (0.393)	-0.105 (0.503)	-0.141 (0.506)
$\text{ATT}_{t=12}$	-0.753** (0.264)	-0.737** (0.251)	-0.557 (0.753)	-0.722 (0.813)	-0.887* (0.428)	-0.939* (0.445)
$\text{ATT}_{t=18}$	-1.143* (0.482)	-1.129* (0.463)	-1.236 (0.842)	-1.506 (0.980)	-1.423* (0.703)	-1.500* (0.748)
Tuning (r / lambda)	0	0	1	1	0.0027	0.0027
F-test p-value	0.533	0.541	0.714	0.763	0.763	0.735
TOST Equiv. p-value	0.529	0.527	0.338	0.300	0.182	0.191
Observations	9760	9760	9024	9024	9024	9024
Years	32	32	32	32	32	32
Treated Reservations	75	75	52	52	52	52
Gaming Control	No	Yes	No	Yes	No	Yes

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates averaged across treated reservations. FEct uses two-way fixed effects; IFEct augments with r latent interactive fixed effects selected via cross-validation; MC uses matrix completion with regularization parameter lambda selected via cross-validation. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

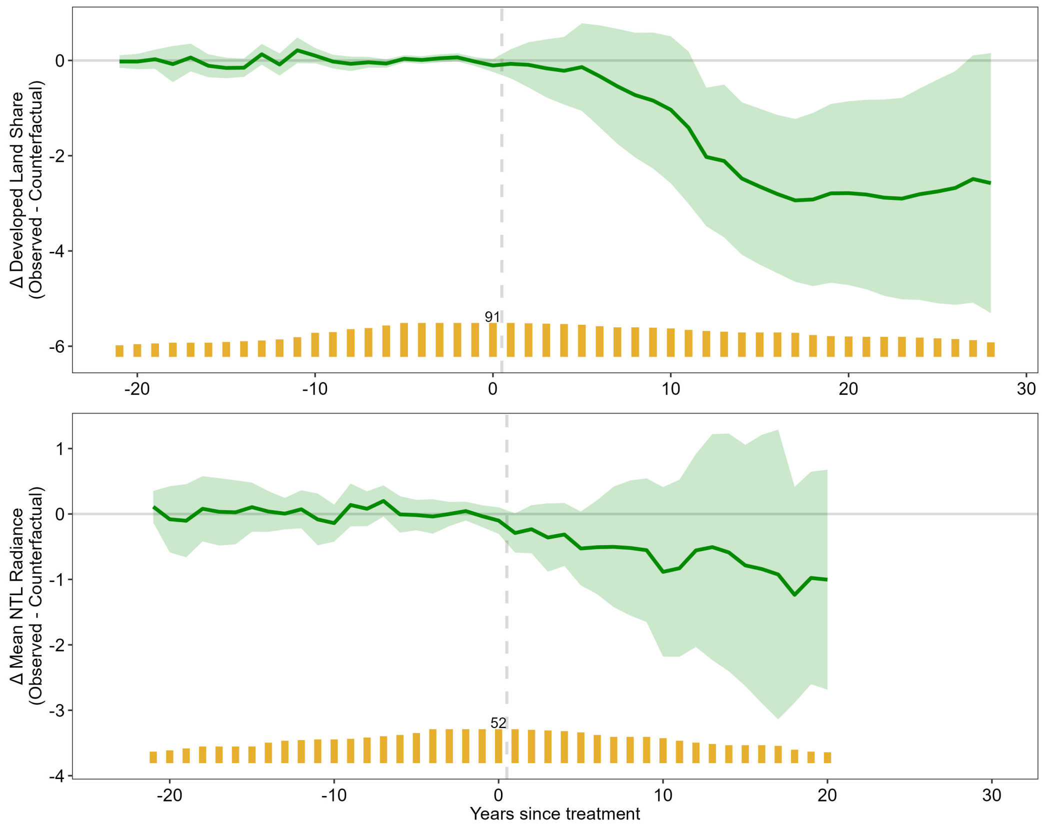

Figure 4: Dynamic treatment effects of self-governance. Plots generated using estimates from IFEct models using the share of developed land (top) and average NTL intensity (bottom) as the outcome. Each point estimate represents the average difference between the estimated control counterfactual and the observed outcome value for all treated units at a given time relative to treatment onset. 95% confidence intervals generated by 1,000 block bootstraps clustered at the unit level. The histogram at bottom of each plot depicts the number of treated units observed at each period. Only time periods where at least 30% of treated units are used in the estimate are plotted.

Outcome Validation

A key assumption in this analysis is that the remote sensing measures must function as reasonable proxies for economic activity on the reservation. To validate that these measures accurately capture economic activity on reservations, I estimate the effect of tribal gaming using these outcome measures. The economic benefits of gaming to reservations are well understood and often discussed. The uncontroversial nature of gaming's overall effect makes it a good test to check the validity of my two economic growth indicators.

In order to estimate the effect of gaming, I again apply the same three counterfactual estimators, plotting in Figure 5 the ATT over time for both outcomes for the IFEct estimator.[20]

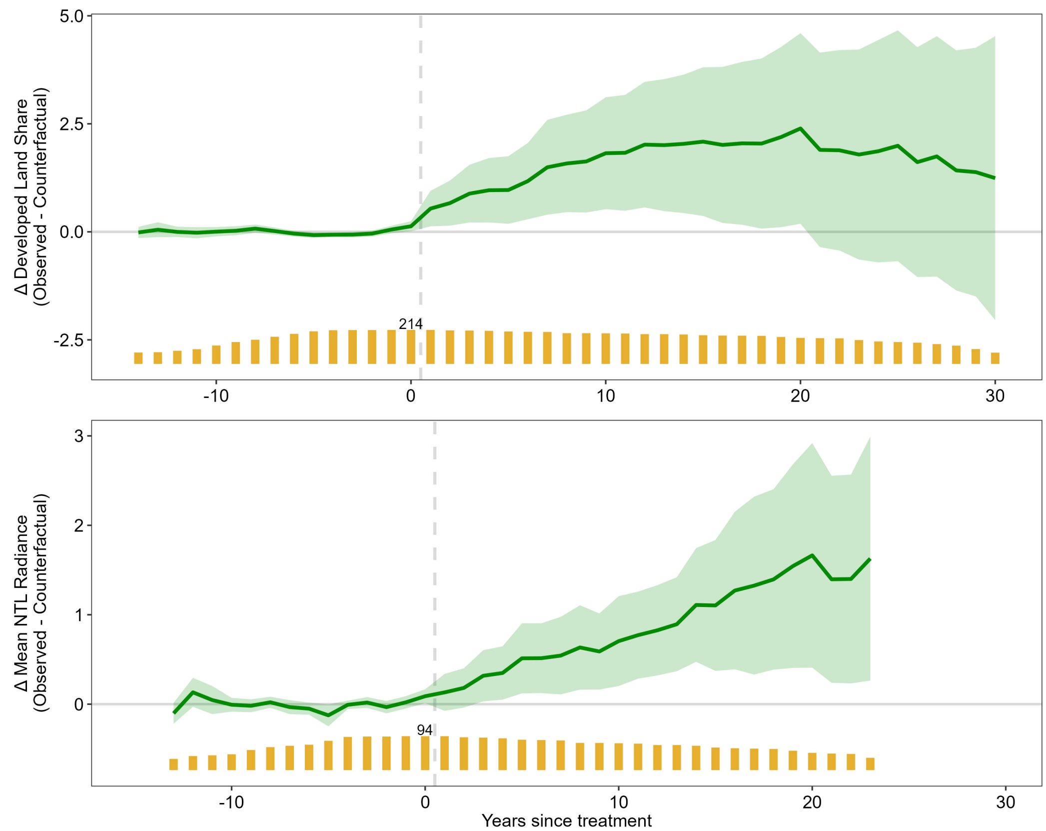

Figure 5: Dynamic treatment effects of gaming. Plots generated using estimates from IFEct models using the share of developed land (top) and average NTL intensity (bottom) as the outcome. Each point estimate represents average difference between the estimated control counterfactual and the observed outcome value for all treated units at a given time relative to treatment onset. 95% confidence intervals generated by 1,000 block bootstraps clustered at the unit level. The histogram at bottom of each plot depict the number of treated units observed at each period. Only time periods where at least 30% of treated units are used in the estimate are plotted.

For both the NTL intensity and developed land share measure, I am able to detect a clear, positive effect of a tribe owning a casino. More specifically, these results show that opening a casino increased the share of developed land on a reservation by about 1.58 percentage points on average over the next 30 years and the average NTL intensity by about 0.822 over the next 20 years. These results indicate that both remote sensing measures are likely fair proxies for reservation economic development.

[20] A table summarizing results from all three estimators is available in Appendix C.

I also examine how change in the remote sensing data correlates with median AIAN household income data on reservations. I pull this income data from the Decennial Census (1990 and 2000) and ACS 5-year running averages (2005-2023). I report the results in Appendix D. Broadly, both measures correlate highly with income change between the two Census years, but do not perform as well predicting ACS-reported income. However, using ACS data for reservations is largely discouraged given poor sample coverage on reservations (Connolly and Jacobs 2020). Regardless, the validity of the remote sensing measures is more modest in this test.

Robustness Check: Placebo Test

The pre-treatment difference between control and treated reservations is fairly tight around zero in my results. But this may actually mean that the IFEct estimator is over-fitting to the pre-treatment data and the estimated effects are really just the inability for the individual factor loading interactions to properly predict post-treatment outcomes. To evaluate how serious this issue may be, I employ a placebo test which artificially moves the start of treatment back ten years. By doing this, I hide pre-treatment outcomes from the estimator. If it estimates an effect in these placebo years, the over-fitting concern is justified. In Appendix E, I report the results of the placebo tests for both outcomes with a dynamic treatment plot. Both remain consistently close to zero in the placebo years. This adds confidence that the reported results above are not due to overfitting.

Robustness Check: Alternative Estimators

Another concern may be that the counterfactual estimator strategies may be an attempt to cherry-pick a bespoke empirical strategy to find significant results. To show the consistency of the estimated ATTs, I report the results using a classic two-way fixed effects design, along with modern improved estimators, in Appendix F. All models report a similar effect in magnitude and statistical significance. One exception to this is that the effect on developed land share is not statistically significant using CSDID not-yet-treated controls. However, this may be due to the staggered nature of my data where many treatment cohorts consist of only one or two reservations.

6 Mechanism Testing

Next, I examine how the treatment effect varies across reservation and tribal characteristics to test which mechanisms drive the costs of self-governance. I group eight proxies into three hypothesized mechanisms. The first, *resource sufficiency*, captures whether a tribal government has the administrative resources and access needed to manage its expanded responsibilities under an SGC; I measure this with reservation size, AIAN population, tribal enrollment, and distance to the nearest large city. The second, *policymaking efficiency*, captures whether the tribe's institutional structure allows it to translate preferences into policy with low coordination costs; I measure this with the degree of ethnic factionalization in the tribe, the electoral system for the tribal executive, and the use of direct-democracy assemblies. The third, *interest capture*, captures whether the political incentives of on-reservation residents diverge from broader development goals; I measure this with the residency voting requirement, which restricts the electorate to current on-reservation residents. I dichotomize each proxy, either as above/below the median average reservation value or as the presence or absence of a particular institutional form.

To test these predictions, I estimate separate ATTs for each subgroup within the same IFEct models. Each model is estimated using the entire sample of reservations and then the ATTs are estimated by averaging the treatment effects for only those units within the subgroup. This does not test for the statistical significance between the subgroups. However, it does allow for diagnostic testing for each subgroup effect while maintaining the entire sample for counterfactual generation. It is also worth noting that these characteristics and institutions are not randomly assigned and causal interpretation would not be justified. This exercise is initial evidence for the potential role of these mechanisms in economic outcomes.

I begin with the resource sufficiency proxies in Table 2. The pattern is clear and consistent across the first three measures: the negative effects from self-governance are concentrated among reservations with large land areas, large AIAN populations, and high enrollment. For each of these proxies, the "large" subgroup shows a strongly negative and statistically significant ATT on developed land share that grows in magnitude over the post-treatment horizon, while the "small" subgroup shows null effects with point estimates near zero. The same pattern carries over to NTL radiance for reservation size and AIAN population, although the small-group estimates are noisier and the enrollment split weakens. This aligns with my expectation that the administrative demands of an SGC grow with the size and complexity of a reservation: tribes managing more land, more people, or more enrollees face a greater burden when they take on the responsibilities of self-governance.

Distance to the nearest large city, the fourth resource-sufficiency proxy, does not show a comparably clean split. Both the far- and near-city subgroups have similar overall ATTs on developed land that are small in magnitude over the full horizon and reach statistical significance only in the late post-treatment periods. The far subgroup does show a somewhat more negative effect on NTL radiance, but the difference between the subgroups is modest.

Next, in Table 3, I present the results for the governance proxies, separated into two mechanisms: policymaking efficiency and interest capture. Before interpreting the coefficients, it is worth emphasizing that many of the groups in these models have low reservation counts, particularly with the NTL outcome where later data availability means more reservations lack pre-treatment observations. As a result, the NTL models find few significant differences across subgroups.

Within policymaking efficiency, the ethnic factionalization split does not produce statistically significant effects in either subgroup of the developed land model. The signs also point in opposite directions across the two outcomes: high factionalization is more negative for developed land, while low factionalization shows a more negative late-period effect for NTL radiance. My initial expectation was that tribes with high ethnic factionalization would have lower capacity to coordinate on policy and, thus, worse performance under an SGC. The low count of treated reservations in each subgroup, however, makes these estimates noisy and likely not robust to alternative specifications.

The other two policymaking-efficiency proxies have more compelling results in the developed land model. As I predicted, reservations whose tribal government has a direct-democracy general assembly show a noticeably larger negative effect from SGC adoption than reservations without one. These institutions may be slower or less efficient at translating policy preferences into

Table 2: Heterogeneous Effects of SGC by Resource Sufficiency

	N_{tr}	ATT	$\text{ATT}_{t=1}$	$\text{ATT}_{t=6}$	$\text{ATT}_{t=12}$	$\text{ATT}_{t=18}$
Panel A: Developed Land Share						
Reservation Size						
Large	47	-3.405** (0.486)	-0.397** (0.102)	-1.655** (0.313)	-3.652** (0.524)	-4.643** (0.646)
Small	44	0.795 (1.236)	0.300 (0.276)	1.231 (0.986)	0.178 (1.334)	0.084 (1.798)
AIAN Population						
Large	48	-2.849** (0.540)	-0.281** (0.107)	-1.085** (0.357)	-2.857** (0.689)	-4.167** (0.741)
Small	42	0.823 (1.250)	0.313 (0.308)	1.041 (1.040)	-0.086 (1.388)	0.507 (2.083)
Enrollment						
Large	56	-2.760** (0.552)	-0.797** (0.177)	-1.510** (0.408)	-2.206** (0.561)	-3.681** (0.738)
Small	35	0.455 (1.375)	-0.190 (0.325)	0.091 (0.865)	0.100 (1.415)	0.198 (1.725)
Distance to City						
Far	48	-1.599 (1.023)	-0.033 (0.223)	-0.159 (0.807)	-1.967 (1.026)	-3.086* (1.217)
Near	43	-1.465 (0.782)	-0.084 (0.204)	-0.394 (0.612)	-1.954* (0.921)	-2.461* (1.159)
Panel B: NTL Radiance						
Reservation Size						
Large	24	-1.019** (0.426)	-0.164* (0.077)	-0.388* (0.184)	-0.999* (0.465)	-1.924* (0.857)
Small	28	-0.413 (0.794)	-0.413 (0.269)	-0.713 (0.651)	-0.427 (1.279)	-0.731 (1.560)
AIAN Population						
Large	22	-1.005** (0.394)	-0.084 (0.111)	-0.414 (0.269)	-1.124* (0.542)	-1.767* (0.711)
Small	29	0.245 (0.776)	0.013 (0.419)	0.521 (0.946)	-0.730 (0.866)	-0.713 (1.734)
Enrollment						
Large	30	-0.916 (0.558)	-0.128 (0.198)	-0.564 (0.459)	-0.899 (0.871)	-1.851* (0.833)
Small	22	-0.391 (0.766)	-0.532* (0.238)	-0.565 (0.441)	-0.305 (0.947)	-1.016 (1.383)
Distance to City						
Far	26	-0.751 (0.438)	-0.008 (0.147)	0.015 (0.608)	-1.016* (0.505)	-1.622* (0.698)
Near	26	0.045 (0.802)	-0.046 (0.444)	0.170 (0.769)	-0.828 (0.980)	-1.087 (1.645)

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. Subgroup categories are organized by the hypothesized mechanism through which self-governance compacting may affect outcomes. N_{tr} is the number of treated reservations in each subgroup. Continuous variables (reservation size, AIAN population, enrollment, distance to city, factionalization) are split at the median across reservations. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

concrete action. Reservations with a directly-elected executive also show a strong negative effect that is roughly five times the magnitude of the indirect-election subgroup. This was contrary to my initial expectation that direct election would sharpen accountability and improve performance.

The interest capture mechanism, measured by the residency voting requirement, shows a similar pattern. Reservations restricting voting to on-reservation residents show a statistically significant negative effect on developed land share, while reservations that allow off-reservation voting do not. I had expected the opposite — that giving voice to off-reservation members would dilute on-reservation interests and weaken the SGC effect.

To visualize how these mechanisms operate, in Figure 6 I plot the dynamic treatment effects for one proxy from each of the first two mechanism categories: reservation size (resource sufficiency) and executive selection (policymaking efficiency). Both proxies show clear separation between their subgroups in the developed land outcome, supporting the idea that more than one mechanism contributes to the heterogeneity in SGC effects. The reservation-size split is the cleanest, with large reservations bearing essentially all of the negative effect while small reservations track close to zero. The executive-selection split is noisier but follows a similar shape, with directly-elected reservations diverging negatively from the indirect-election subgroup over time.

Table 3: Heterogeneous Effects of SGC by Governance Mechanism

	N_{tr}	ATT	ATT$_{t=1}$	ATT$_{t=6}$	ATT$_{t=12}$	ATT$_{t=18}$
Panel A: Developed Land Share						
Policymaking Efficiency						
Ethnic Factionalization						
High Factionalization	32	-0.428 (0.780)	0.194 (0.451)	0.418 (0.798)	-0.647 (0.684)	-1.110 (0.945)
Low Factionalization	13	0.042 (1.507)	0.097 (0.342)	0.284 (1.124)	0.168 (1.829)	0.080 (2.434)
Executive Selection						
Direct Election	49	-2.180** (0.792)	-0.684** (0.244)	-1.116 (0.602)	-1.916* (0.787)	-2.896** (1.036)
Indirect Election	16	-0.421 (1.811)	-0.405 (0.480)	-0.642 (1.159)	-0.688 (1.512)	-0.490 (2.078)
Direct Democracy						
General Assembly	37	-2.157** (0.853)	-0.707* (0.292)	-1.191 (0.706)	-1.881* (0.856)	-2.779* (1.124)
No Assembly	28	-1.034 (1.352)	-0.495 (0.320)	-0.710 (0.827)	-1.183 (1.136)	-1.478 (1.557)
Interest Capture						
Residency Voting Requirement						
No Off-Rez Voting	19	-2.046* (0.813)	-0.461* (0.198)	-1.023* (0.404)	-1.709* (0.757)	-2.795* (1.097)
Off-Rez Voting	46	-1.516 (1.061)	-0.679* (0.303)	-0.972 (0.768)	-1.525 (0.988)	-1.959 (1.284)
Panel B: NTL Radiance						
Policymaking Efficiency						
Ethnic Factionalization						
High Factionalization	14	0.468 (0.693)	-0.142 (0.467)	0.680 (0.807)	0.816 (1.332)	-0.164 (0.668)
Low Factionalization	8	-0.524 (0.322)	-0.170 (0.169)	-0.347 (0.354)	-0.783 (0.559)	-1.283* (0.612)
Executive Selection						
Direct Election	26	-0.592 (0.612)	-0.182 (0.253)	0.236 (0.647)	-0.724 (0.951)	-1.901 (0.991)
Indirect Election	5	-0.152 (1.107)	0.028 (0.418)	0.265 (1.278)	-0.585 (0.630)	-1.023 (1.126)
Direct Democracy						
General Assembly	17	-0.366 (0.718)	-0.216 (0.391)	0.557 (0.871)	-0.544 (1.196)	-1.603 (0.945)
No Assembly	14	-0.715 (0.660)	-0.067 (0.160)	-0.159 (0.701)	-0.918 (0.639)	-1.907 (1.092)
Interest Capture						
Residency Voting Requirement						
No Off-Rez Voting	8	-0.591 (1.167)	-0.365 (0.561)	-0.392 (0.905)	-0.709 (1.971)	-1.514 (1.490)
Off-Rez Voting	23	-0.488 (0.533)	-0.073 (0.219)	0.488 (0.709)	-0.698 (0.503)	-1.803 (0.944)

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. Subgroup categories are organized by the hypothesized mechanism through which self-governance compacting may affect outcomes. N_{tr} is the number of treated reservations in each subgroup. Continuous variables (reservation size, AIAN population, enrollment, distance to city, factionalization) are split at the median across reservations. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

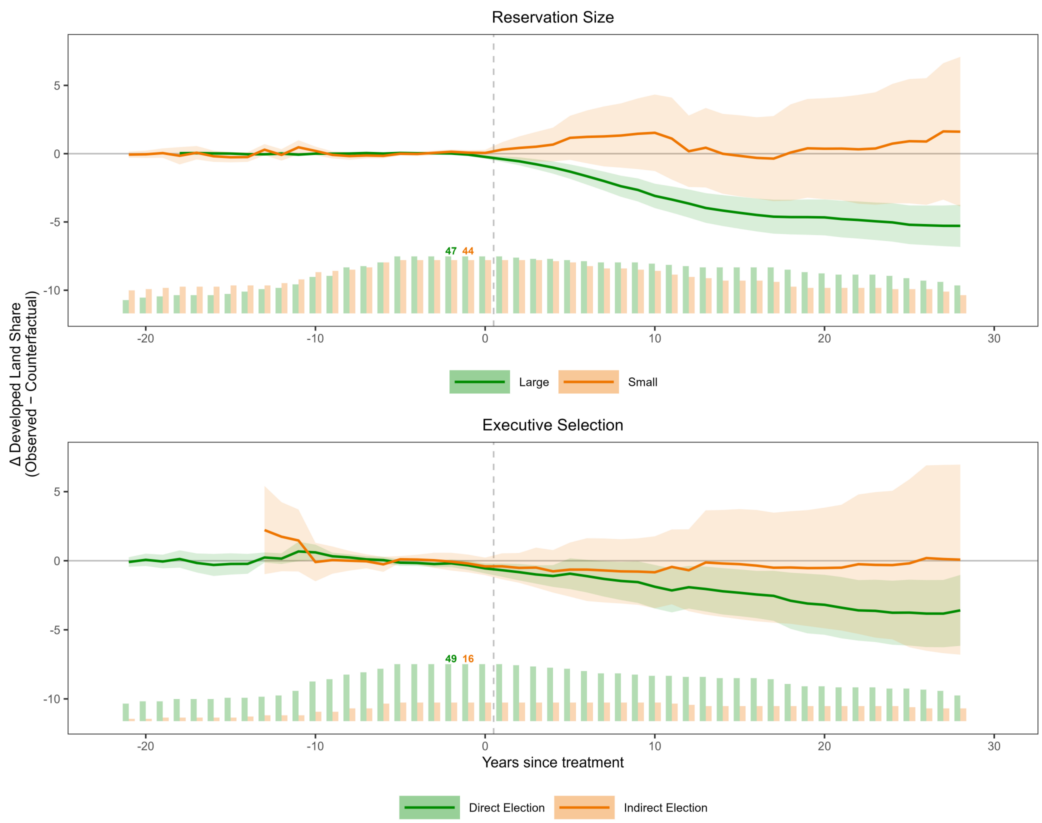

Figure 6: Reservation Size and executive election system effects comparison. Top panel presents difference in estimated effects of SGC adoption by large and small reservation size. The bottom panel presents the same effect, but split by executive electoral system.

Alternate Mechanism: Policy Preferences

One argument against the capacity mechanism is that the change in development is actually a reflection of the average policy preference of tribe members. This argument would assume that SGC tribes were not suffering from a lack of governance capacity, but instead the tribal governments, presumably based on voter sentiment, prefer to reduce the rate of development. In the initial analysis, I make the assumption that those on reservations want to increase development, but it is possible that the tribe may prefer less development compared to the BIA. News stories of tribes opposing development on culturally important natural land are common, suggesting tribes can value the environment differently than the federal government. Quantifying tribal preferences, however, is challenging because we lack public opinion surveying of tribe members. This makes direct evaluation of this argument challenging.

Instead, to test this argument, I go back to the land use measure. If the effect were driven purely by a preference for less development, we would expect the land not converted to developed uses to remain in or revert to its baseline composition with no particular pattern across natural cover categories. However, if the effect reflects a specific policy preference around natural resource extraction, we would expect to see selective changes in the categories most associated with visible extractive activity.

One useful feature of land-use outcomes is that every pixel in a reservation is coded and can only possess one value at a time. This means that when I estimate that the share of developed land pixels grew slower than expected for SGC reservations, that expected share must go to some other category of land-use. What other category saw an increased share relative to the un-treated expectation can then potentially help us understand what is actually occurring on these reservations.

In Table 4, I report the effect of entering an SGC on the different land classification categories in the NLCD using the IFEct estimator. I find that the reduction in developed land is accompanied by a corresponding increase of similar magnitude in forest cover. Conversely, shrubland (desert land) and herbaceous land (grasslands) see no change or a decrease, respectively. If the reduction in developed land reflected a general preference against development, we would not necessarily expect the shift to be concentrated in forest cover specifically. This potentially suggests tribes are choosing less deforestation rather than less development broadly. That said, this pattern could also reflect the fact that the reservations driving the developed land effect happen to be located in forested areas, in which case the forest increase would simply reflect the local land composition rather than a deliberate policy choice.

Alternate Mechanism: External Investment Flight

Another possible explanation for the main findings is that entering an SGC reduces external investment. Reservations tend to be rural and lower income, so investment capital must come from tribal businesses, the federal government, or off-reservation investors. As Anderson and Parker (2008) and Wellhausen et al. (2017) argue, greater tribal policy power can make external investors

Table 4: Effect of Self-Governance Compacting on Land Cover Categories

	Forest (1)	Shrubland (2)	Herbaceous (3)	Cultivated (4)	Wetlands (5)	Barren (6)
ATT	2.114** (0.678)	0.283 (0.848)	-1.061* (0.480)	-0.022 (0.504)	0.125* (0.058)	-0.042 (0.110)
$\text{ATT}_{t=1}$	0.114 (0.180)	0.180 (0.416)	-0.699 (0.413)	-0.129 (0.175)	0.009 (0.026)	0.015 (0.029)
$\text{ATT}_{t=6}$	0.485 (0.695)	1.040 (0.970)	-0.917 (0.514)	-0.252 (0.409)	0.065 (0.050)	-0.073 (0.126)
$\text{ATT}_{t=12}$	2.489** (0.945)	0.533 (1.134)	-1.496* (0.617)	-0.050 (0.579)	0.173* (0.074)	-0.023 (0.112)
$\text{ATT}_{t=18}$	2.993** (0.955)	-0.234 (1.144)	-0.820 (0.647)	0.292 (0.641)	0.157* (0.074)	-0.133 (0.167)
Latent Factors (r)	4	4	0	4	4	2
F-test p-value	0.872	0.274	0.498	0.885	0.621	0.620
TOST Equiv. p-value	0.086	0.161	0.895	0.000	0.000	0.000
Observations	12792	12792	12792	12792	12792	12792
Years	39	39	39	39	39	39
Treated Reservations	91	91	91	91	91	91

Note: Each column reports the IFEct estimate of the SGC effect on a different NLCD land cover category (percentage share), with gaming as a control. Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates averaged across treated reservations. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

more apprehensive: tribal governments are less familiar than the federal government, potentially more variable, and may raise the perceived risk of expropriation or unfair treatment.

To test this argument, I examine the effect of SGCs on the likelihood of a tribe having a casino. Many tribes partner with established gaming firms like Caesars or Harrah's to manage their casinos. If SGCs deter off-reservation investors, we would expect them to reduce the likelihood of a tribe having a casino as private partners grow hesitant. Using the binary casino measure as an outcome, I estimate a linear probability model with the Callaway and Sant'Anna (2021) difference-in-differences estimator, clustering standard errors by reservation. As Figure 7 shows, the effect is null to positive rather than negative. This suggests SGCs did not drive away external investment, at least among gaming investors.

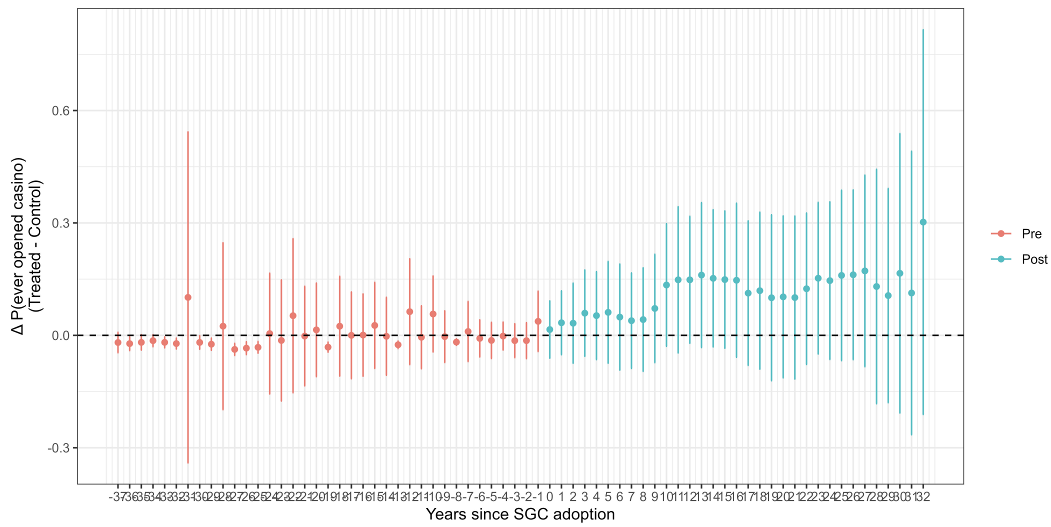

Figure 7: Effect of SGCs on Casino Opening. Dynamic treatment estimates from the Callaway-Sant'Anna staggered difference-in-differences estimator. Each point is the difference in the probability that a reservation's tribe has ever opened a casino, between SGC-adopting reservations and not-yet-adopting reservations, at the given number of years relative to SGC adoption. Bars are 95% simultaneous confidence intervals from the multiplier bootstrap.

7 Conclusion

This study examined whether local self-governance improves community outcomes by analyzing the economic effects of expanded self-governance for American Indian tribal governments. Contrary to prevailing assumptions in both policy and academic discourse, the findings indicate that compacting significantly slowed reservation economic growth.

Given the normative appeal of Indigenous self-governance, this finding is unfortunate. However, the negative effect was not ubiquitous; a number of reservations saw no decrease in economic activity. Furthermore, the results also indicate that performance under self-governance improved as tribes gained more experience. This is important to keep in mind. It takes time to develop bureaucratic strength and expertise, so a learning curve should be expected. Additionally, if governance capacity is the underlying issue, this means a solution is possible. Before implementing an SGC, tribes should learn from more experienced self-governance tribes how to handle compacted programs to better prepare. And the federal government should better fund self-governance programs to give tribes more resources to apply.

These findings contribute to broader debates about the limits and potential of local governance. They underscore that greater autonomy does not automatically translate into improved outcomes; the institutional and administrative capacity of local governments is a central determinant of success. This insight extends to the study of American local politics more generally, where similar questions about capacity, resources, and autonomy remain pressing.

For scholars of American Indian politics, the results highlight persistent challenges in building and sustaining tribal governance capacity. They align with prior work suggesting that sovereignty carries costs that Native nations may willingly bear for the sake of self-determination. The findings therefore call for policies and institutional support aimed at strengthening tribal administrative and fiscal capacity so that self-governance produces both political and economic benefits.

This study highlights a number of avenues for future research on tribal institutions, capacity, and governance. Continued exploration of how to measure tribal governance capacity will be important for our understanding what is possible under self-governance. We need to keep working to understand tribal institutions, both in breadth and depth, to better conceptualize and model the features of tribal governments. And we need to work more on learning what tribe members want from their governments to better interpret or judge the actions of the governments.

Finally, this study suggests opportunities for cross-fertilization between the study of Indigenous governance and local politics. Many of the day-to-day challenges faced by American Indian tribal governments mirror those of local governments. Scholars of American Indian politics have studied how tribes deal with issues like policing and public safety (Crepelle et al. 2022; Crepelle, Fegley and Murtazashvili 2024), business and infrastructure development (Bauer, Feir and Gregg 2022; Ratté and Anderson 2022), community health (Foxworth et al. 2022), and engagement with local, state, and federal governments (Witmer and Boehmke 2007; Evans 2011*a*). Yet rarely do these works draw on ideas developed in the local politics literature, nor are their findings incorporated into future local politics work. By recognizing that issues commonly analyzed in local political studies—such as housing, education, and land use—have direct analogues in the Indigenous context, this paper attempts to bring these two literatures into a closer dialogue for the future benefit of both.

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A Treatment Adoption Across the Panel

Figure A1: Panel view of self-governance compact adoption across reservations over time. Each row represents a reservation, ordered by treatment timing. Shading indicates treatment status by year.

B VIIRS Nighttime Light Imputation

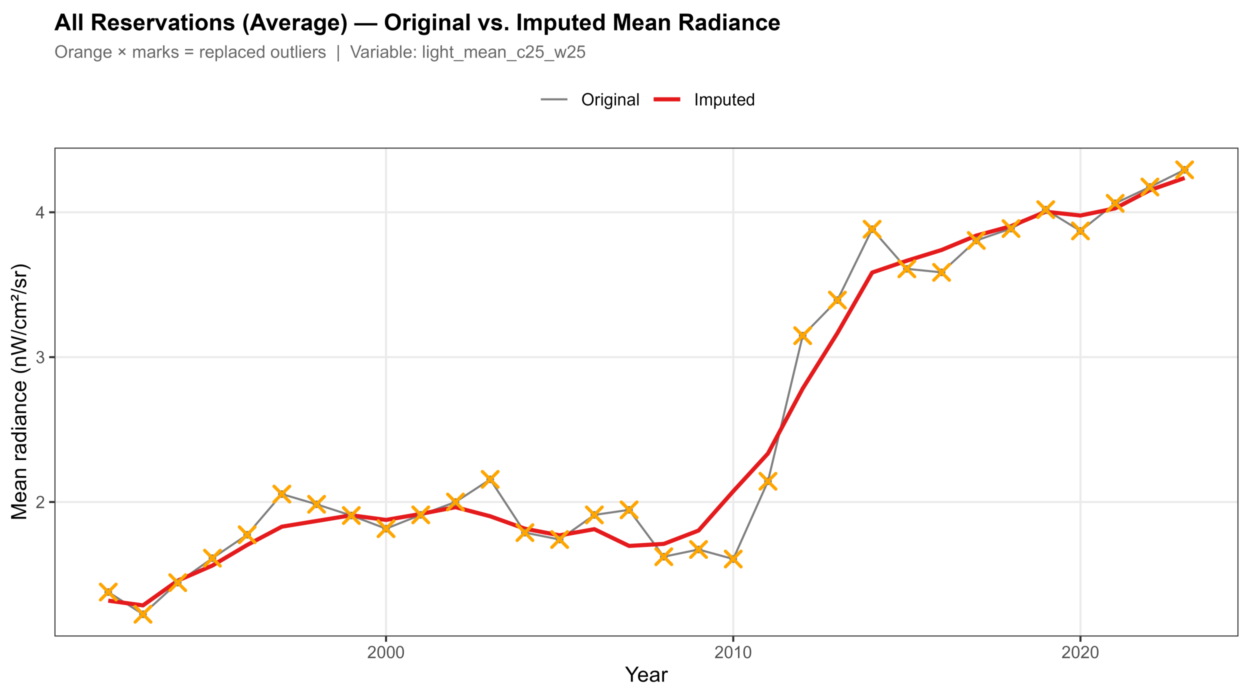

Figure B1: Average nighttime light radiance across all reservations, showing the DMSP-OLS series (1992–2013), the VIIRS series (2012–present), and the imputed bridge between the two. The imputation leverages the overlap period (2012–2013) to calibrate the DMSP values onto the VIIRS scale.

C Outcome Validation: Effect of Casino Gaming

Table C1: Effect of Casino Gaming on Economic Development (Outcome Validation)

	FEct (1)	IFEct (2)	MC (3)
Panel A: Developed Land Share			
ATT	1.645* (0.724)	1.581* (0.772)	1.632* (0.787)
$\text{ATT}_{t=1}$	0.690* (0.296)	0.537* (0.210)	0.695* (0.278)
$\text{ATT}_{t=6}$	1.427** (0.481)	1.174** (0.451)	1.409** (0.494)
$\text{ATT}_{t=12}$	1.991** (0.690)	2.018** (0.741)	1.941* (0.758)
$\text{ATT}_{t=18}$	2.022* (0.892)	2.043* (1.005)	1.987* (0.994)
Tuning (r / lambda)	0	3	0.0013
F-test p-value	0.215	0.338	0.054
TOST Equiv. p-value	0.727	0.021	0.404
Observations	11856	11037	11037
Years	39	39	39
Treated Reservations	230	209	209
Panel B: NTL Radiance			
ATT	1.189** (0.265)	0.822** (0.251)	0.897** (0.281)
$\text{ATT}_{t=1}$	0.153* (0.072)	0.131 (0.105)	0.186* (0.095)
$\text{ATT}_{t=6}$	0.570** (0.166)	0.514** (0.199)	0.567** (0.193)
$\text{ATT}_{t=12}$	0.796** (0.236)	0.826** (0.258)	0.955** (0.309)
$\text{ATT}_{t=18}$	1.605** (0.378)	1.395** (0.515)	1.482** (0.545)
Tuning (r / lambda)	0	2	0.0019
F-test p-value	0.675	0.481	0.441
TOST Equiv. p-value	0.096	0.024	0.018
Observations	7712	5216	5216
Years	32	32	32
Treated Reservations	168	90	90

Note: Treatment is casino gaming operation. Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates averaged across treated reservations. FEct uses two-way fixed effects; IFEct augments with r latent interactive fixed effects selected via cross-validation; MC uses matrix completion with regularization parameter lambda selected via cross-validation. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to gaming adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

D Outcome Validation: Correlation with HH Income

Table D1: Within-unit panel regressions of log household income on log remote sensing measures. Columns 1–4 use the 15-wave ACS 5-year rolling panel without fixed effects; columns 5–8 add reservation and 5-year-period fixed effects. Columns 9–12 use the four decennial census years (income observed in 1990 and 2000 only) without fixed effects; columns 13–16 add reservation and census-year fixed effects. All RS predictors enter as $\log(x+1)$ to accommodate zero-valued observations. Standard errors clustered at the reservation. 1990 VIIRS uses the 1992 reservation-level value as a proxy (nightlight coverage begins 1992).

Dependent Variables:	log(HHINC tot, ACS)		log(HHINC AIAN, ACS)		log(HHINC tot, ACS)		log(HHINC AIAN, ACS)		log(HHINC tot, Census)		log(HHINC AIAN, Census)		log(HHINC tot, Census)		log(HHINC AIAN, Census)	
	ACS No FE				ACS TWFE				Census No FE				Census TWFE			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Variables																
Constant	10.52^{***}	10.54^{***}	10.42^{***}	10.44^{***}					9.885^{***}	9.930^{***}	9.777^{***}	9.800^{***}				
	(0.0409)	(0.0232)	(0.0436)	(0.0245)					(0.0405)	(0.0228)	(0.0424)	(0.0233)				
log(NLCD+1)	0.0306		0.0461^{**}		-0.3839		-0.4073		0.0530^{**}		0.0479^{**}		0.7413^{***}		0.7721^{**}	
	(0.0187)		(0.0198)		(0.3126)		(0.3777)		(0.0214)		(0.0216)		(0.2464)		(0.3180)	
log(VIIRS+1)		0.0793^{***}		0.1186^{***}		-0.0137		0.0374		0.1352^{***}		0.1632^{***}		0.2558^{***}		0.3174^{***}
		(0.0271)		(0.0283)		(0.0632)		(0.0733)		(0.0308)		(0.0332)		(0.0719)		(0.0840)
Fixed-effects																
Reservation					Yes	Yes	Yes	Yes					Yes	Yes	Yes	Yes
Period					Yes	Yes	Yes	Yes								
Census Year													Yes	Yes	Yes	Yes
Fit statistics																
Observations	4,023	3,992	3,879	3,858	4,019	3,990	3,875	3,854	477	472	454	450	372	370	342	342
R^2	0.00594	0.02266	0.01117	0.04185	0.71172	0.71324	0.66963	0.66981	0.01553	0.04530	0.01101	0.05574	0.84603	0.84444	0.83705	0.83371
Within R^2					0.00185	7.12×10^{-5}	0.00148	0.00038					0.08948	0.05345	0.08299	0.06420

Clustered (Reservation) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table D2: Long-difference regressions of change in log household income on change in log remote sensing measures, one observation per reservation. Columns 1–4 use the change between the 2005–2009 and 2019–2023 ACS 5-year estimates; columns 5–8 use the change between the 1990 and 2000 decennial censuses. All variables enter as $\log(x + 1)$ for the RS predictors and $\log(x)$ for household income. Heteroskedasticity- robust standard errors. 1990 VIIRS uses the 1992 reservation-level value as a proxy (nightlight coverage begins 1992).

Dependent Variables:	Δ log(HHINC tot, ACS)		Δ log(HHINC AIAN, ACS)		Δ log(HHINC tot, Census)		Δ log(HHINC AIAN, Census)	
	ACS (2005-09 to 2019-23)				Census (1990 to 2000)			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Constant	0.4828***	0.4840***	0.4551***	0.4194***	0.3793***	0.4277***	0.4746***	0.5235***
	(0.0440)	(0.0371)	(0.0515)	(0.0376)	(0.0299)	(0.0246)	(0.0354)	(0.0272)
Δ log(NLCD+1)	-0.1621		-0.1281		0.7413***		0.7721**	
	(0.3757)		(0.4948)		(0.2464)		(0.3180)	
Δ log(VIIRS+1)		-0.0600		0.0820		0.2558***		0.3174***
		(0.0763)		(0.0966)		(0.0719)		(0.0840)
Fit statistics								
Observations	242	241	230	230	186	185	171	171
R^2	0.00101	0.00263	0.00044	0.00384	0.08948	0.05345	0.08299	0.06420
Adjusted R^2	-0.00316	-0.00154	-0.00394	-0.00053	0.08453	0.04828	0.07757	0.05866

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

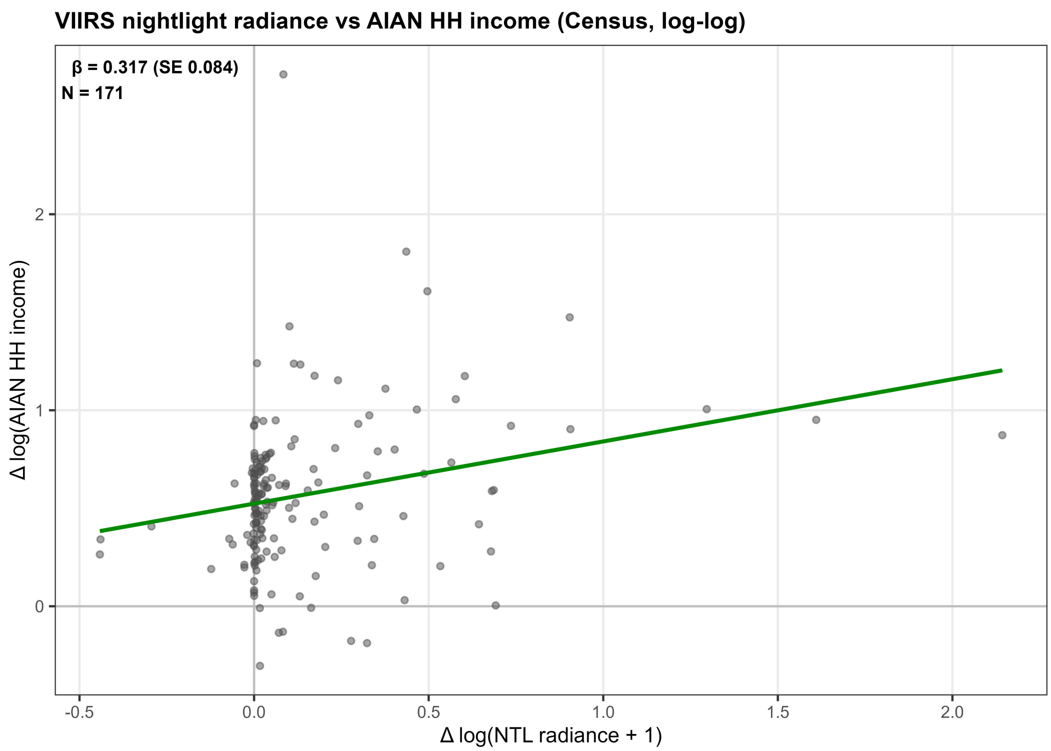

Figure D1: Long-difference scatter of change in log(median AIAN household income) on change in log(mean VIIRS nightlight radiance + 1) between the 1990 and 2000 decennial censuses; each point is one reservation. The fitted line reflects the OLS slope reported in the upper-left corner, with heteroskedasticity-robust standard errors. The 1990 VIIRS value is proxied by the 1992 reservation-level reading because the DMSP-OLS nightlight series begins in 1992. Construction follows the long-difference panel of Henderson, Storeygard, and Weil (2012, Figure 6).

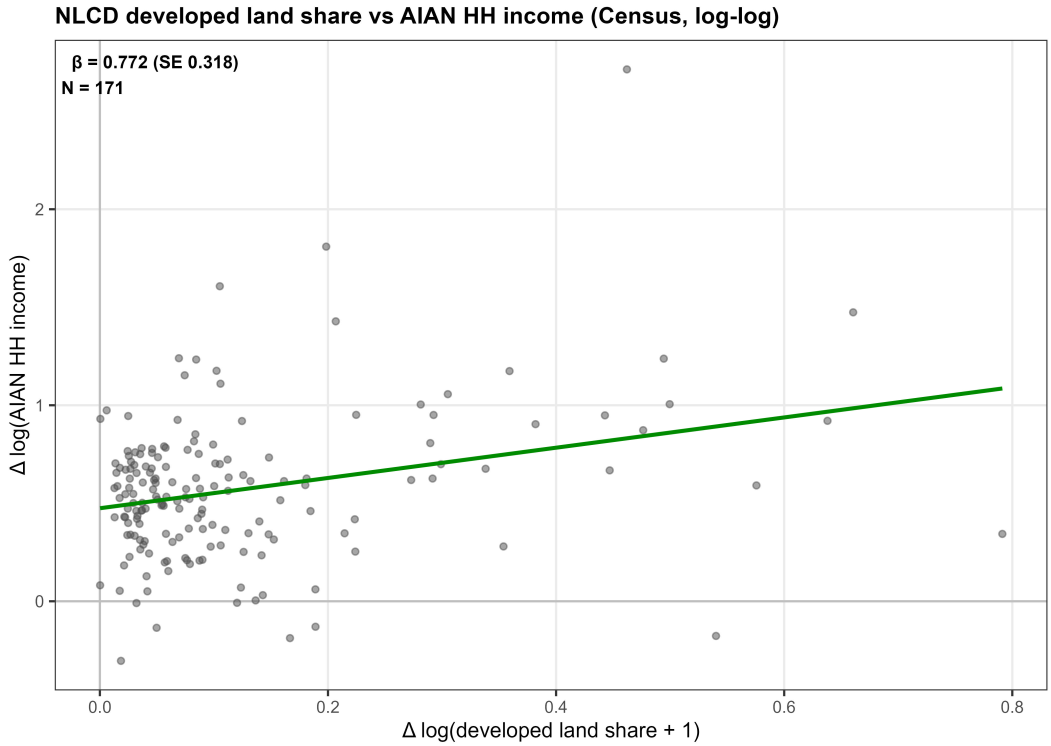

Figure D2: Long-difference scatter of change in log(median AIAN household income) on change in log(developed land share + 1) between the 1990 and 2000 decennial censuses; each point is one reservation. The fitted line reflects the OLS slope reported in the upper-left corner, with heteroskedasticity-robust standard errors.

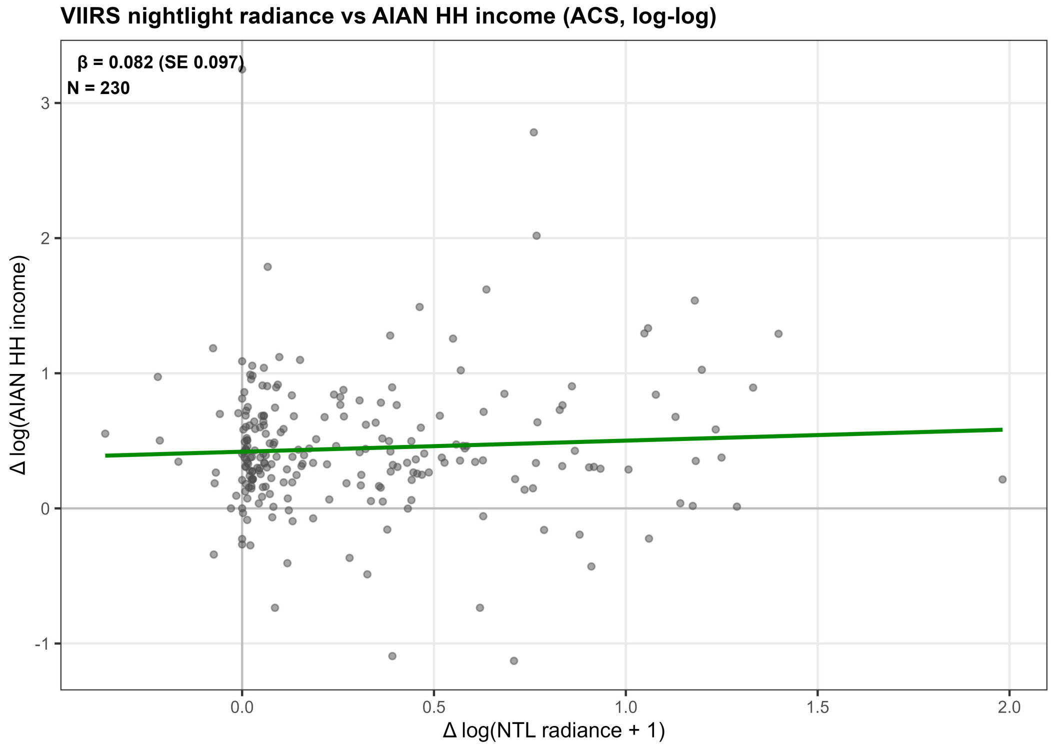

Figure D3: Long-difference scatter of change in log(median AIAN household income) on change in log(mean VIIRS nightlight radiance + 1) between the 2005–2009 and 2019–2023 ACS 5-year estimates; each point is one reservation. The fitted line reflects the OLS slope reported in the upper-left corner, with heteroskedasticity-robust standard errors.

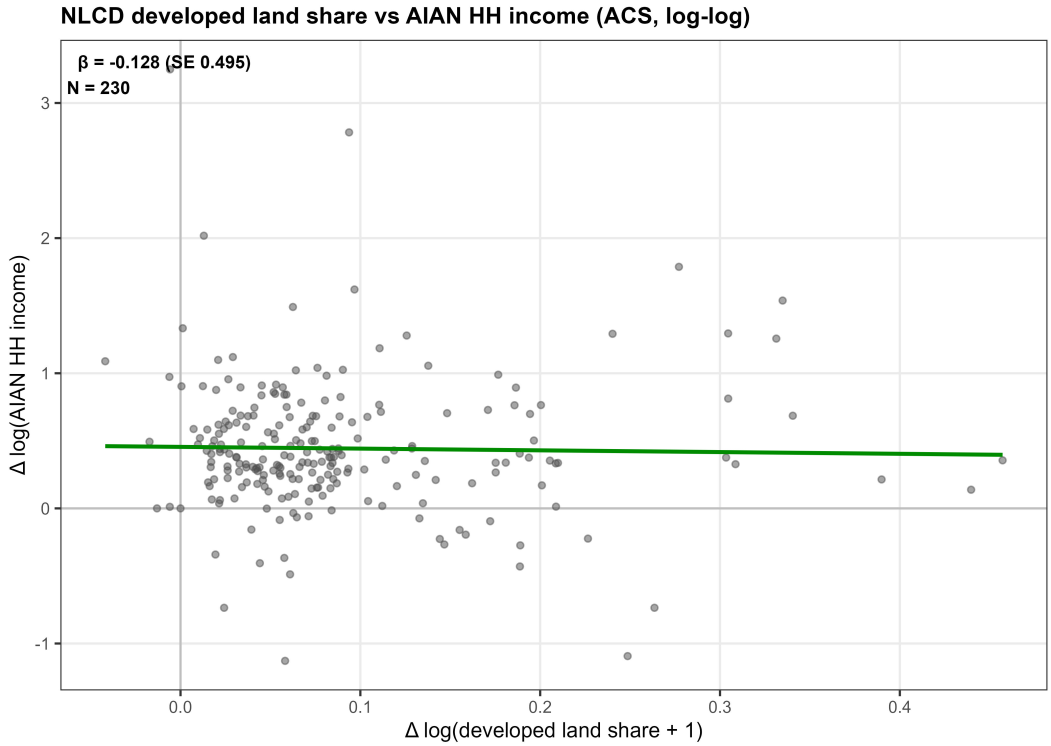

Figure D4: Long-difference scatter of change in log(median AIAN household income) on change in log(developed land share + 1) between the 2005–2009 and 2019–2023 ACS 5-year estimates; each point is one reservation. The fitted line reflects the OLS slope reported in the upper-left corner, with heteroskedasticity-robust standard errors.

E Placebo Test

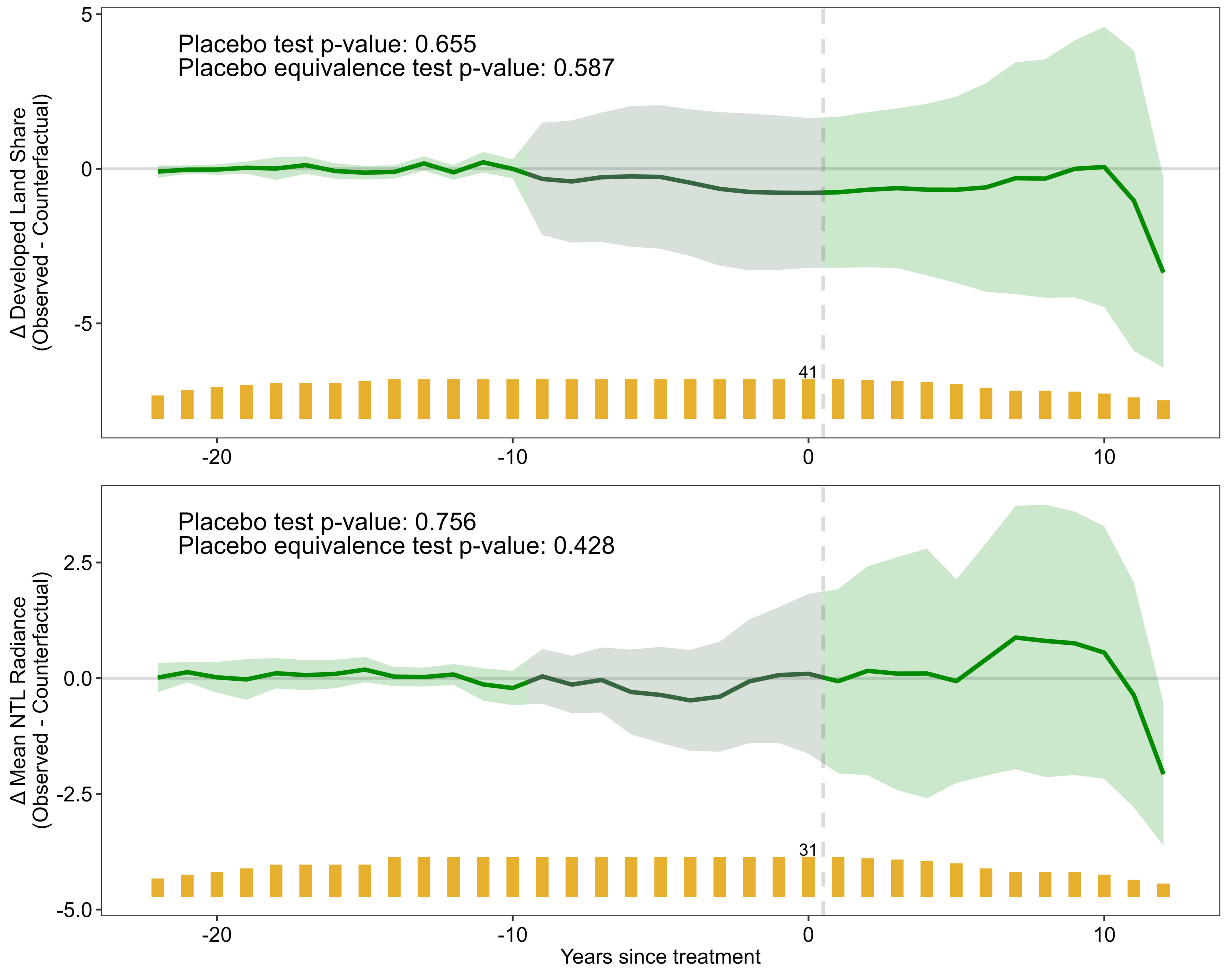

Figure E1: Placebo test results. Treatment timing is shifted 10 years prior to actual compact adoption, and the model is re-estimated to check for false treatment effects. The shaded region indicates the placebo post-treatment window.

F Alternative Difference-in-Differences Estimators

Table F1: SGC Effect Across Alternative Estimators

	TWFE	Stacked	Sun-Abraham	CS
	(1)	(2)	(3)	(4)
Panel A: Developed Land Share				
ATT	-1.642* (0.643)	-1.793** (0.531)	-1.436* (0.612)	-1.213 (0.831)
$\text{ATT}_{t=1}$	-0.050 (0.055)	-0.058 (0.051)	-0.070 (0.058)	-0.139 (0.119)
$\text{ATT}_{t=6}$	-0.463 (0.329)	-0.501 (0.285)	-0.531 (0.320)	-0.616 (0.511)
$\text{ATT}_{t=12}$	-1.447* (0.591)	-1.467** (0.477)	-1.374* (0.601)	-1.187 (0.867)
$\text{ATT}_{t=18}$	-2.295** (0.745)	-2.289** (0.564)	-2.257** (0.825)	-1.787 (1.144)
Observations				
Years				
Treated Reservations	0	0	0	0
Panel B: NTL Radiance				
ATT	-0.348 (0.544)	-0.434 (0.469)	-0.505 (0.297)	-0.409 (0.289)
$\text{ATT}_{t=1}$	-0.128 (0.082)	-0.140 (0.075)	-0.140* (0.070)	-0.137 (0.076)
$\text{ATT}_{t=6}$	-0.250 (0.282)	-0.265 (0.251)	-0.360 (0.219)	-0.289 (0.221)
$\text{ATT}_{t=12}$	-0.611* (0.260)	-0.589** (0.199)	-0.555** (0.209)	-0.472* (0.217)
$\text{ATT}_{t=18}$	-1.076* (0.488)	-1.069** (0.304)	-0.941* (0.448)	-0.806* (0.404)
Observations				
Years				
Treated Reservations	0	0	0	0

Note: Each column reports the estimated ATT from a different estimator, all with gaming as a control. TWFE is the classic two-way fixed effects regression. Stacked uses cohort-specific 2×2 comparisons (Cengiz et al. 2019). Sun-Abraham is the interaction-weighted estimator (Sun & Abraham 2021). CS is the Callaway & Sant'Anna (2021) estimator using not-yet-treated controls. Standard errors in parentheses (cluster-robust for TWFE/Stacked/Sun-Abraham; analytical for CS). CS estimates may be less precise due to the large number of small treatment cohorts inherent in staggered SGC adoption. * $p < 0.05$; ** $p < 0.01$.

G Heterogeneous Treatment Effects by Reservation Characteristics

G.1 Reservation Size

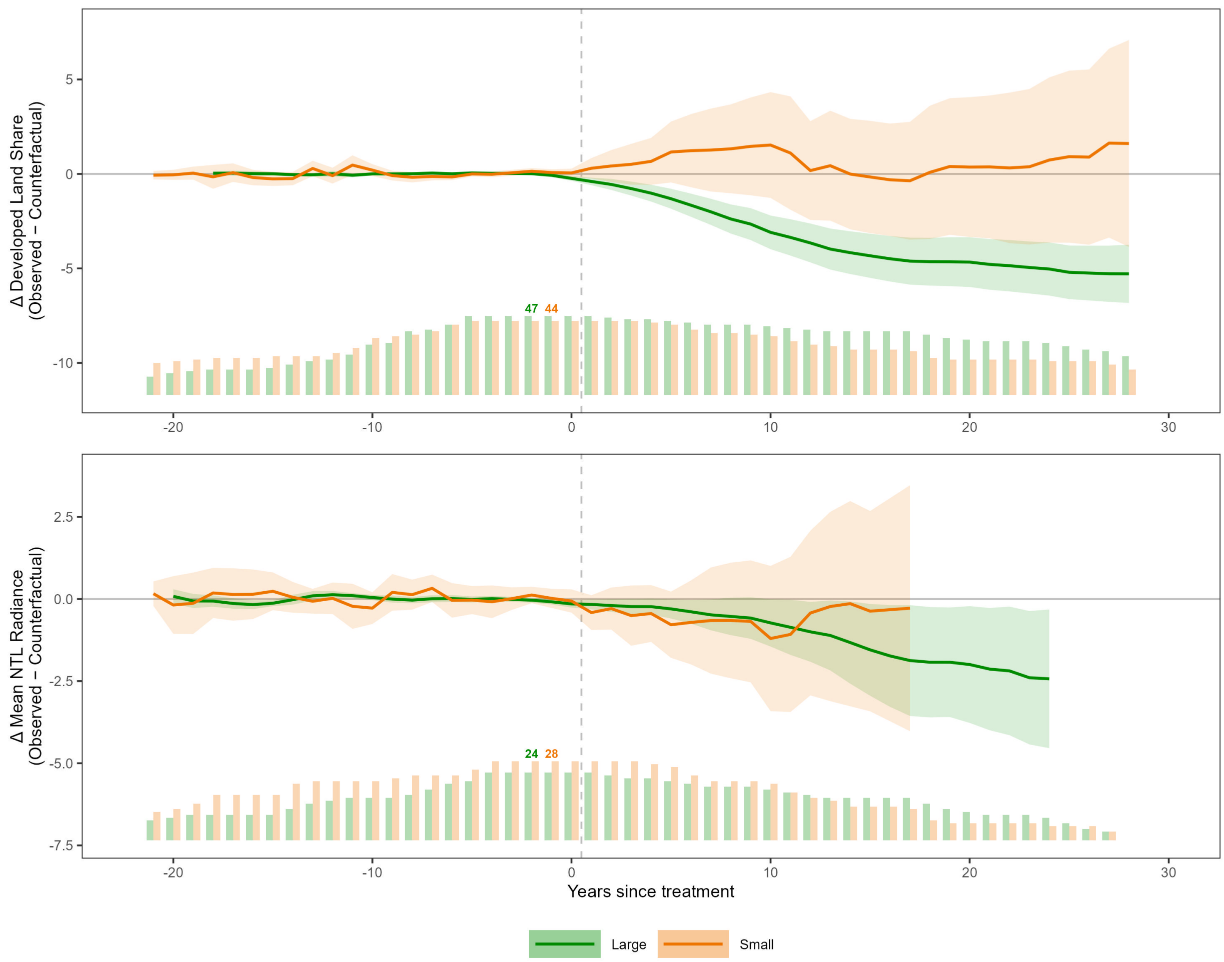

Figure G1: Dynamic treatment effects by reservation size (median split on area). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G1: Heterogeneous Effects of SGC by Reservation Size — Full Diagnostics

	Large	Small
Panel A: Developed Land Share		
ATT	-3.405** (0.486)	0.795 (1.236)
$\text{ATT}_{t=1}$	-0.397** (0.102)	0.300 (0.276)
$\text{ATT}_{t=6}$	-1.655** (0.313)	1.231 (0.986)
$\text{ATT}_{t=12}$	-3.652** (0.524)	0.178 (1.334)
$\text{ATT}_{t=18}$	-4.643** (0.646)	0.084 (1.798)
Latent Factors (r)	4	4
F-test p-value	0.100	0.100
TOST Equiv. p-value	0.044	0.044
Observations	12792	12792
Years	39	39
Treated Reservations	47	44
Panel B: NTL Radiance		
ATT	-1.019** (0.426)	-0.413 (0.794)
$\text{ATT}_{t=1}$	-0.164* (0.077)	-0.413 (0.269)
$\text{ATT}_{t=6}$	-0.388* (0.184)	-0.713 (0.651)
$\text{ATT}_{t=12}$	-0.999* (0.465)	-0.427 (1.279)
$\text{ATT}_{t=18}$	-1.924* (0.857)	-0.731 (1.560)
Latent Factors (r)	1	1
F-test p-value	0.763	0.763
TOST Equiv. p-value	0.300	0.300
Observations	9024	9024
Years	32	32
Treated Reservations	24	28

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_\varepsilon$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

G.2 AIAN Population

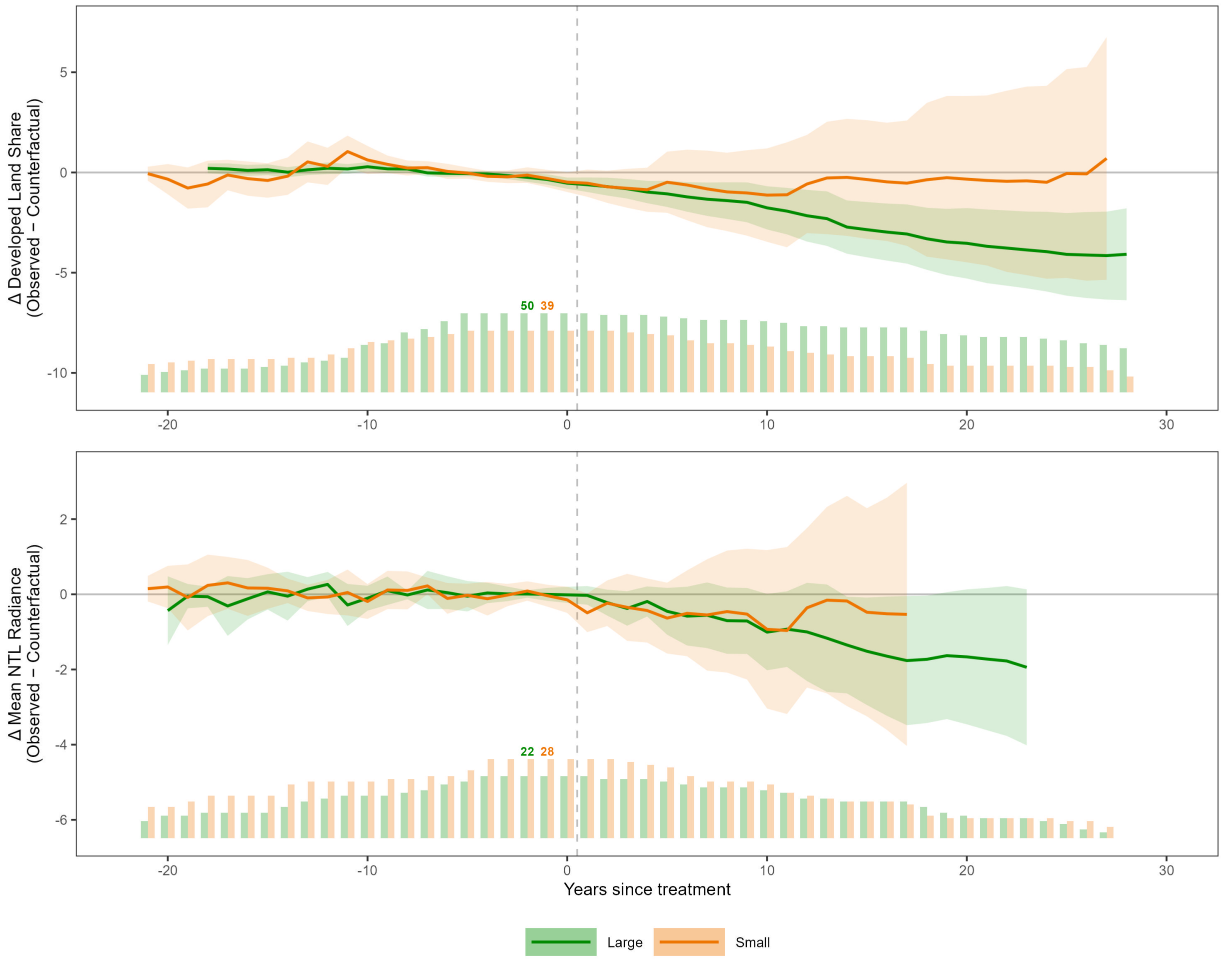

Figure G2: Dynamic treatment effects by AIAN population (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G2: Heterogeneous Effects of SGC by AIAN Population — Full Diagnostics

	Large	Small
Panel A: Developed Land Share		
ATT	-2.849** (0.540)	0.823 (1.250)
$\text{ATT}_{t=1}$	-0.281** (0.107)	0.313 (0.308)
$\text{ATT}_{t=6}$	-1.085** (0.357)	1.041 (1.040)
$\text{ATT}_{t=12}$	-2.857** (0.689)	-0.086 (1.388)
$\text{ATT}_{t=18}$	-4.167** (0.741)	0.507 (2.083)
Latent Factors (r)	3	3
F-test p-value	0.240	0.240
TOST Equiv. p-value	0.055	0.055
Observations	12714	12714
Years	39	39
Treated Reservations	48	42
Panel B: NTL Radiance		
ATT	-1.005** (0.394)	0.245 (0.776)
$\text{ATT}_{t=1}$	-0.084 (0.111)	0.013 (0.419)
$\text{ATT}_{t=6}$	-0.414 (0.269)	0.521 (0.946)
$\text{ATT}_{t=12}$	-1.124* (0.542)	-0.730 (0.866)
$\text{ATT}_{t=18}$	-1.767* (0.711)	-0.713 (1.734)
Latent Factors (r)	3	3
F-test p-value	0.553	0.553
TOST Equiv. p-value	0.321	0.321
Observations	8992	8992
Years	32	32
Treated Reservations	22	29

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\, p < 0.05$; $^{**}\, p < 0.01$.

G.3 Tribal Enrollment

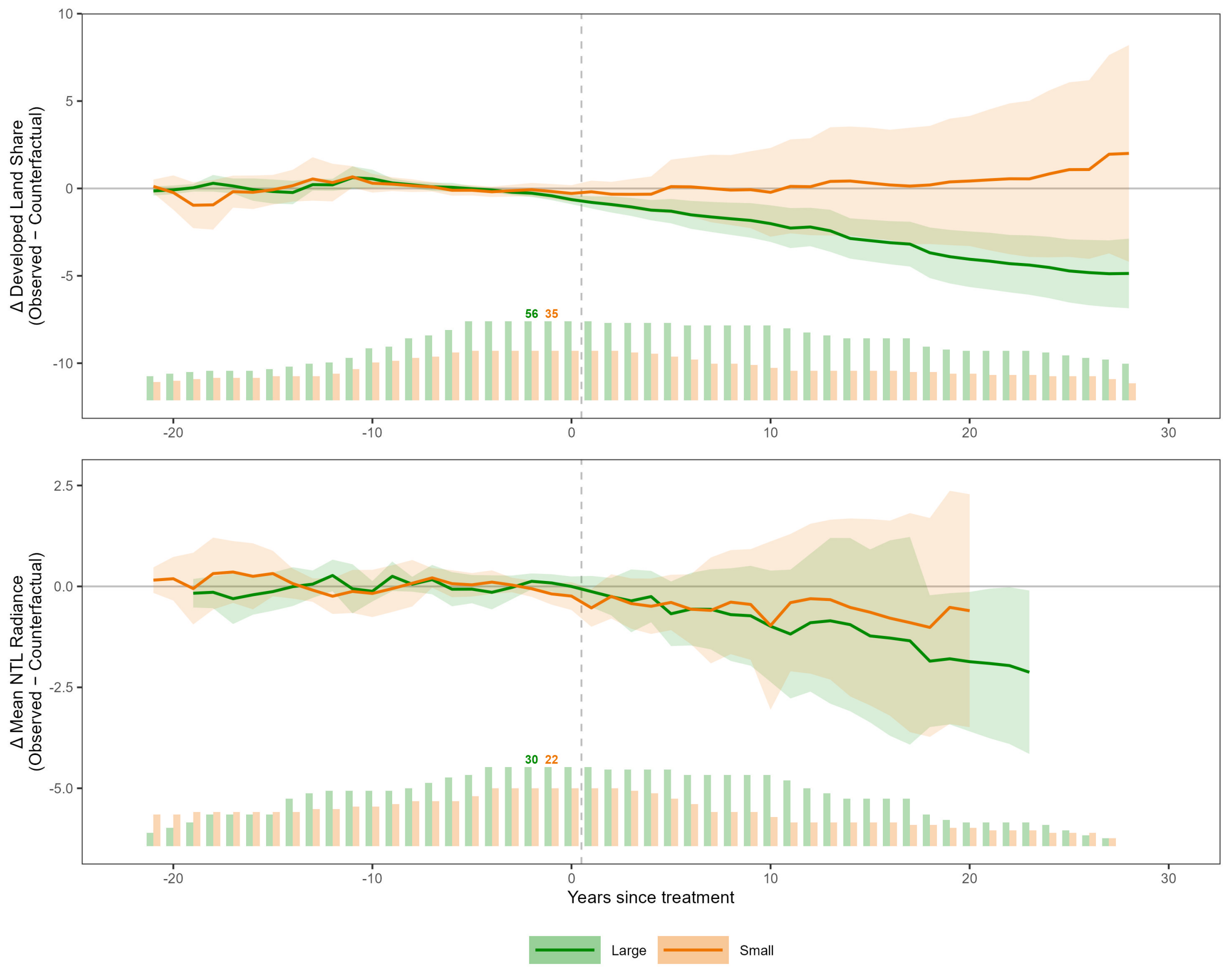

Figure G3: Dynamic treatment effects by tribal enrollment (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G3: Heterogeneous Effects of SGC by Enrollment — Full Diagnostics

	Large	Small
Panel A: Developed Land Share		
ATT	-2.760** (0.552)	0.455 (1.375)
$\text{ATT}_{t=1}$	-0.797** (0.177)	-0.190 (0.325)
$\text{ATT}_{t=6}$	-1.510** (0.408)	0.091 (0.865)
$\text{ATT}_{t=12}$	-2.206** (0.561)	0.100 (1.415)
$\text{ATT}_{t=18}$	-3.681** (0.738)	0.198 (1.725)
Latent Factors (r)	1	1
F-test p-value	0.001	0.001
TOST Equiv. p-value	0.968	0.968
Observations	12714	12714
Years	39	39
Treated Reservations	56	35
Panel B: NTL Radiance		
ATT	-0.916 (0.558)	-0.391 (0.766)
$\text{ATT}_{t=1}$	-0.128 (0.198)	-0.532* (0.238)
$\text{ATT}_{t=6}$	-0.564 (0.459)	-0.565 (0.441)
$\text{ATT}_{t=12}$	-0.899 (0.871)	-0.305 (0.947)
$\text{ATT}_{t=18}$	-1.851* (0.833)	-1.016 (1.383)
Latent Factors (r)	1	1
F-test p-value	0.724	0.724
TOST Equiv. p-value	0.298	0.298
Observations	8960	8960
Years	32	32
Treated Reservations	30	22

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_\varepsilon$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

G.4 Distance to City

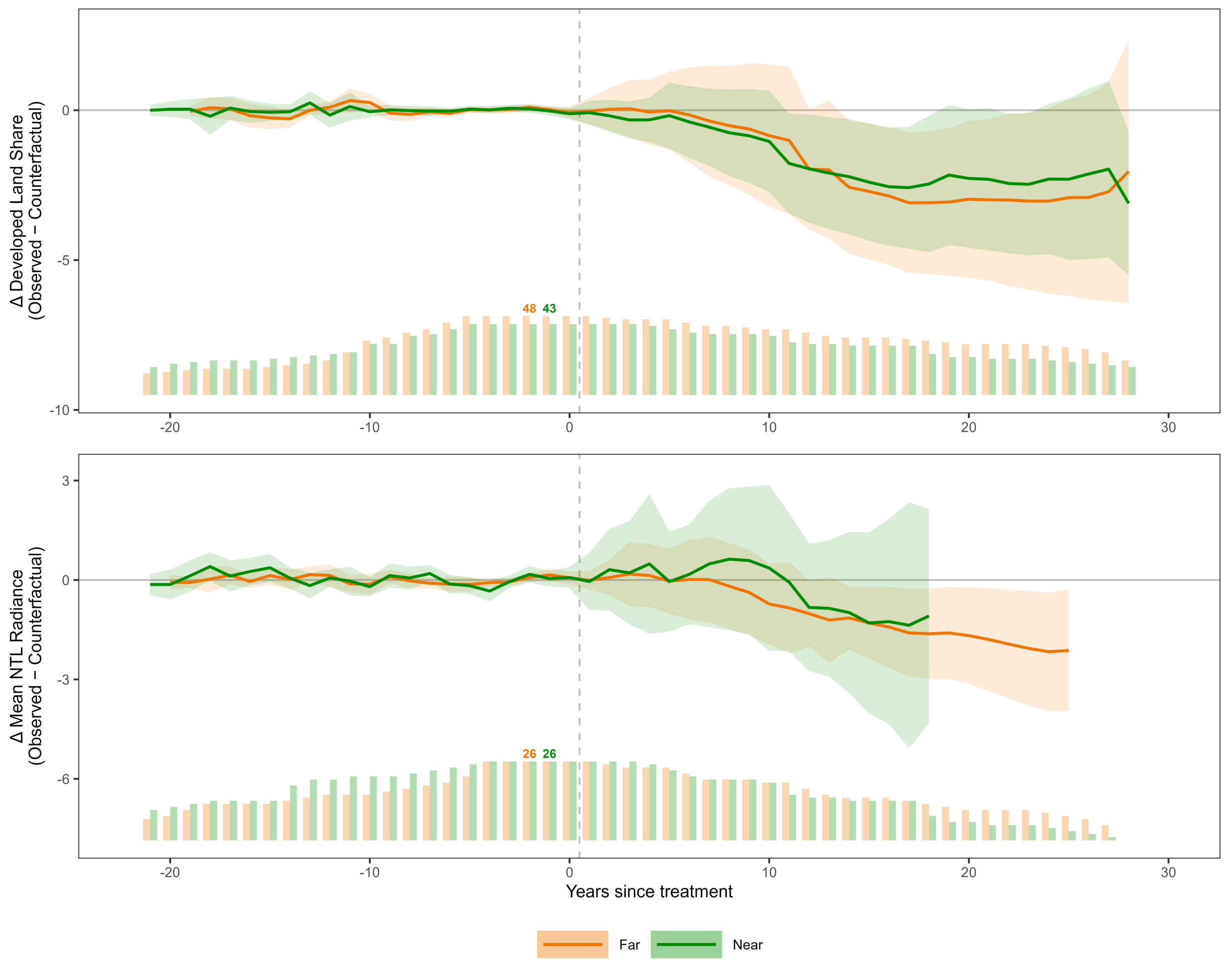

Figure G4: Dynamic treatment effects by distance to city with at least 100,000 residents (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G4: Heterogeneous Effects of SGC by Distance to City — Full Diagnostics

	Far	Near
Panel A: Developed Land Share		
ATT	-1.599 (1.023)	-1.465 (0.782)
$\text{ATT}_{t=1}$	-0.033 (0.223)	-0.084 (0.204)
$\text{ATT}_{t=6}$	-0.159 (0.807)	-0.394 (0.612)
$\text{ATT}_{t=12}$	-1.967 (1.026)	-1.954* (0.921)
$\text{ATT}_{t=18}$	-3.086* (1.217)	-2.461* (1.159)
Latent Factors (r)	4	4
F-test p-value	0.110	0.110
TOST Equiv. p-value	0.048	0.048
Observations	12714	12714
Years	39	39
Treated Reservations	48	43
Panel B: NTL Radiance		
ATT	-0.751 (0.438)	0.045 (0.802)
$\text{ATT}_{t=1}$	-0.008 (0.147)	-0.046 (0.444)
$\text{ATT}_{t=6}$	0.015 (0.608)	0.170 (0.769)
$\text{ATT}_{t=12}$	-1.016* (0.505)	-0.828 (0.980)
$\text{ATT}_{t=18}$	-1.622* (0.698)	-1.087 (1.645)
Latent Factors (r)	3	3
F-test p-value	0.489	0.489
TOST Equiv. p-value	0.311	0.311
Observations	8960	8960
Years	32	32
Treated Reservations	26	26

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

G.5 Executive Selection

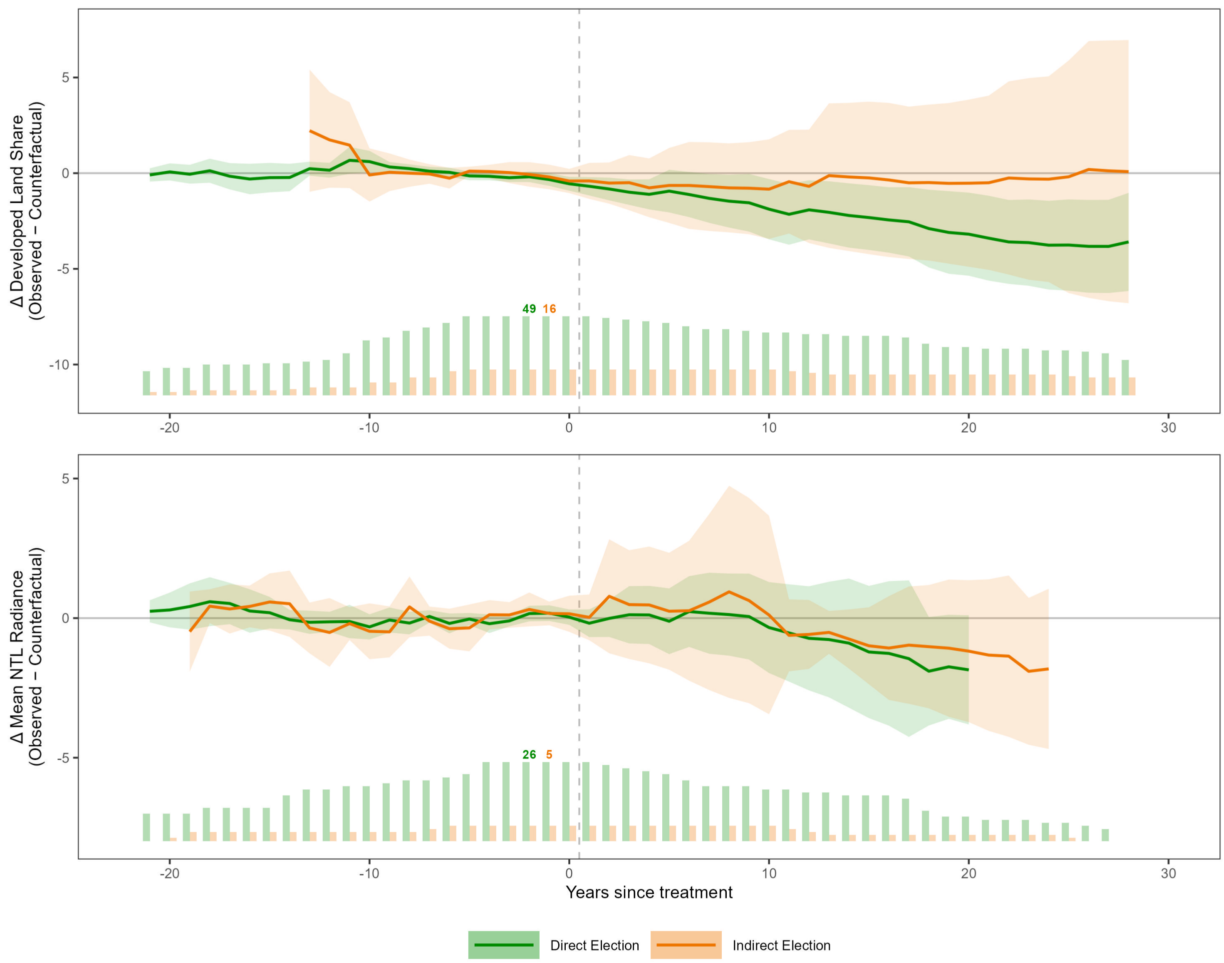

Figure G5: Dynamic treatment effects by executive selection method. Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G5: Heterogeneous Effects of SGC by Executive Selection — Full Diagnostics

	Direct Election	Indirect Election
Panel A: Developed Land Share		
ATT	-2.180** (0.792)	-0.421 (1.811)
$\text{ATT}_{t=1}$	-0.684** (0.244)	-0.405 (0.480)
$\text{ATT}_{t=6}$	-1.116 (0.602)	-0.642 (1.159)
$\text{ATT}_{t=12}$	-1.916* (0.787)	-0.688 (1.512)
$\text{ATT}_{t=18}$	-2.896** (1.036)	-0.490 (2.078)
Latent Factors (r)	1	1
F-test p-value	0.029	0.029
TOST Equiv. p-value	0.939	0.939
Observations	8775	8775
Years	39	39
Treated Reservations	49	16
Panel B: NTL Radiance		
ATT	-0.592 (0.612)	-0.152 (1.107)
$\text{ATT}_{t=1}$	-0.182 (0.253)	0.028 (0.418)
$\text{ATT}_{t=6}$	0.236 (0.647)	0.265 (1.278)
$\text{ATT}_{t=12}$	-0.724 (0.951)	-0.585 (0.630)
$\text{ATT}_{t=18}$	-1.901 (0.991)	-1.023 (1.126)
Latent Factors (r)	2	2
F-test p-value	0.936	0.936
TOST Equiv. p-value	0.655	0.655
Observations	5952	5952
Years	32	32
Treated Reservations	26	5

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

G.6 Residency Voting Requirement

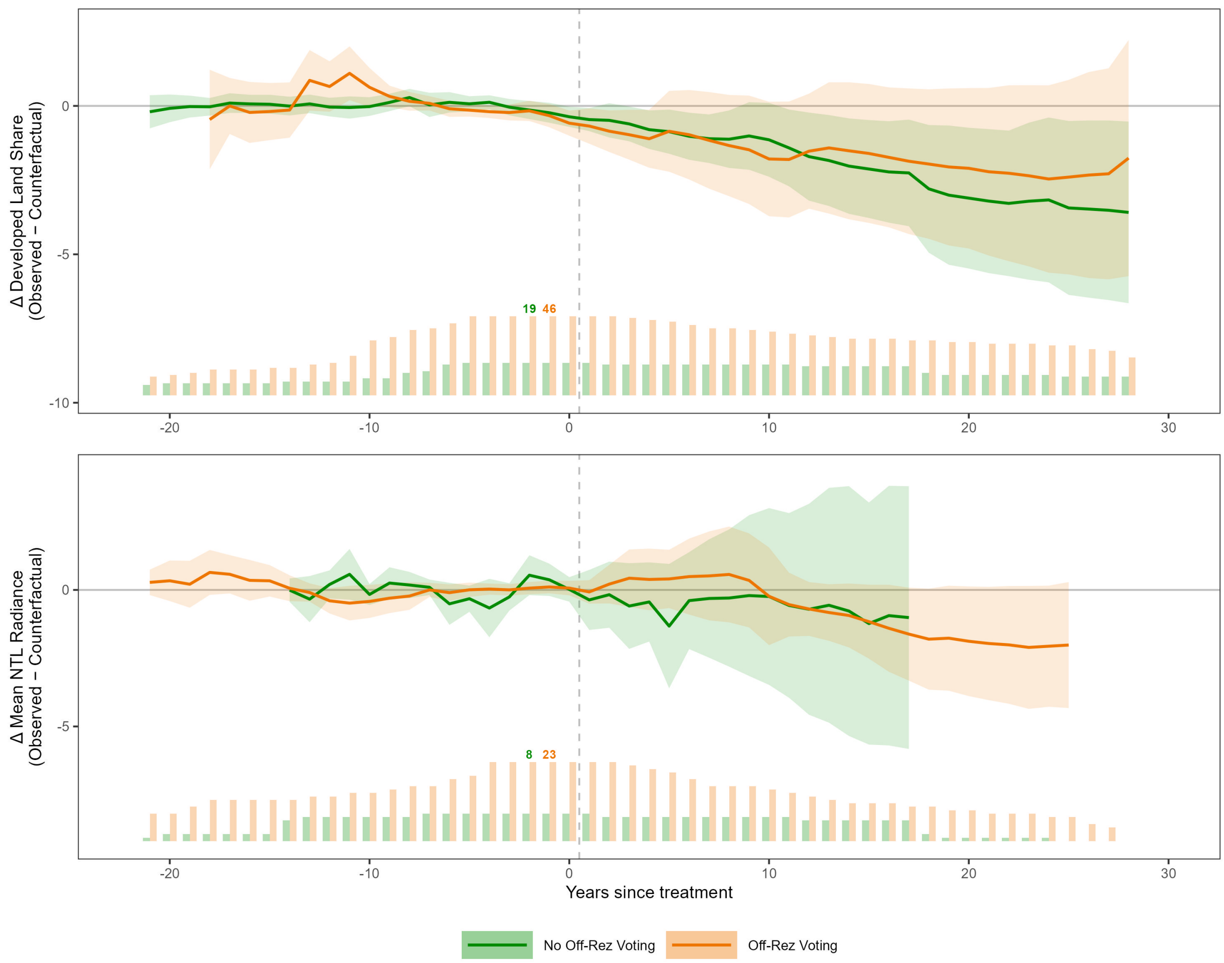

Figure G6: Dynamic treatment effects by residency voting requirement. Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G6: Heterogeneous Effects of SGC by Residency Voting Requirement — Full Diagnostics

	No Off-Rez Voting	Off-Rez Voting
Panel A: Developed Land Share		
ATT	-2.046* (0.813)	-1.516 (1.061)
$\text{ATT}_{t=1}$	-0.461* (0.198)	-0.679* (0.303)
$\text{ATT}_{t=6}$	-1.023* (0.404)	-0.972 (0.768)
$\text{ATT}_{t=12}$	-1.709* (0.757)	-1.525 (0.988)
$\text{ATT}_{t=18}$	-2.795* (1.097)	-1.959 (1.284)
Latent Factors (r)	1	1
F-test p-value	0.029	0.029
TOST Equiv. p-value	0.939	0.939
Observations	8775	8775
Years	39	39
Treated Reservations	19	46
Panel B: NTL Radiance		
ATT	-0.591 (1.167)	-0.488 (0.533)
$\text{ATT}_{t=1}$	-0.365 (0.561)	-0.073 (0.219)
$\text{ATT}_{t=6}$	-0.392 (0.905)	0.488 (0.709)
$\text{ATT}_{t=12}$	-0.709 (1.971)	-0.698 (0.503)
$\text{ATT}_{t=18}$	-1.514 (1.490)	-1.803 (0.944)
Latent Factors (r)	2	2
F-test p-value	0.936	0.936
TOST Equiv. p-value	0.655	0.655
Observations	5952	5952
Years	32	32
Treated Reservations	8	23

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

G.7 Direct Democracy

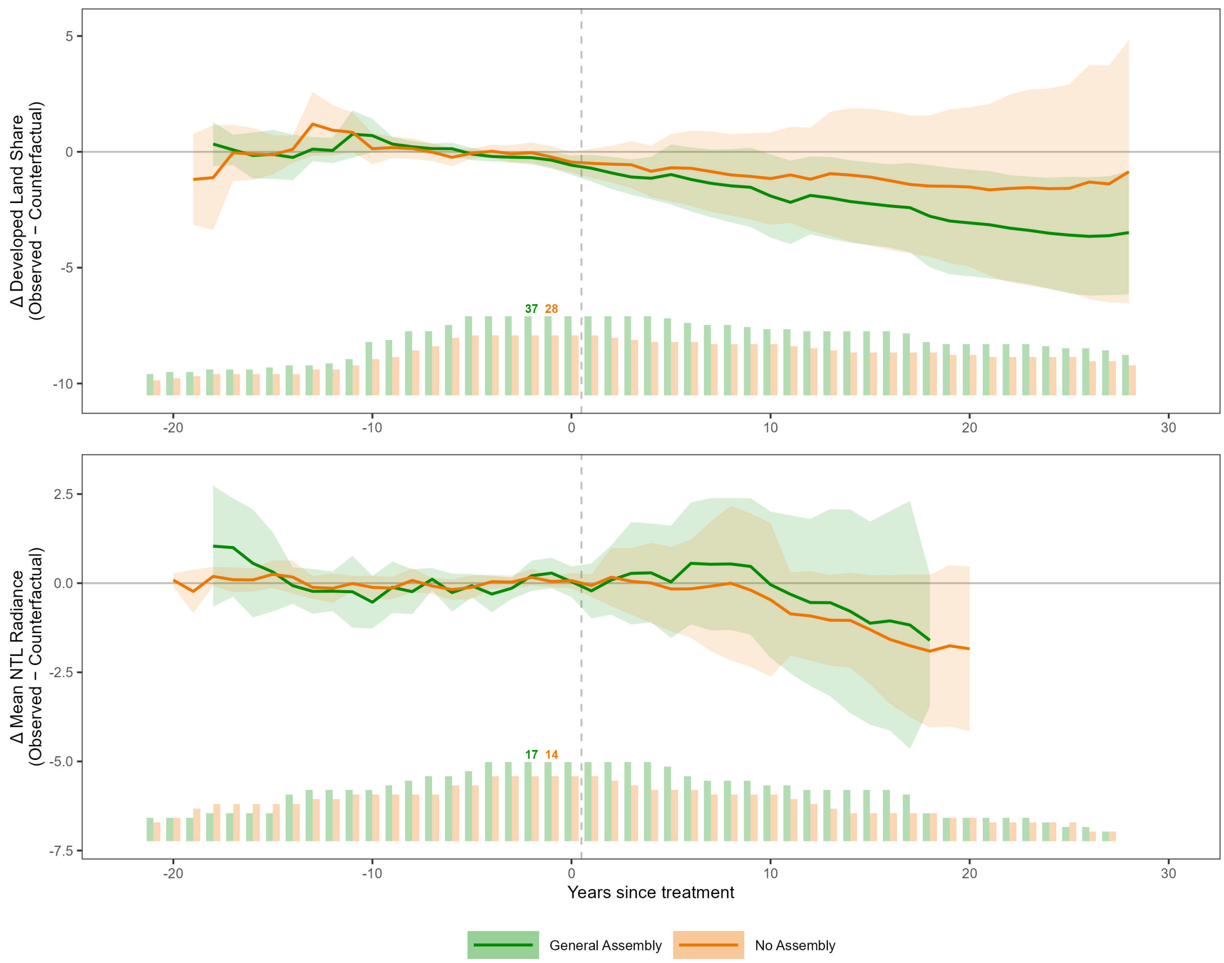

Figure G7: Dynamic treatment effects by direct democracy (general assembly vs. representative government). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G7: Heterogeneous Effects of SGC by Direct Democracy — Full Diagnostics

	General Assembly	No Assembly
Panel A: Developed Land Share		
ATT	-2.157** (0.853)	-1.034 (1.352)
$\text{ATT}_{t=1}$	-0.707* (0.292)	-0.495 (0.320)
$\text{ATT}_{t=6}$	-1.191 (0.706)	-0.710 (0.827)
$\text{ATT}_{t=12}$	-1.881* (0.856)	-1.183 (1.136)
$\text{ATT}_{t=18}$	-2.779* (1.124)	-1.478 (1.557)
Latent Factors (r)	1	1
F-test p-value	0.029	0.029
TOST Equiv. p-value	0.939	0.939
Observations	8775	8775
Years	39	39
Treated Reservations	37	28
Panel B: NTL Radiance		
ATT	-0.366 (0.718)	-0.715 (0.660)
$\text{ATT}_{t=1}$	-0.216 (0.391)	-0.067 (0.160)
$\text{ATT}_{t=6}$	0.557 (0.871)	-0.159 (0.701)
$\text{ATT}_{t=12}$	-0.544 (1.196)	-0.918 (0.639)
$\text{ATT}_{t=18}$	-1.603 (0.945)	-1.907 (1.092)
Latent Factors (r)	2	2
F-test p-value	0.936	0.936
TOST Equiv. p-value	0.655	0.655
Observations	5952	5952
Years	32	32
Treated Reservations	17	14

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. $^{*}\ p < 0.05$; $^{**}\ p < 0.01$.

G.8 Ethnic Fractionalization

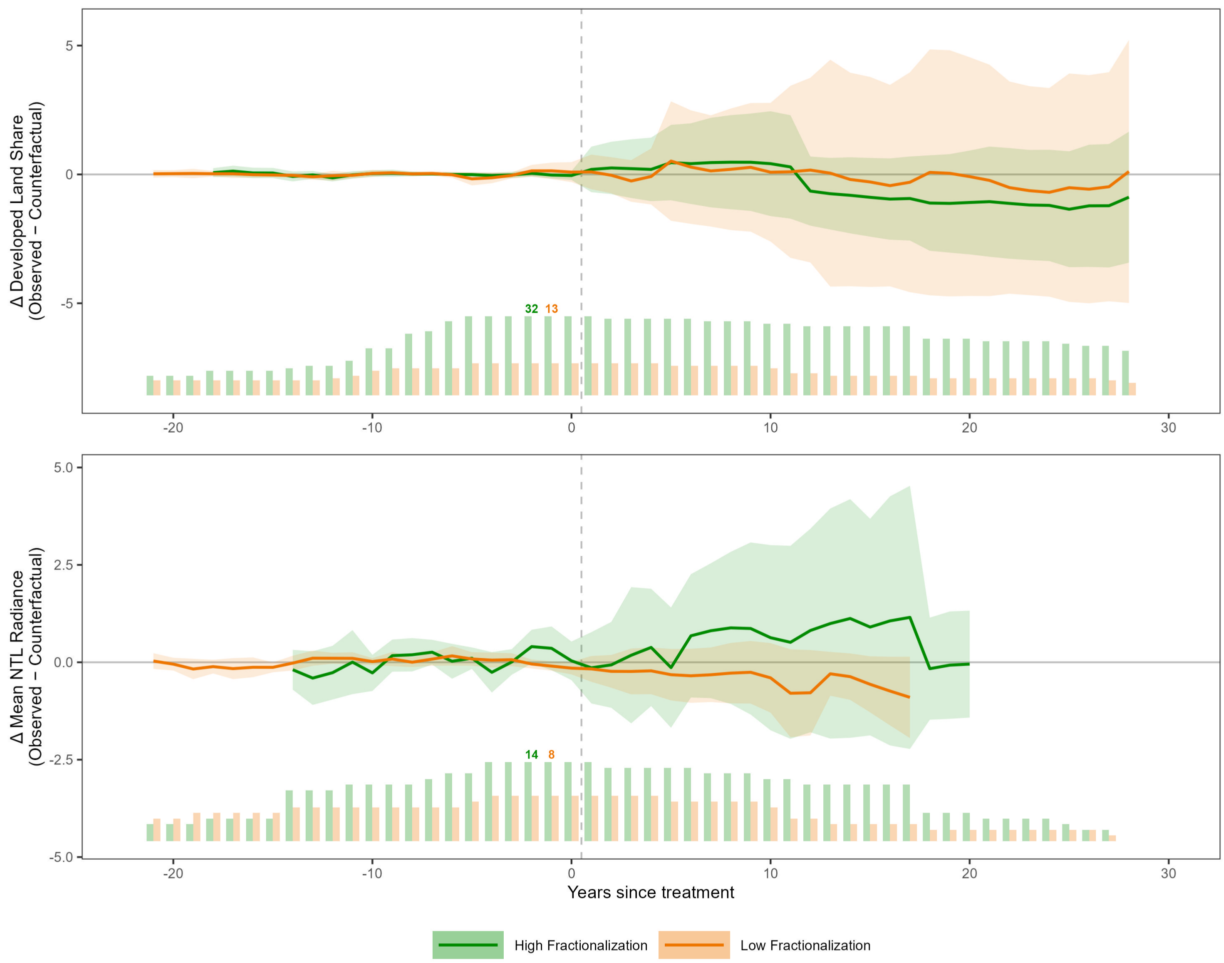

Figure G8: Dynamic treatment effects by ethnic fractionalization (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G8: Heterogeneous Effects of SGC by Ethnic Factionalization — Full Diagnostics

	High Factionalization	Low Factionalization
Panel A: Developed Land Share		
ATT	-0.428 (0.780)	0.042 (1.507)
$\text{ATT}_{t=1}$	0.194 (0.451)	0.097 (0.342)
$\text{ATT}_{t=6}$	0.418 (0.798)	0.284 (1.124)
$\text{ATT}_{t=12}$	-0.647 (0.684)	0.168 (1.829)
$\text{ATT}_{t=18}$	-1.110 (0.945)	0.080 (2.434)
Latent Factors (r)	3	3
F-test p-value	0.781	0.781
TOST Equiv. p-value	0.004	0.004
Observations	7098	7098
Years	39	39
Treated Reservations	32	13
Panel B: NTL Radiance		
ATT	0.468 (0.693)	-0.524 (0.322)
$\text{ATT}_{t=1}$	-0.142 (0.467)	-0.170 (0.169)
$\text{ATT}_{t=6}$	0.680 (0.807)	-0.347 (0.354)
$\text{ATT}_{t=12}$	0.816 (1.332)	-0.783 (0.559)
$\text{ATT}_{t=18}$	-0.164 (0.668)	-1.283* (0.612)
Latent Factors (r)	1	1
F-test p-value	0.804	0.804
TOST Equiv. p-value	0.473	0.473
Observations	4992	4992
Years	32	32
Treated Reservations	14	8

Note: Coefficients represent the post-treatment difference between observed outcomes and reservation-specific counterfactual estimates, estimated separately for each subgroup using the IFEct estimator with gaming control. The F-test examines whether all pre-treatment ATTs are jointly zero over the 10 years prior to compact adoption. The TOST equivalence test examines whether pre-treatment ATTs fall within $\pm 0.25\hat{\sigma}_{\varepsilon}$ of zero. Pre-treatment diagnostics are model-level (not group-specific). Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.