

Power to Prosper: Self-Governance and Economic Development

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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 reduced economic development 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

Local governance is often exalted as a positive force for supporting democracy and protecting individual and collective rights. Tocqueville believed municipal power was the backbone of liberty and freedom in the United States ([Tocqueville 1835](#)). Vincent Ostrom and colleagues argued against "Gargantua," a single, dominant government, and in support of the effectiveness of polycentric systems with many inter-dependent, self-governing municipalities ([Ostrom, Tiebout and Warren 1961](#)). Later, Elinor Ostrom championed the capacity of local communities to govern themselves collectively without top-down intervention by a central government ([Ostrom 1990](#)).

Beyond the instrumentalism of local self-governance, there is also a strong normative appeal that communities (particularly vulnerable minority communities) deserve the right to determine how their societies are run ([Weinstock 2001](#)). Internationally, this normative argument is the backbone for agreements like the UN Declaration on the Rights of Indigenous Peoples ([United Nations General Assembly 2007](#)). Reforms implementing decentralization, federalism, and local self-governance across many countries in the later 20th Century emphasizes the belief in the value of self-governance ([Manor 1999](#)).

Yet, as the study of local politics in America has shown, local self-governance comes with drawbacks. Local governments are constrained in their policymaking in many ways by higher levels of government, but do have considerable power over important features of governance ([Gerber and Hopkins 2011](#)). And while this autonomy may sound appealing, local communities left to their own devices often create highly inequitable distributions of goods, often along lines of race ([Trounstine 2016](#); [Trounstine 2020a](#); [Hankinson and Magazinnik 2023](#)). A major source of this inequity is the ability of a subset of local residents to capture participatory political processes, often bottle-necking policymaking in a way that would be infeasible at higher levels of government ([Einstein, Glick and Palmer 2019](#); [Anzia 2022](#)). And, in contrast to Tocqueville's romantic image of the citizenry actively engaged in the goings-on of the local township, we know that today's average citizen knows very little about the politics in the local government ([Binder et al. 2016](#)) and participation in local elections is abysmally low ([Hajnal 2009](#)), raising the question of whether

local officials are held as accountable as their federal counterparts are.

This leaves local self-governance at a cross-roads. Theoretically, it has normative and instrumental appeal for better governance. However, when decision-making is scaled down, local self-governance becomes susceptible to a number of flaws that can reduce the efficiency and equity of outcomes. These flaws raise the question, does increasing local self-governance produce better outcomes for local communities compared to more centralized forms of decision-making?

In this paper, I address this question by examining an important modern transfer of political power in the US from the federal government to local communities: the adoption of self-governance compacts in American Indian tribal governments beginning in the early 1990s. Approximately half of all tribal governments use these compacts to take over control of many local programs that were previously managed by the federal government, including vital policies like policing, natural resource management, and job training. In this era, reservation economies have seen significant growth, but there is scant evidence on whether self-governance drove this growth ([Kalt 2022](#)).

I conceptualize the economic effects of local self-governance as the net outcome of two opposing forces. Local autonomy mitigates agency loss by improving electoral control and ideological congruence between voters and policymakers. However, it may lower governance capacity due to reductions in policy experience, fiscal resources, and skilled labor access. This model implies that the net effect of local self-governance depends on which of these countervailing forces is greater. Additionally, it implies that the effect should be more positive as local governance capacity increases.

American Indian tribal governments are some of the most poorly understood and least studied governments in the American federalist system ([Ferguson 2016](#)). This knowledge deficit partially stems from low data availability, leading to studies of American Indian politics often lacking the ability to make many cross-tribe comparisons or examine outcomes over a long period of time. In this paper, I avoid these constraints and construct a panel dataset measuring economic development in almost all federally recognized tribal communities in the contiguous United States for over three decades. I do so by utilizing remote sensing data on land usage and nightlight density to create

yearly estimates of economic development for native reservations. The end result is the largest public dataset ever constructed on American Indian reservation economies, covering 328 tribal areas and 39 years of economic development for over 12,000 reservation-year observations.

After establishing the validity of these indicators, I apply modern techniques in difference-in-differences analysis to estimate the effect of increased self-governance, focusing on counterfactual-based time series methods proposed by [Liu, Wang and Xu \(2024\)](#). I find that entering into a self-governance compact resulted in *slower* economic development over time, although the effect fades after significant time under a compact. Compared to the positive impact of casino gaming, self-governance had a negative effect of roughly equal magnitude (86–99% as large in the most believable estimates). Following this result, I test for a governance capacity mechanism by constructing a number of plausible proxies. This includes the collection and coding of hundreds of tribal constitutions to code relevant institutional features like voting restrictions and executive electoral systems. I find tentative evidence pointing towards to expected positive relationship between governance capacity and economic growth under expanded self-governance.

These findings contribute to a broader understanding of local politics in the United States. Because tribal governments faces many of the same policy issues (e.g. land use, business development, public safety) as municipal governments, but have a much 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 ([Trounstone 2020b](#)). This work highlights the more often neglected study of local governance capacity, which has largely been left to public administration scholars (See [Hall 2008](#); [Wang et al. 2012](#), and [Terman and Feiock 2015](#) for contemporary examples). 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](#)).

This paper also seeks to incorporate indigenous politics into the literature on 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 and lobbying local, state, and federal governments (Witmer and Boehmke 2007; Evans 2011a). Yet rarely do these works draw on ideas developed in local politics literature, nor are their findings incorporated into future local politics work. This paper work attempts to bring these two literatures into a closer dialogue for the future benefit of both.

Tribal governments do, however, rest in a unique position in the United States and this paper speaks to the unique qualities of US indigenous politics. Most clearly, this paper contributes to work examining 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 also adds to burgeoning literature examining the effects of tribal institutions, particularly tribal constitutions (Cornell and Kalt 1990; Evans 2011b; 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). Overall, the findings of this paper emphasize that reservations bear a sovereignty-development trade-off. (Anderson and Parker 2008; Wellhausen et al. 2017).

2 Self-Governance and Tribes

2.1 Tribal 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 lead 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. This power ranged across a variety of policy areas, including health 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 being compared to autocrats or dictators (Castile 1998). 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.

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 play 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 they can run the program.¹ 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. Any program that is not deemed an "inherent federal function" can be included in a contract and the BIA is required to allow the

¹Additional 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.

program to be contracted if requested by a tribe. As of 2024, 92% of tribes were 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 a 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 they didn't need as much funding for housing and transfer a portion of the housing funds to their 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, they could have all BIA programs included under one of the three categories. Today, approximately 50% of tribes have a compact. In the contiguous United States, only 29.5% of recognized tribes have a SGC. Uptake is higher in Alaska where 84.6% of recognized tribes have a SGC. What programs are being contracted/compact versus left for direct service by each tribe, however, is not public knowledge.

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 governance quality.

2.2 Benefits of Self-Governance

Classic theories of local self-governance and decentralization focus on heterogeneity in preferences for public goods across different communities to explain the value of local governance. 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 (Oates 1972). Tiebout (1956) emphasizes the role of competition between local governments to attract residents and citizen self-sorting to maximize the efficiency gains of local governance. However, even without movement, local governance can still improve the efficiency of public goods provision if preferences broadly vary from one location to another (Oates 1999). Oates (1999) also 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 political infeasible or unsustainable.

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. Only through their power in the national electorate can a population influence their local officials. Conversely, in a decentralized system, these officials are directly accountable to the local population, increasing the accountability for local officials and decreasing the agency loss of the local population (Faguet 2014). Enhancing the accountability of local decision-makers to the service population increases communities' ability to reward or sanction leadership, thereby promoting more effective governance. Additionally, when

local populations have control over who governs them, they have the opportunity to select better leaders whose personal preferences are more aligned with 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. Descriptively, a number of studies have found positive returns to indigenous self-governance. 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. This work, however, largely focuses on native communities in developing countries where central government power is often weaker. In contrast, American Indian communities seek self-governance in the wealthiest country in the world and compete with not only a high capacity federal government, but also relatively strong state and local governments. In the US context, it has been noted that the era of self-governance has coincided with significant reservation economic growth (Kalt 2022). However, evidence pointing to self-governance as the driver of this growth is scant, limited to a few early case studies (Krepps and Caves 1994, Harris, Blomstrom and Nakamura 1995).

There is good reason theoretically that tribal governments should expect positive returns to self-governance. First, we should expect preferences around the mixture of public goods will 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, including increased flooding and declines in salmon populations, which play vital cultural and economic roles in their communities. Other tribes, such as those in Arizona, may face less pressing environmental concerns and instead struggle maintaining public safety and order as they deal with organized crime and drug smuggling crossing through their reservation from Mexico. Presumably, the Washington tribes would prefer to increase supply of climate change resiliency

programs while the Arizona tribe would prefer to increase their supply of law enforcement and public safety policy.

Such nuances, however, are not available to tribal communities when reservation policies are set at the national level by Congress and executed through the BIA. Most reservation populations are simply too small and politically inconsequential to justify policy differentiation at the national level. Without granting significant discretionary power to the BIA, Congress would struggle to properly identify what mix of public goods, say, the Snoqualmie Indian Tribe in Washington prefers compared to Yerington Paiute Tribe in Arizona compared to the other 550+ tribes that are tasked to serve. Likewise, even if they had that discretionary power, the BIA would likely lack the funding and tribe-specific knowledge to understand what each native community preferred. The heterogeneity in preferences across each tribe suggests that there may be efficiency gains to decentralizing federal Indian programs and passing their administration.

The BIA is also unlikely to be responsive to the judgment of a reservation. While the BIA does have a number of cooperative channels to communicate with tribal governments, the BIA is ultimately responsible to Congress and the Executive. The work of the BIA is restricted by how Congress appropriates funding and defines its programs, as well as by the directives of the President in how to carry out these programs. Tribal governments who are dissatisfied with how a certain program is run by the federal have to exert enough political power in DC to move the policy preferences of legislators or the President. Because of their small size, tribal electoral power is limited to a small set of highly competitive races ([McCool 1985](#)). And while many tribes have embraced lobbying to represent their interests, their influence is dwarfed in size by other organized interests ([Carlson 2023](#)).

Under an SGC, the chain of accountability is shortened. Now instead of going through the federal government, the tribal government is responsible for the programs. Because most tribal governments are directly elected by the tribe, tribe members now have the opportunity to make their preferences heard when 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 also become naturally more 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 of 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 governed under a SGC will have higher economic growth than reservations not governed under a SGC.

2.3 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. For example, scholars of decentralization worry that decentralized systems create incentives for poor fiscal behavior by local governments. For example, Rodden (2006) argues that cities may recklessly overspend knowing that they will be bailed out by higher governments. Others worry that competition among local governments will create a “race to the bottom” effect for taxes that fund public goods in order to attract private investment (Cai and Treisman 2004).

Perhaps the most notable concern is that increasing accountability closer to the local population will actually make governance worse. For some, the concern is that local governments simply lack the resources and general capacity to supply public goods as efficiently as a centralized government (Prud’Homme 1995, Carter 2022). Thus the potential gains from a more efficient allocation of public goods are wiped away by the inability of the local governments to actually produce the goods. Others worry that local governments may be more susceptible to corruption (Bardhan and Mookherjee 2006). In areas where political knowledge and participation are low, or where the population faces significant economic disadvantage, there is a greater risk of elite capture of institutions.

These fears mirror many of the issues with local government that Americanist scholars have noted. Instead of worrying about illicit bribery and corruption reducing the effectiveness of local public policy in favor of a select few, however, American local politics points to the ability of a select group of residents to use legal participatory processes to capture (or at least delay) local decision-making ([Einstein, Glick and Palmer 2019](#), [Anzia 2022](#)). Local elections, a form of participation with a lower barrier to entry, have lower turnout rates compared to federal elections ([Hajnal 2009](#)). This lack of participation leads once again to the over-representation of white, wealthy homeowners interests ([Oliver and Ha 2007](#), [Schaffner, Rhodes and La Raja 2020](#)). In terms of knowledge, at best, voters have about equal levels of knowledge between national and local politics, but other studies point to local knowledge being even lower ([Shaker 2012](#), [Binder et al. 2016](#)). Americans don't even seem to understand what their local officials are responsible for ([de Benedictis-Kessner 2018](#)). Newspapers, one of the key distributors of local political information ([Mondak 1995](#)), have faced significant cutbacks in recent years ([Peterson 2021](#)). The newspaper's successors, television and internet news, heavily emphasize national news ([Hopkins 2022](#)). And not only is information on local politics harder to come by, but the heuristics that many voters rely on in national elections are also not available at the local level, leaving low knowledge voters to make decisions on particularly poor candidate qualities ([Bernhard and Freeder 2020](#)). In terms of aligning public policy with local preferences, there is evidence that both state and local governments set policy away from the median preference — state governments with a conservative bias, and local governments with a liberal bias ([Simonovits and Payson 2023](#)). When characterized this way, it's almost hard to believe that local governance in America could ever result in a more responsive, efficient government.

Given the mixed arguments surrounding self-governance, it is unsurprising that empirical studies have found mixed results ([Martinez-Vazquez and McNab 2003](#), [Treisman 2007](#), [Faguet and Sánchez 2008](#)). One argument to clarify these results is that not all decentralization is the same. [Weingast \(2009, 2014\)](#) argues that many of the issues and solutions to decentralization lay in how the federalist system is designed. Systems where local authority is protected, but budgets have hard constraints, and where politicians cooperate electorally and respect the powers of different levels of

government can preserve the economic benefits of decentralization while avoiding the downsides and preventing a collapse of the system.

Additionally, the principal-agent justification for self-governance assumes that local populations will be able to hold their local officials accountable. This may not always be the case when the local government lacks effective democratic institutions or when the population lacks the civic skills and institutions to wield their influence over local governance (Faguet 2014). Additionally, weak local governments with inadequate resources or no governance experience will struggle to provide public goods (Foa 2022). This observation fits in with work on bureaucracies which find that public goods providers need time to learn and develop expertise (Gailmard and Patty 2012).

These concerns regarding the costs to self-governance are also easy to see in the tribal context. In the grand scheme of American politics, modern tribal governments are young. Tribal governments were only formally recognized by the federal government starting in the 1930s, with many not being acknowledged until much later. And for much of their modern history, tribal governments have lacked significant power or resources until the expansions of tribal self-governance previously outlined and the adoption of tribal gaming. One piece of evidence on tribal inexperience and resource constraints is that tribes frequently try to expand services after contracting or compacting BIA programs, only to realize that more funding from the BIA is not possible and they are forced to scale back their plans (Henson 2008). On top of this inexperience, many tribal governments adopted constitutions that did not resemble their traditional forms of governance and existing social structures, leading to a mis-match between *de jure* rules and *de facto* power (Cornell and Kalt 1992, Lemont 2009).

Additionally, expanding self-governance might lead to outside investors being less likely invest on the reservation out of uncertainty over the tribe's ability to govern (Anderson and Parker 2008, Wellhausen et al. 2017). Evidence from past legislation controlling tribal governments shows that tribes who accepted greater federal oversight had moderate long-run growth while tribes who eschewed oversight had much greater variability in their development, both positive and negative (Frye and Parker 2016). Thus, expanding self-governance might increase downside economic risk

on reservations.

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

H1b: Reservations governed under a SGC will have lower economic growth than reservations not governed under a SGC.

Taken together, these two theoretic perspectives 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 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 entering an SGC should become more positive.

3 Data and Methods

3.1 Unit of Analysis

This paper uses American Indian federal reservations and other tribal land areas as the unit of analysis.² This excludes state-recognized reservations and Alaskan Native communities. Reservation boundaries occasionally change which present 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.³ Most tribal governments control one reserva-

²For brevity, I simply refer to these areas as reservations for the rest of the paper. Other types of tribal land, like Oklahoma Statistical Tribal Areas (OTSAs) and Tribal Designated Statistical Areas (TDSAs), make up only a small percentage of the included units.

³Reservation 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.

tion, but there are a handful of tribes with more than one reservation. In total, my dataset includes 328 reservations belonging to 305 tribes.

3.2 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.⁴ 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 a SGC as of 2024. This translate 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 a 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 highlights a key limitation of the data: the specific programs covered by each SGC are not public. As previously discussed, tribal governments can choose to mix-and-match direct service, self-determination contracting, and self-governance compacting. This means that treatment intensity will have unobserved variation. A tribe with a SGC that covers every compactable BIA program and a tribe with a SGC that covers just one minor 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

⁴An 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.

North 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.

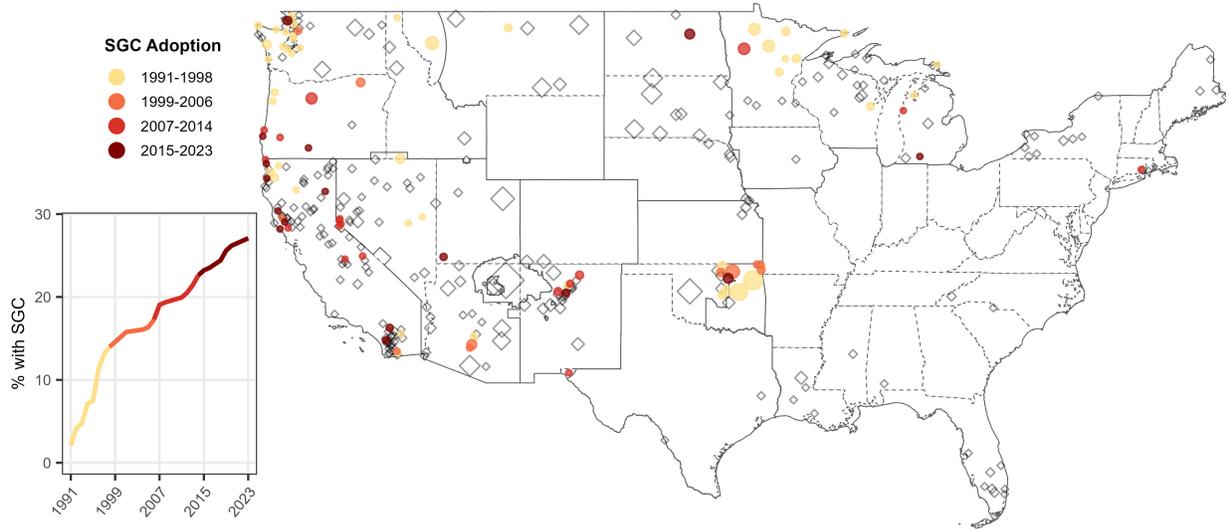


Figure 1: Map depicting SGC adoption timing by reservation. Untreated reservations are marked with diamonds, treated reservations are marked as circles and colored by timing of SGC adoption. Size of all points based on relative geographic size of the reservation. Line graph in the bottom-left corner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

3.3 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. Nightlight (NTL) intensity has become a particularly popular measure of economic development in a wide range of contexts (e.g. [Min 2015](#), [Kroth, Larcinese and Wehner 2016](#), [Zhou and Shaver 2021](#)). 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](#)). 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 time periods 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.⁵ 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 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 use 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

⁵Only pixels which were at least 25% covered by the reservation were included in these calculations. The same rule was used for nighttime light pixels.

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, *500times500*-meter cells. After removing pixels covering water, I calculate the average intensity across all pixels on the reservation.⁶

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 map A, we see a direct satellite image of the entire reservation, showing us what this reservation looks like in reality. In map 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 the 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. Map 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, map D presents the NTL data for the same area and year, with the water areas already removed. Fairly consistent with map C, we see a relatively high luminosity in the urban part of the reservation, and almost no luminosity in the rural areas.

⁶For 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.

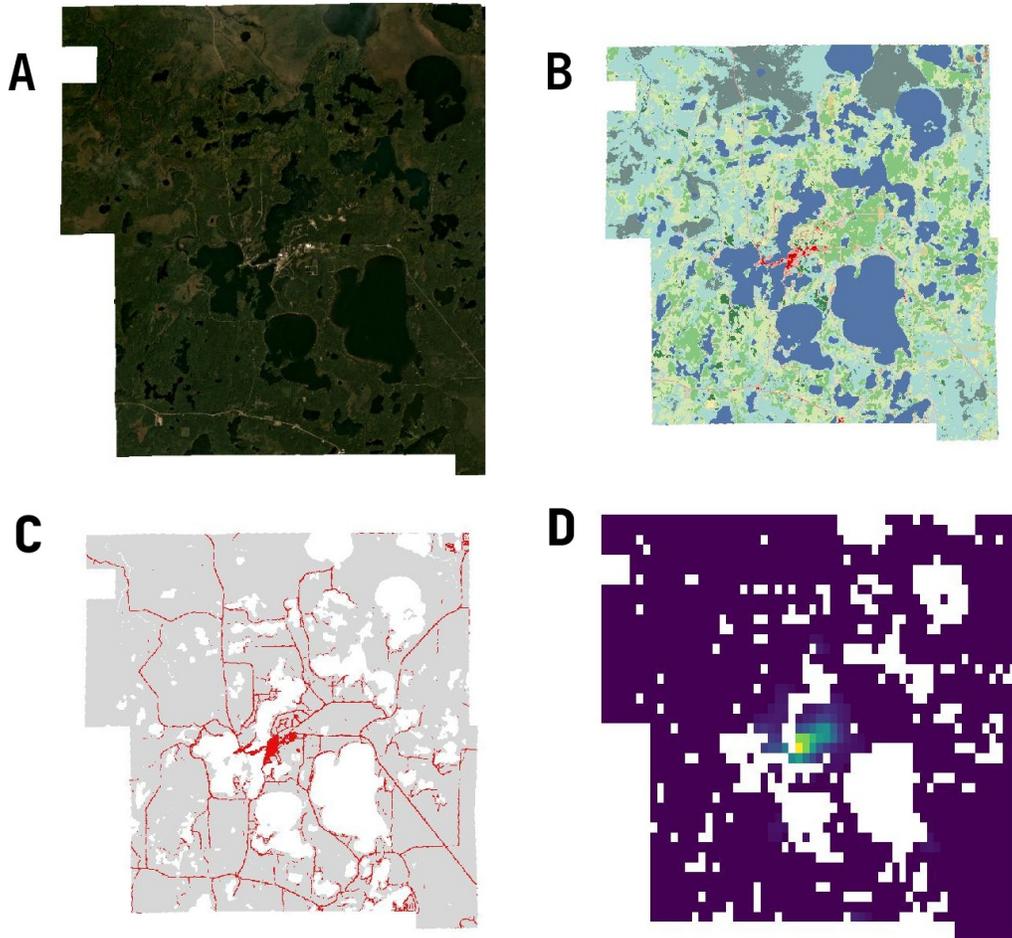


Figure 2: Example of NLCD and NTL data using the Lac du Flambeau reservation. Map A depicts the reservation through a basic satellite composite image to use as a quality reference for the other data. Map B presents the NLCD data from the same year as map A with all categories presented. Map C depicts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

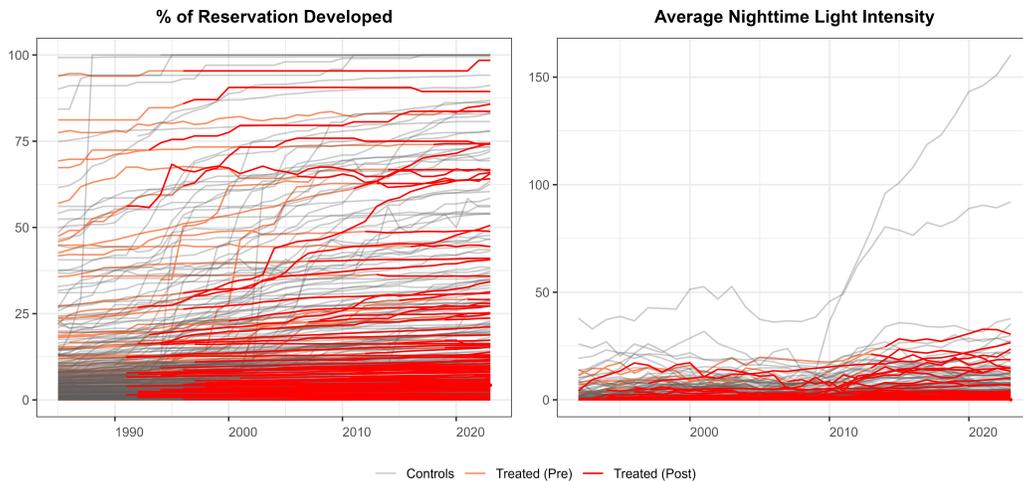


Figure 3: Change in economic development indicators. The leftmost panels depict the share of developed land and average nighttime light intensity for all reservations in sample. The two panels in the rightmost column present these same measures zoomed into the 0-0.25 range.

3.4 Measures of Governance Capacity

In order to test whether tribal governance capacity drives the relationship between SGCs and economic growth, I need to find measures that can serve as meaningful proxies. This is very challenging, given both the lack of data availability and uniqueness of tribes. 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.

Measuring tribal governance capacity is a largely unexplored concept. As a first attempt to address this, I collect eight measures — five drawn from basic characteristics of the reservation and tribe, and three from tribal constitutions.

Four measures capture basic characteristics of the reservation and tribe that indicate whether

a tribal government has adequate resources to cover the programs it runs. These are reservation geographic size, American Indian and Alaska Native (AIAN) population, tribal enrollment, and median AIAN household income. Larger reservations, greater populations, and higher enrollments all represent greater demand on tribal resources. Unlike most local governments, taxation of tribe members is not a primary revenue generator for most tribal governments (Ratté and Anderson 2022; Wilkins 2024), so a larger population does not necessarily translate into greater government revenue. Thus, these three measures proxy for the cost burden on governance. Income works in the opposite direction — higher household incomes proxy for broader economic opportunity on or near the reservation, which in turn benefits tribal government revenue through tribally-owned businesses or greater market access.

Reservation size is calculated as the geographic area of the reservation. AIAN population and AIAN household median income are taken from American Community Survey data. 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.⁷ Each of these four measures are averaged across all available years and binarized at the median.⁸

Another characteristic of tribes I consider is ethnic fractionalization, which captures a different kind of capacity constraint. 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 fractionalized 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 relies on Tiller (2015) and other historical sources to code a binary measure

⁷Data available at this archive: <https://web.archive.org/web/20250831195217/https://www.hud.gov/helping-americans/public-indian-housing-ihbgformula>.

⁸Averaging across years addresses several concerns. None of these measures 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 ACS estimates for small reservation populations are noisy, and yearly measures raise post-treatment bias concerns since economic development likely affects population, enrollment, and income. Categorizing reservations as roughly above or below the median captures stable relative differences that are unlikely to shift significantly over the study period.

of whether a reservation was created from unified or previously unrelated groups.

Increasingly, scholars have pursued looking at tribal constitutions to identify Native institutions (e.g. [Tatum et al. 2014](#), [Cordell et al. 2020](#), [Piano and Rouanet 2024](#)). I build on this work by collecting three measures of tribal political institutions from constitutions: executive election system, residency voting requirements, and the presence of a direct democracy-style assembly.⁹ 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](#)). Directly elected executives have greater discretion to act and are more responsive to voters rather than council interests, suggesting higher governance capacity. Direct democracy-style assemblies allow all adult members to participate in government decisions. While common, such assemblies can be costly, vulnerable to capture by dedicated attendees, and slower to make policy than a typical council, suggesting lower governance capacity. Finally, many tribes have a large share, in many cases a majority, of enrolled members living off-reservation, yet some restrict voting to reservation residents. This should shift government responsiveness toward on-reservation populations who benefit most from reservation development. For each of these institutions, I code a binary measure for whether or not the tribal government (1) directly elected their executive, (2) restricted voting rights geographically, and (3) had a direct democracy-style assembly.

3.5 Empirical Strategy

The primary empirical strategy I employ in this study is a series of counterfactual estimators proposed by [Liu, Wang and Xu \(2024\)](#). The key idea is straightforward: for each treated reservation-year, estimate what the outcome *would have been* in the absence of treatment, and then take the difference between the observed and counterfactual outcome as the treatment effect. The model used to generate these counterfactuals is fit using only untreated observations — all treated reservation-

⁹I 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 code all constitutions using the hand-coding as an accuracy reference.

years are held out — which avoids the negative weighting problem that afflicts conventional TWFE models..

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

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

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:

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

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.

$$\hat{Y}_{it}(0) = \beta_1 \text{Gaming}_{it} + \alpha_i + \xi_t + \varepsilon_{it} \quad (3)$$

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 believable meets the identification assumptions.

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

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

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:

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

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 E.

4 Validating Measures of Economic Growth

In order for any finding in this study to be valid, the remote sensing measures must function as a reasonable proxies for economic development on the reservation. While both land-use land cover and NTL intensity measures have been used successfully to measure economic growth, it isn’t certain that these measures will accurately capture development on reservations. Many reservations have significant portions of land that will never been developed due to cultural/historical importance or economic unsuitability. Additionally, many reservations have significant portions of which are actually held privately by non-native residents where the tribal government will have little influence.

To validate that these measures will accurately capture the economic development I are interested in, I first look to 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. This exercise is also useful for benchmarking the effect sizes I estimate after in the main results. I use a binary measure of tribal gaming to represent whether a reservation’s tribal government operated a gaming establishment in time t .¹⁰

¹⁰I 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.

In order to estimate the effect of gaming, I rely on reservation and year fixed effects to estimate the counterfactual outcomes under control, as well as the factor modeling for IFect and MC estimators. I run all three estimators, plotting in Figure 4 the ATT over time for both outcomes for the estimator which passed both the F-test and TOST equivalence test, IFect.¹¹

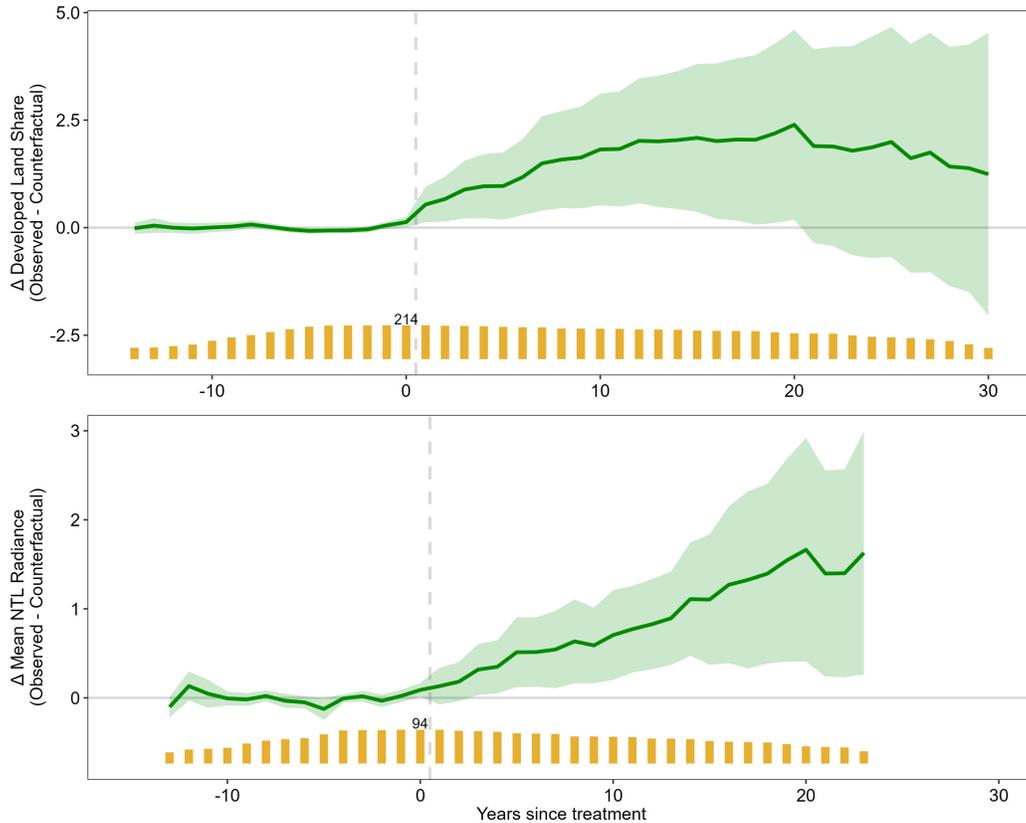


Figure 4: Dynamic treatment effects of gaming. Plots generated using estimates from IFect models using the share of developed land (top) and average NTL intensity (bottom) as the outcome. Each point estimate represents average difference between the estimated control counterfactual and the observed outcome 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 shares measure, I am able to detect a clear, positive effect to 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

¹¹A table summarizing results from all three estimators is available in Appendix C.

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.

5 Effect of SGCs

With evidence for the utility of my remote sensing outcomes, I now estimate the effect of self-governance compacting on the remote sensing measures. Similar to the gaming validation test, 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. Similar to the gaming validation, 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 expand in this context.

Inspecting how the effect evolves over time, however, reveals a more interesting pattern. Focusing on the top portion of Figure 5 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 then levels out or increases in the 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 cost to training.

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

	FEct			IFEct			MC		
	(1)	(2)	(3)	(4)	(5)	(6)			
<i>Panel A: Developed Land Share</i>									
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)			
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)			
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)			
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)			
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 / λ)	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			
<i>Panel B: NTL Radiance</i>									
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)			
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)			
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)			
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)			
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 / λ)	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 λ 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. * $p < 0.05$; ** $p < 0.01$.

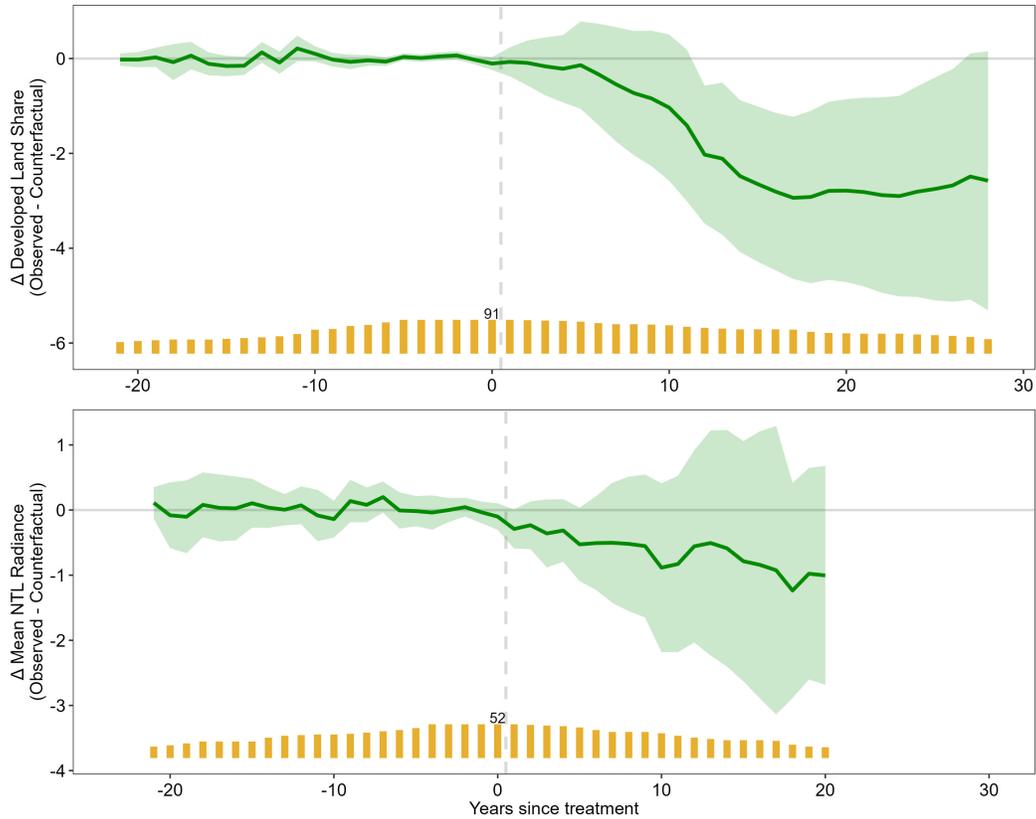


Figure 5: Dynamic treatment effects of self-governance. Plots generated using estimates from IFect models using the share of developed land (top) and average relative NTL intensity (bottom) as the outcome. Each point estimate represents average difference between the estimated control counterfactual 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.

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 predictive 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 D, I report the results of the placebo tests for

both outcomes with a dynamic treatment plot. Both show remarkable consistency in never drifting away from an ATT estimate of 0 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 E. All models report a similar effect in magnitude and statistical significance. One exception to this is 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.

Robustness Check: Other Land-use Categories

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 understand what is actually occurring on these reservations.

In Appendix F, I report the effect of SGCs on the different land classification categories in the NLCD using the IFeCt estimator. I find no effect on shrubland (desert terrain), land used for crops and ranching, or barren land. There is an inconsistent negative effect herbaceous land (i.e. grass fields), but more interesting is an increase in wetland and forests. The forest result, in particular, is statistically significant and matches the magnitude of the effect on developed land.

One potential interpretation of this result may be that actually the reduced level of developed land is not due to a lack of tribal governance capacity, but actually a deliberate choice by the tribe to slow the extraction of timber. This is possible, and would fit with other work on indigenous land maintenance (e.g. [Baragwanath and Bayi 2020](#)). However, it could also just be that reservations where capacity is low also happen to be in more forested areas as opposed to grasslands or deserts.

6 Capacity Mechanism

Next, I move on to look at how the treatment effect varies across different measures of tribal governance capacity to test whether greater capacity reduces the costs to self-governance. As a reminder, I have 8 different proxies of tribal governance capacity: reservation size, Native population, tribal enrollment size, Native household income, the degree of ethnic fractionalization in the tribe, the electoral system for the tribe executive, residency voting requirements, and the use of direct democracy-style assemblies. For each of these proxies, I dichotomize them, either as above/below the median average reservation value or 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. Instead, this exercise is initial evidence for the potential role of tribal governance capacity in economic outcomes.

First, I report the results across the different tribal and reservation characteristics in Table 2. Looking at the different sub-group pairs, we see the negative effects from self-governance are concentrated among reservations with large land areas, large populations, high enrollment, and low household income. This trend is largely consistent to both outcome measures, although only in late-treatment periods with NTL intensity. This aligns with my expectation that these factors place a greater burden on tribal governments to provide services and manage their territory.

One exception, however, is the ethnic fractionalization measure. My assumption was tribes with high ethnic fractionalization would have lower capacity and, thus, worse performance under an SGC. However, in the developed land share model, neither the low nor high sub-groups had a statistically significant effect. In later treatment years, the high fractionalization reservations did

have a more negative effect in magnitude, but this is in contrast to the NTL intensity results which point to the low fractionalization group as observing a greater cost. However, the low count of reservations in either of these sub-groups mean the estimates are noisy and likely not robust to different model specifications.

In Table 3, I present the results for the constitutions-based tribal institutions. Before interpreting the coefficients, it is worth emphasizing again that many of the groups in these models have low reservation counts like the ethnic fractionalization results, particularly with the NTL outcome the later data availability means we have more reservations without pre-treatment observations. Potentially as a result, the NTL models find very differences across sub-groups and no statistically significant effects.

The developed land models, however, have slightly more compelling results. As I predicted, reservations where the tribal government has a direct democracy-style assembly had a noticeably larger, negative effect from SGC adoption. Because of these institutions, these tribes may be less effective or less efficient with public policy. Conversely, contrary to my expectations, reservations with directly elected executives and restrictions on off-reservation voting.

Perhaps, similar to the results on the effect of SGCs on forest land, it isn't that directly elected executives and restricted voting don't increase the tribal governance capacity. Instead, it may be that these institutions increase the power of the tribe to intentionally slow growth.

The results for these institutions are also less clear compared to the tribe and reservation characteristics explored in Table 2. For example, in Figure 6, I compare the sub-group treatment effect plots based on reservation size to executive electoral system. In the reservation size results, the large reservations have a strong negative effect which is distinct from the effects observed among smaller reservations. The executive electoral system results, however, lack that clear distinction between the groups. It is also possible that the difference in effects estimated for the constitutional institutions is more an issue of power than any actual difference.

Table 2: Heterogeneous Effects of SGC by Demographic and Geographic Characteristics

	N_{tr}	ATT	ATT _{$t=1$}	ATT _{$t=6$}	ATT _{$t=12$}	ATT _{$t=18$}
Panel A: Developed Land Share						
<i>Reservation Size</i>						
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)
<i>AIAN Population</i>						
Large	50	-2.474** (0.655)	-0.612** (0.185)	-1.219** (0.377)	-2.159** (0.658)	-3.309** (0.789)
Small	39	-0.420 (1.312)	-0.535 (0.346)	-0.625 (0.898)	-0.576 (1.250)	-0.363 (1.957)
<i>Enrollment</i>						
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)
<i>Median Income</i>						
High	46	-0.952 (0.948)	-0.251 (0.155)	-0.314 (0.437)	-0.785 (0.947)	-1.598 (1.289)
Low	41	-2.617** (0.754)	-0.277 (0.275)	-1.001 (0.645)	-2.271** (0.692)	-4.061** (1.014)
<i>Ethnic Fractionalization</i>						
High Fractionalization	32	-0.428 (0.780)	0.194 (0.451)	0.418 (0.798)	-0.647 (0.684)	-1.110 (0.945)
Low Fractionalization	13	0.042 (1.507)	0.097 (0.342)	0.284 (1.124)	0.168 (1.829)	0.080 (2.434)
Panel B: NTL Radiance						
<i>Reservation Size</i>						
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)
<i>AIAN Population</i>						
Large	22	-0.946 (0.452)	-0.029 (0.127)	-0.581 (0.399)	-1.001 (0.667)	-1.730* (0.864)
Small	28	-0.459 (0.774)	-0.493 (0.262)	-0.506 (0.582)	-0.359 (1.082)	-1.126 (1.437)
<i>Enrollment</i>						
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)
<i>Median Income</i>						
High	26	0.186 (0.616)	-0.390 (0.230)	-0.331 (0.501)	0.725 (0.887)	-0.102 (0.822)
Low	23	-0.416 (0.268)	-0.294 (0.227)	0.084 (0.414)	-0.333 (0.243)	-0.872 (0.499)
<i>Ethnic Fractionalization</i>						
High Fractionalization	14	0.468 (0.693)	-0.142 (0.467)	0.680 (0.807)	0.816 (1.332)	-0.164 (0.668)
Low Fractionalization	8	-0.524 (0.322)	-0.170 (0.169)	-0.347 (0.354)	-0.783 (0.559)	-1.283* (0.612)

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. N_{tr} is the number of treated reservations in each subgroup. Continuous variables (size, population, enrollment, income, fractionalization) are split at the median across reservations. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

Table 3: Heterogeneous Effects of SGC by Constitutional Design

	N_{rr}	ATT	ATT _{$t=1$}	ATT _{$t=6$}	ATT _{$t=12$}	ATT _{$t=18$}
Panel A: Developed Land Share						
<i>Executive Selection</i>						
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)
<i>Residency Voting Requirement</i>						
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)
<i>Direct Democracy</i>						
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)
Panel B: NTL Radiance						
<i>Executive Selection</i>						
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)
<i>Residency Voting Requirement</i>						
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)
<i>Direct Democracy</i>						
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)

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. N_{rr} is the number of treated reservations in each subgroup. Continuous variables (size, population, enrollment, income, fractionalization) are split at the median across reservations. Standard errors from 1,000 bootstrap replications in parentheses. * $p < 0.05$; ** $p < 0.01$.

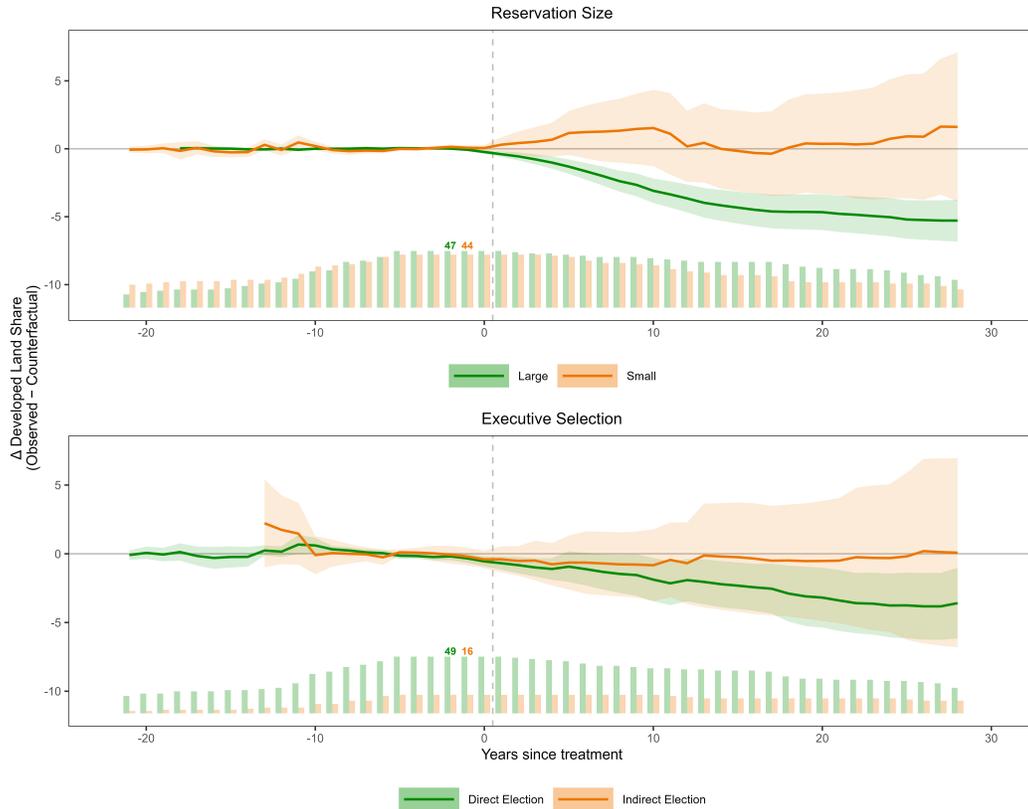


Figure 6: Reservation Size and executive election system effects comparison. Top panel presents difference in estimated effects of SGC adoption by large and small reservation size. The bottom panel presents the same effect, but split by executive electoral system.

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 reduced reservation economic performance. The results also suggest that governance capacity plays a critical mediating role in this relationship, as seen in the distinctly negative effects for reservations with greater public goods demands.

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 how we understand what is possible under self-governance. We need to keep working to understand tribal institutions, both in breadth and depth, to better understand and model the features of tribal governments. And we need to work more on learning what tribe members want from their government 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. Issues commonly analyzed in local political studies—such as housing, education, and land use—have direct analogues in the Indigenous context. Incorporating insights from these literatures could deepen understanding of how governance capacity shapes outcomes across diverse local settings.

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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

All Reservations (Average) — Original vs. Imputed Mean Radiance

Orange × marks = replaced outliers | Variable: light_mean_c25_w25

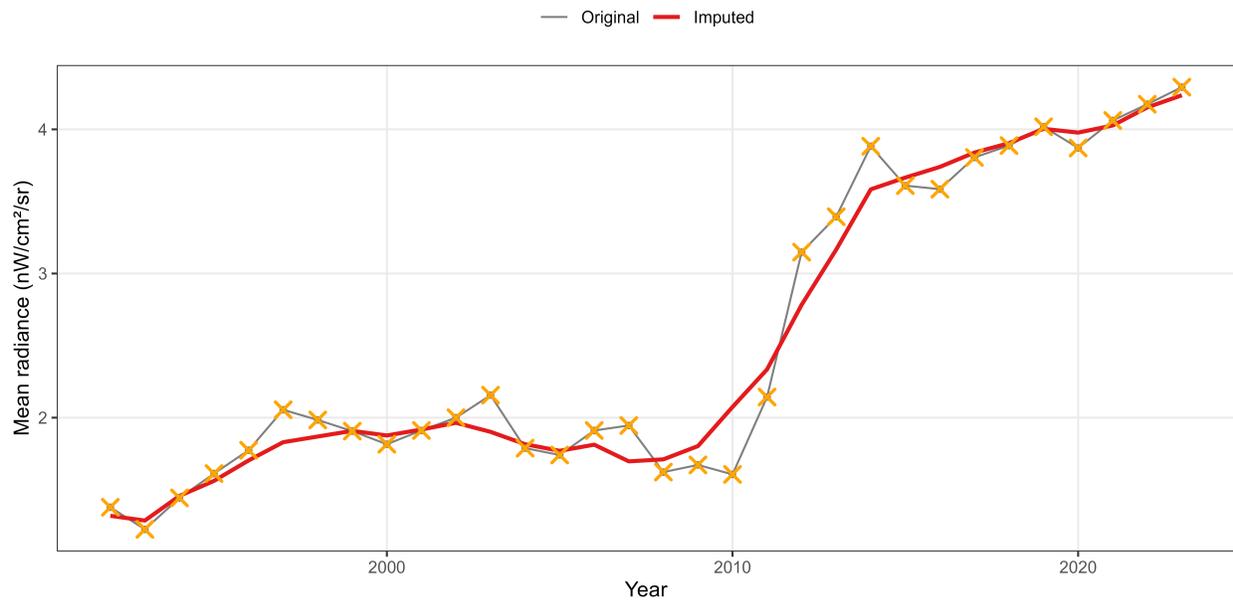


Figure B1: Average nighttime light radiance across all reservations, showing the DMSP-OLS series (1992–2013), the VIIRS series (2012–present), and the imputed bridge between the two. The imputation leverages the overlap period (2012–2013) to calibrate the DMSP values onto the VIIRS scale.

C Outcome Validation: Effect of Casino Gaming

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

	FEct (1)	IFEct (2)	MC (3)
<i>Panel A: Developed Land Share</i>			
ATT	1.645* (0.724)	1.581* (0.772)	1.632* (0.787)
ATT _{t=1}	0.690* (0.296)	0.537* (0.210)	0.695* (0.278)
ATT _{t=6}	1.427** (0.481)	1.174** (0.451)	1.409** (0.494)
ATT _{t=12}	1.991** (0.690)	2.018** (0.741)	1.941* (0.758)
ATT _{t=18}	2.022* (0.892)	2.043* (1.005)	1.987* (0.994)
Tuning (r / λ)	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
<i>Panel B: NTL Radiance</i>			
ATT	1.189** (0.265)	0.822** (0.251)	0.897** (0.281)
ATT _{t=1}	0.153* (0.072)	0.131 (0.105)	0.186* (0.095)
ATT _{t=6}	0.570** (0.166)	0.514** (0.199)	0.567** (0.193)
ATT _{t=12}	0.796** (0.236)	0.826** (0.258)	0.955** (0.309)
ATT _{t=18}	1.605** (0.378)	1.395** (0.515)	1.482** (0.545)
Tuning (r / λ)	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 λ 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 Placebo Test

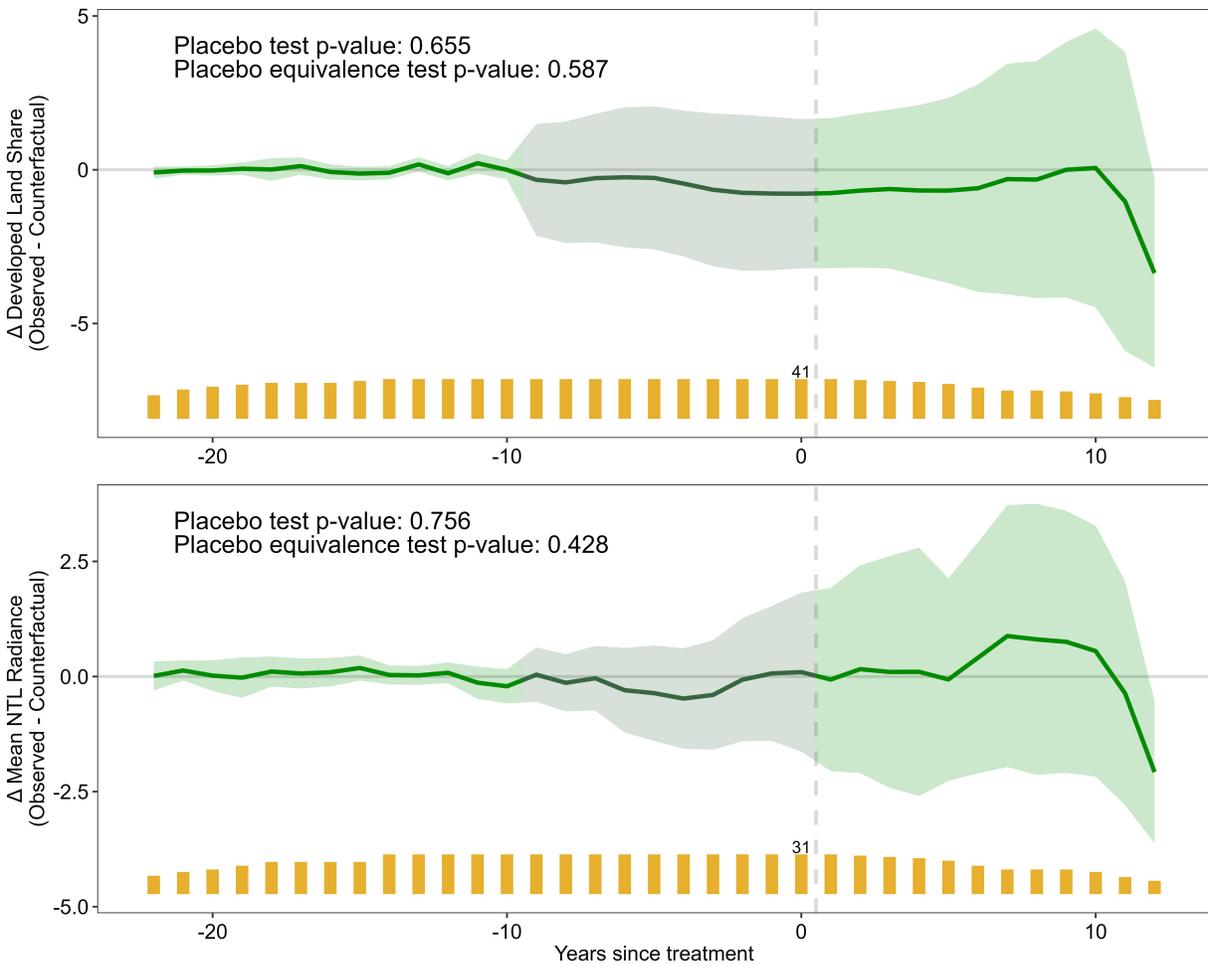


Figure D1: Placebo test results. Treatment timing is shifted 10 years prior to actual compact adoption, and the model is re-estimated to check for false treatment effects. The shaded region indicates the placebo post-treatment window.

E Alternative Difference-in-Differences Estimators

Table E1: SGC Effect Across Alternative Estimators

	TWFE (1)	Stacked (2)	Sun-Abraham (3)	CS (4)
<i>Panel A: Developed Land Share</i>				
ATT	-1.642* (0.643)	-1.793** (0.531)	-1.436* (0.612)	-1.213 (0.831)
ATT _{t=1}	-0.050 (0.055)	-0.058 (0.051)	-0.070 (0.058)	-0.139 (0.119)
ATT _{t=6}	-0.463 (0.329)	-0.501 (0.285)	-0.531 (0.320)	-0.616 (0.511)
ATT _{t=12}	-1.447* (0.591)	-1.467** (0.477)	-1.374* (0.601)	-1.187 (0.867)
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
<i>Panel B: NTL Radiance</i>				
ATT	-0.348 (0.544)	-0.434 (0.469)	-0.505 (0.297)	-0.409 (0.289)
ATT _{t=1}	-0.128 (0.082)	-0.140 (0.075)	-0.140* (0.070)	-0.137 (0.076)
ATT _{t=6}	-0.250 (0.282)	-0.265 (0.251)	-0.360 (0.219)	-0.289 (0.221)
ATT _{t=12}	-0.611* (0.260)	-0.589** (0.199)	-0.555** (0.209)	-0.472* (0.217)
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. * $p < 0.05$; ** $p < 0.01$.

F Effects on Individual Land Cover Categories

Table F1: Effect of Self-Governance Compacting on Land Cover Categories

	Forest (1)	Shrubland (2)	Herbaceous (3)	Cultivated (4)	Wetlands (5)	Barren (6)	Snow/Ice (7)
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)	0.000 (0.000)
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)	-0.000 (0.000)
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)	-0.000 (0.000)
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)	-0.000 (0.000)
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)	0.000 (0.000)
Latent Factors (<i>r</i>)	4	4	0	4	4	2	1
F-test p-value	0.872	0.274	0.498	0.885	0.621	0.620	1.000
TOST Equiv. p-value	0.086	0.161	0.895	0.000	0.000	0.000	0.000
Observations	12792	12792	12792	12792	12792	12792	12792
Years	39	39	39	39	39	39	39
Treated Reservations	91	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. * $p < 0.05$; ** $p < 0.01$.

G Heterogeneous Treatment Effects by Reservation Characteristics

G.1 Reservation Size

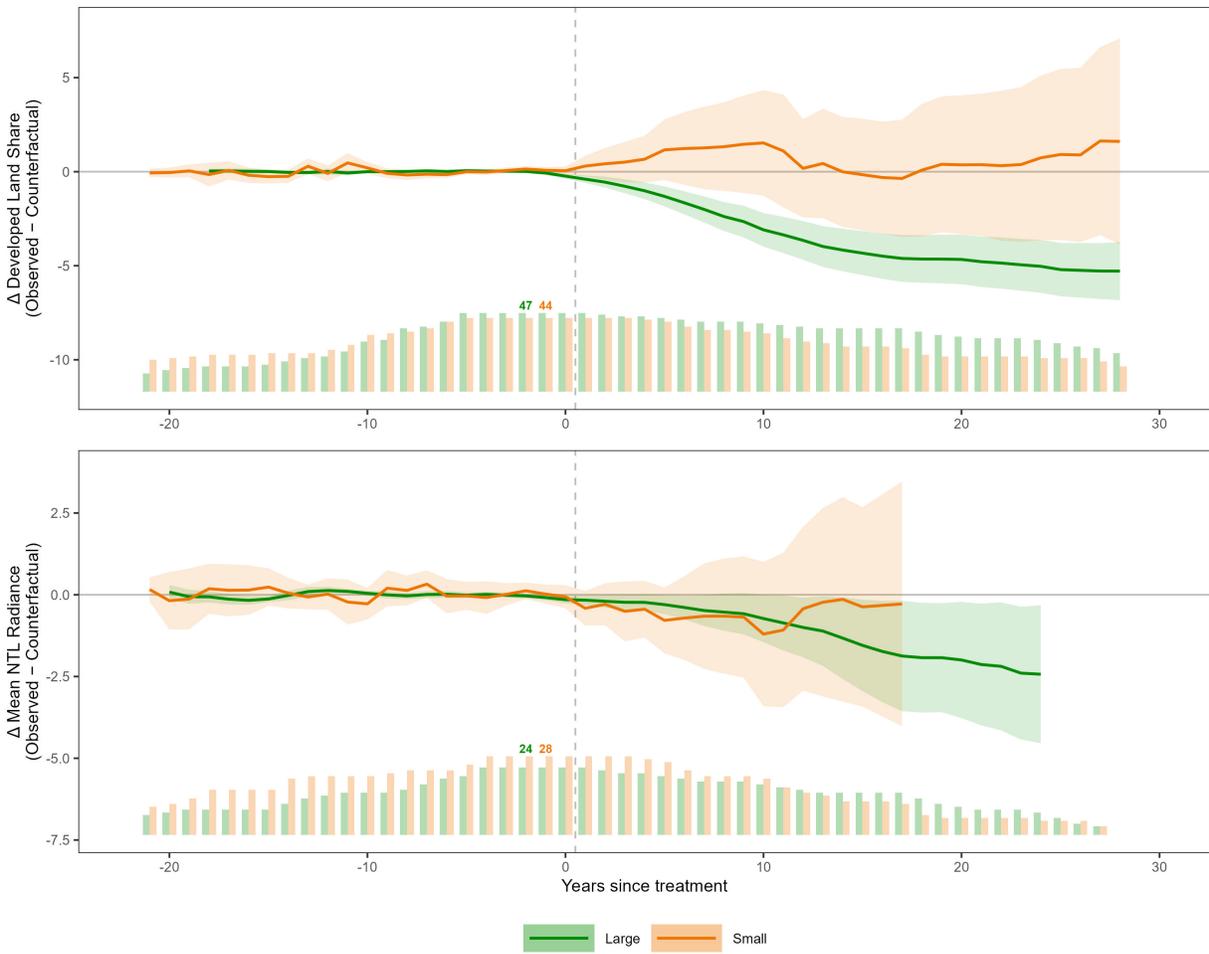


Figure G1: Dynamic treatment effects by reservation size (median split on area). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	Large	Small
<i>Panel A: Developed Land Share</i>		
ATT	-3.405** (0.486)	0.795 (1.236)
ATT _{t=1}	-0.397** (0.102)	0.300 (0.276)
ATT _{t=6}	-1.655** (0.313)	1.231 (0.986)
ATT _{t=12}	-3.652** (0.524)	0.178 (1.334)
ATT _{t=18}	-4.643** (0.646)	0.084 (1.798)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	-1.019** (0.426)	-0.413 (0.794)
ATT _{t=1}	-0.164* (0.077)	-0.413 (0.269)
ATT _{t=6}	-0.388* (0.184)	-0.713 (0.651)
ATT _{t=12}	-0.999* (0.465)	-0.427 (1.279)
ATT _{t=18}	-1.924* (0.857)	-0.731 (1.560)
Latent Factors (<i>r</i>)	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

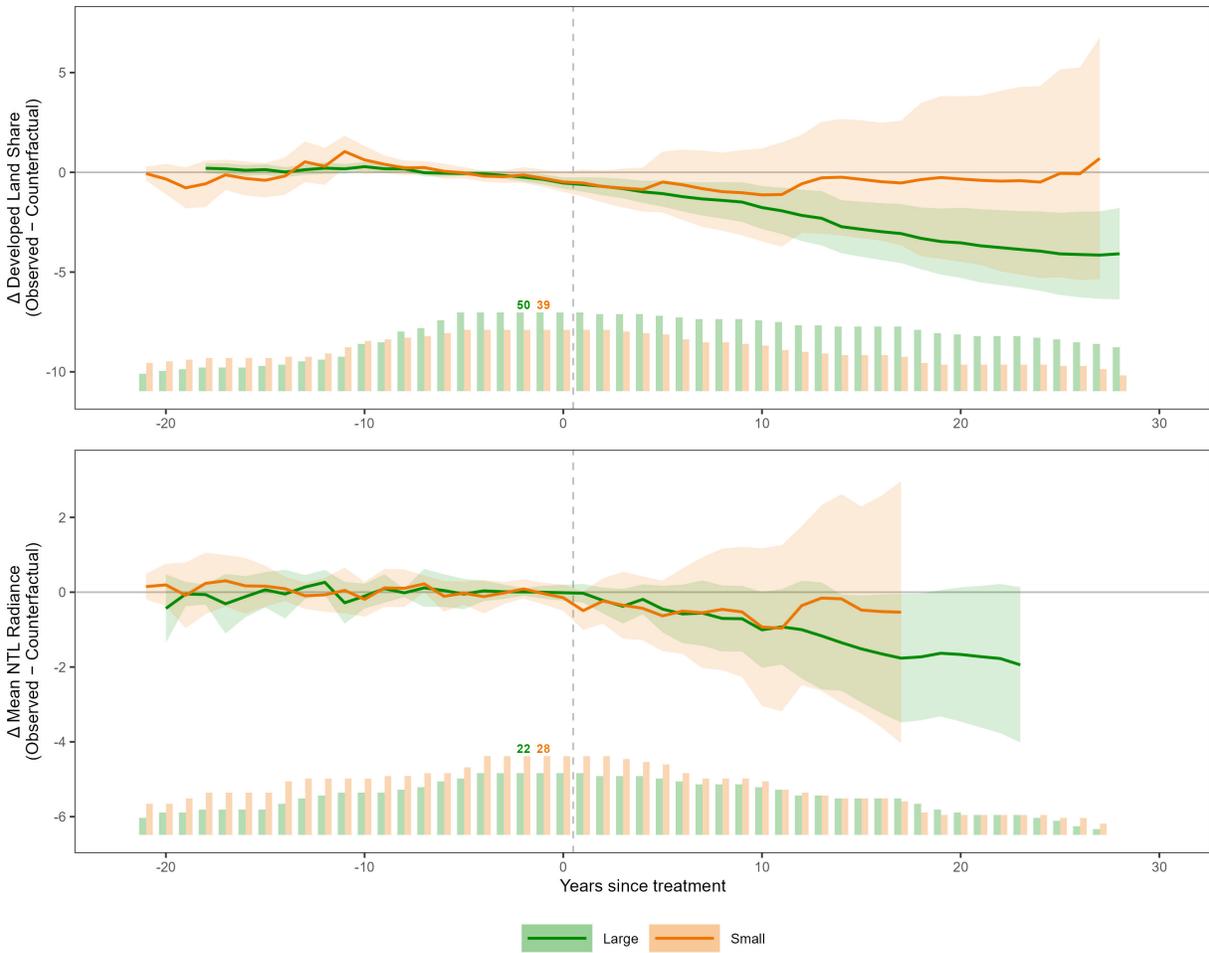


Figure G2: Dynamic treatment effects by AIAN population (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	Large	Small
<i>Panel A: Developed Land Share</i>		
ATT	-2.474** (0.655)	-0.420 (1.312)
ATT _{t=1}	-0.612** (0.185)	-0.535 (0.346)
ATT _{t=6}	-1.219** (0.377)	-0.625 (0.898)
ATT _{t=12}	-2.159** (0.658)	-0.576 (1.250)
ATT _{t=18}	-3.309** (0.789)	-0.363 (1.957)
Latent Factors (<i>r</i>)	1	1
F-test p-value	0.003	0.003
TOST Equiv. p-value	0.965	0.965
Observations	12558	12558
Years	39	39
Treated Reservations	50	39
<i>Panel B: NTL Radiance</i>		
ATT	-0.946 (0.452)	-0.459 (0.774)
ATT _{t=1}	-0.029 (0.127)	-0.493 (0.262)
ATT _{t=6}	-0.581 (0.399)	-0.506 (0.582)
ATT _{t=12}	-1.001 (0.667)	-0.359 (1.082)
ATT _{t=18}	-1.730* (0.864)	-1.126 (1.437)
Latent Factors (<i>r</i>)	1	1
F-test p-value	0.741	0.741
TOST Equiv. p-value	0.279	0.279
Observations	8832	8832
Years	32	32
Treated Reservations	22	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.3 Tribal Enrollment

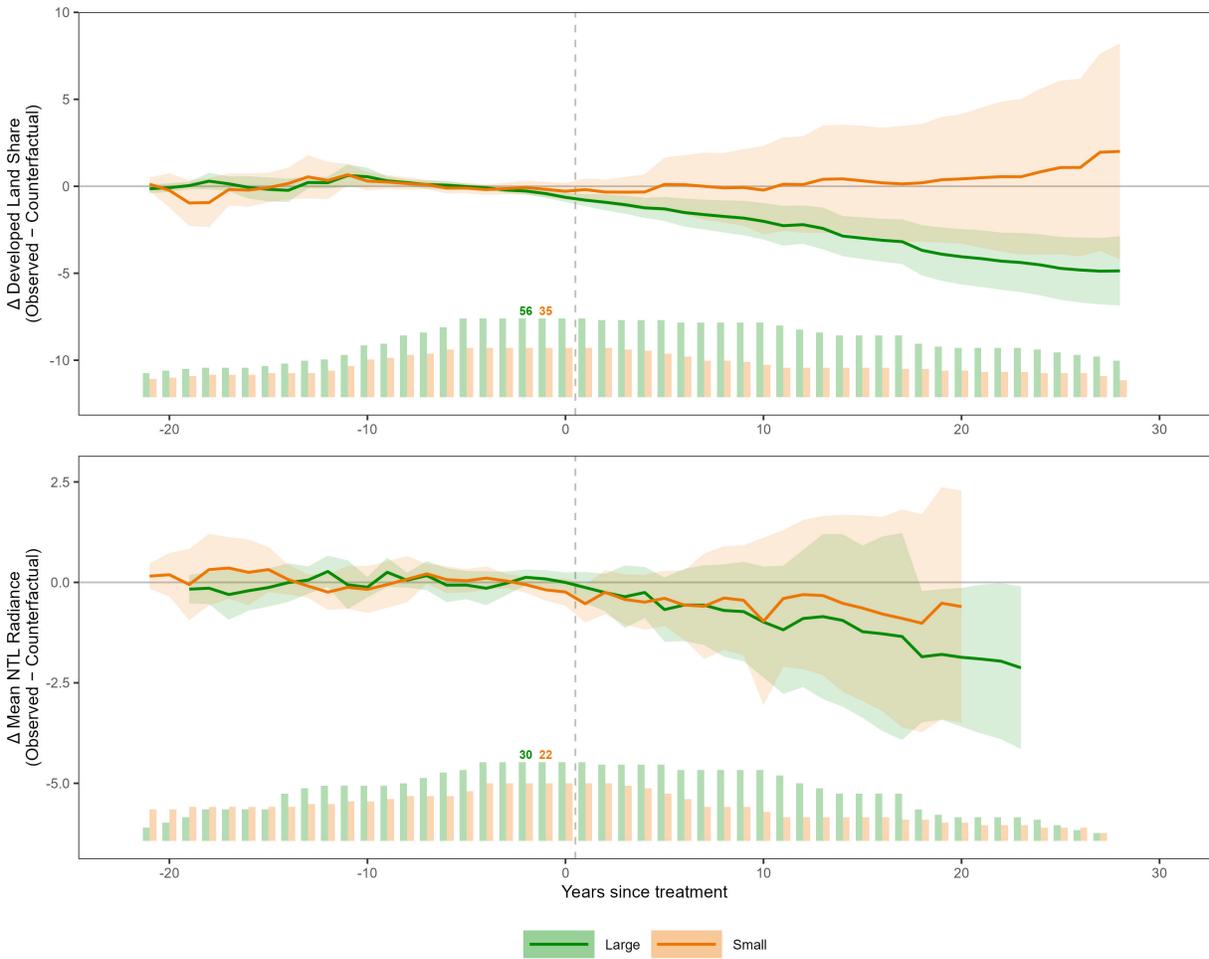


Figure G3: Dynamic treatment effects by tribal enrollment (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	Large	Small
<i>Panel A: Developed Land Share</i>		
ATT	-2.760** (0.552)	0.455 (1.375)
ATT _{t=1}	-0.797** (0.177)	-0.190 (0.325)
ATT _{t=6}	-1.510** (0.408)	0.091 (0.865)
ATT _{t=12}	-2.206** (0.561)	0.100 (1.415)
ATT _{t=18}	-3.681** (0.738)	0.198 (1.725)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	-0.916 (0.558)	-0.391 (0.766)
ATT _{t=1}	-0.128 (0.198)	-0.532* (0.238)
ATT _{t=6}	-0.564 (0.459)	-0.565 (0.441)
ATT _{t=12}	-0.899 (0.871)	-0.305 (0.947)
ATT _{t=18}	-1.851* (0.833)	-1.016 (1.383)
Latent Factors (<i>r</i>)	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 Median Income

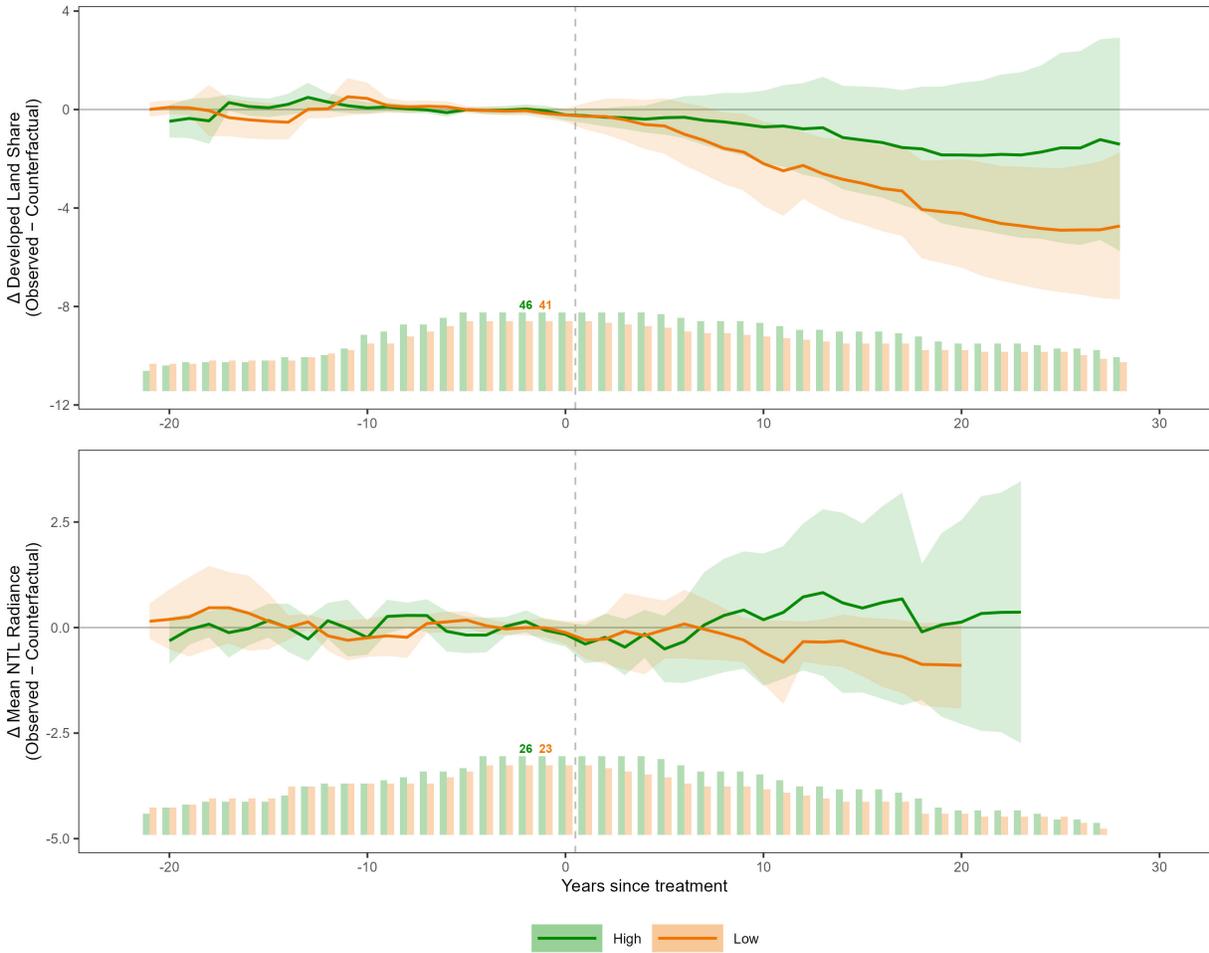


Figure G4: Dynamic treatment effects by AIAN median income (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

Table G4: Heterogeneous Effects of SGC by Median Income — Full Diagnostics

	High	Low
<i>Panel A: Developed Land Share</i>		
ATT	-0.952 (0.948)	-2.617** (0.754)
ATT _{t=1}	-0.251 (0.155)	-0.277 (0.275)
ATT _{t=6}	-0.314 (0.437)	-1.001 (0.645)
ATT _{t=12}	-0.785 (0.947)	-2.271** (0.692)
ATT _{t=18}	-1.598 (1.289)	-4.061** (1.014)
Latent Factors (<i>r</i>)	2	2
F-test p-value	0.385	0.385
TOST Equiv. p-value	0.476	0.476
Observations	11544	11544
Years	39	39
Treated Reservations	46	41
<i>Panel B: NTL Radiance</i>		
ATT	0.186 (0.616)	-0.416 (0.268)
ATT _{t=1}	-0.390 (0.230)	-0.294 (0.227)
ATT _{t=6}	-0.331 (0.501)	0.084 (0.414)
ATT _{t=12}	0.725 (0.887)	-0.333 (0.243)
ATT _{t=18}	-0.102 (0.822)	-0.872 (0.499)
Latent Factors (<i>r</i>)	1	1
F-test p-value	0.764	0.764
TOST Equiv. p-value	0.476	0.476
Observations	8160	8160
Years	32	32
Treated Reservations	26	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.5 Executive Selection

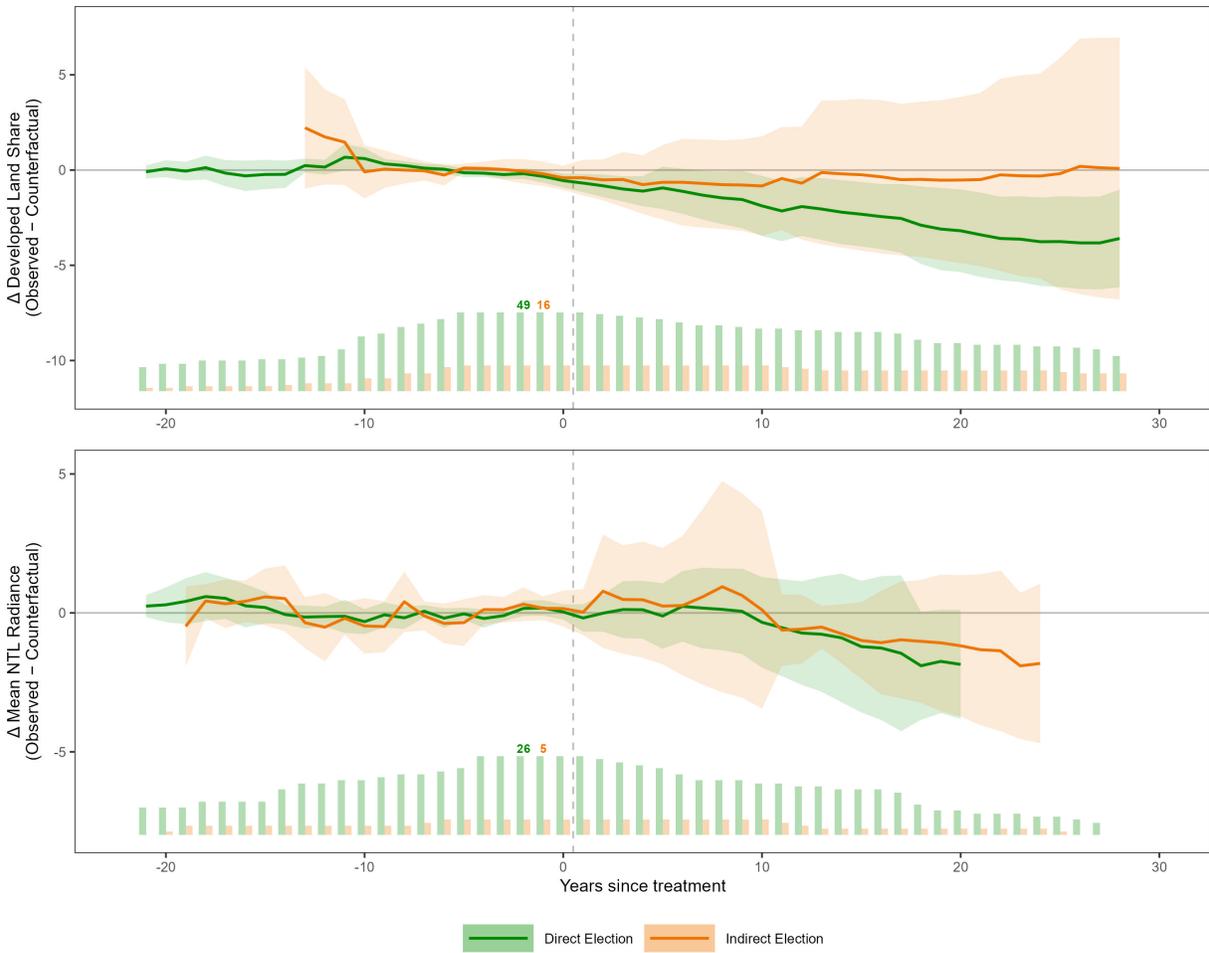


Figure G5: Dynamic treatment effects by executive selection method. Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	Direct Election	Indirect Election
<i>Panel A: Developed Land Share</i>		
ATT	-2.180** (0.792)	-0.421 (1.811)
ATT _{t=1}	-0.684** (0.244)	-0.405 (0.480)
ATT _{t=6}	-1.116 (0.602)	-0.642 (1.159)
ATT _{t=12}	-1.916* (0.787)	-0.688 (1.512)
ATT _{t=18}	-2.896** (1.036)	-0.490 (2.078)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	-0.592 (0.612)	-0.152 (1.107)
ATT _{t=1}	-0.182 (0.253)	0.028 (0.418)
ATT _{t=6}	0.236 (0.647)	0.265 (1.278)
ATT _{t=12}	-0.724 (0.951)	-0.585 (0.630)
ATT _{t=18}	-1.901 (0.991)	-1.023 (1.126)
Latent Factors (<i>r</i>)	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

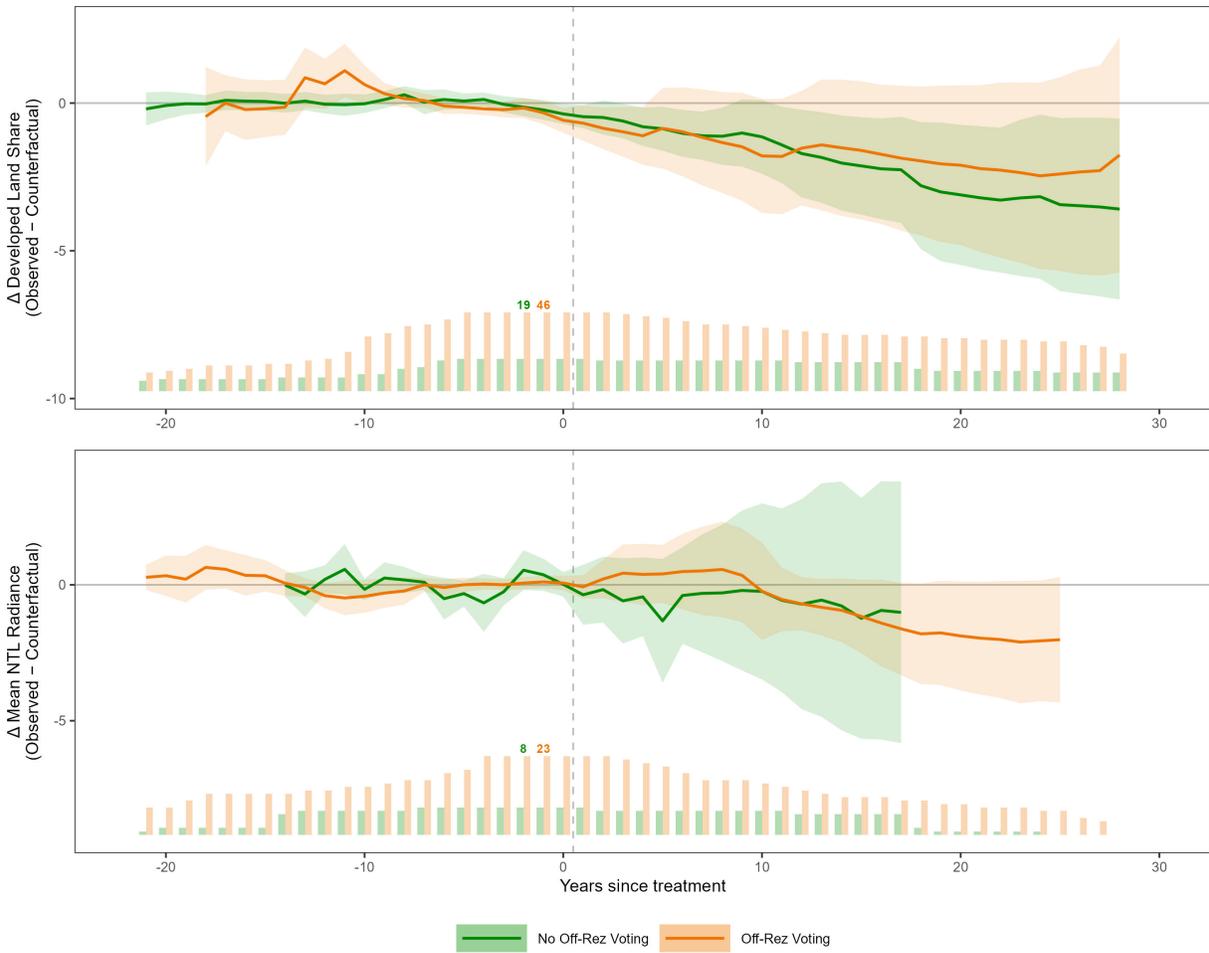


Figure G6: Dynamic treatment effects by residency voting requirement. Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	No Off-Rez Voting	Off-Rez Voting
<i>Panel A: Developed Land Share</i>		
ATT	-2.046* (0.813)	-1.516 (1.061)
ATT _{t=1}	-0.461* (0.198)	-0.679* (0.303)
ATT _{t=6}	-1.023* (0.404)	-0.972 (0.768)
ATT _{t=12}	-1.709* (0.757)	-1.525 (0.988)
ATT _{t=18}	-2.795* (1.097)	-1.959 (1.284)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	-0.591 (1.167)	-0.488 (0.533)
ATT _{t=1}	-0.365 (0.561)	-0.073 (0.219)
ATT _{t=6}	-0.392 (0.905)	0.488 (0.709)
ATT _{t=12}	-0.709 (1.971)	-0.698 (0.503)
ATT _{t=18}	-1.514 (1.490)	-1.803 (0.944)
Latent Factors (<i>r</i>)	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

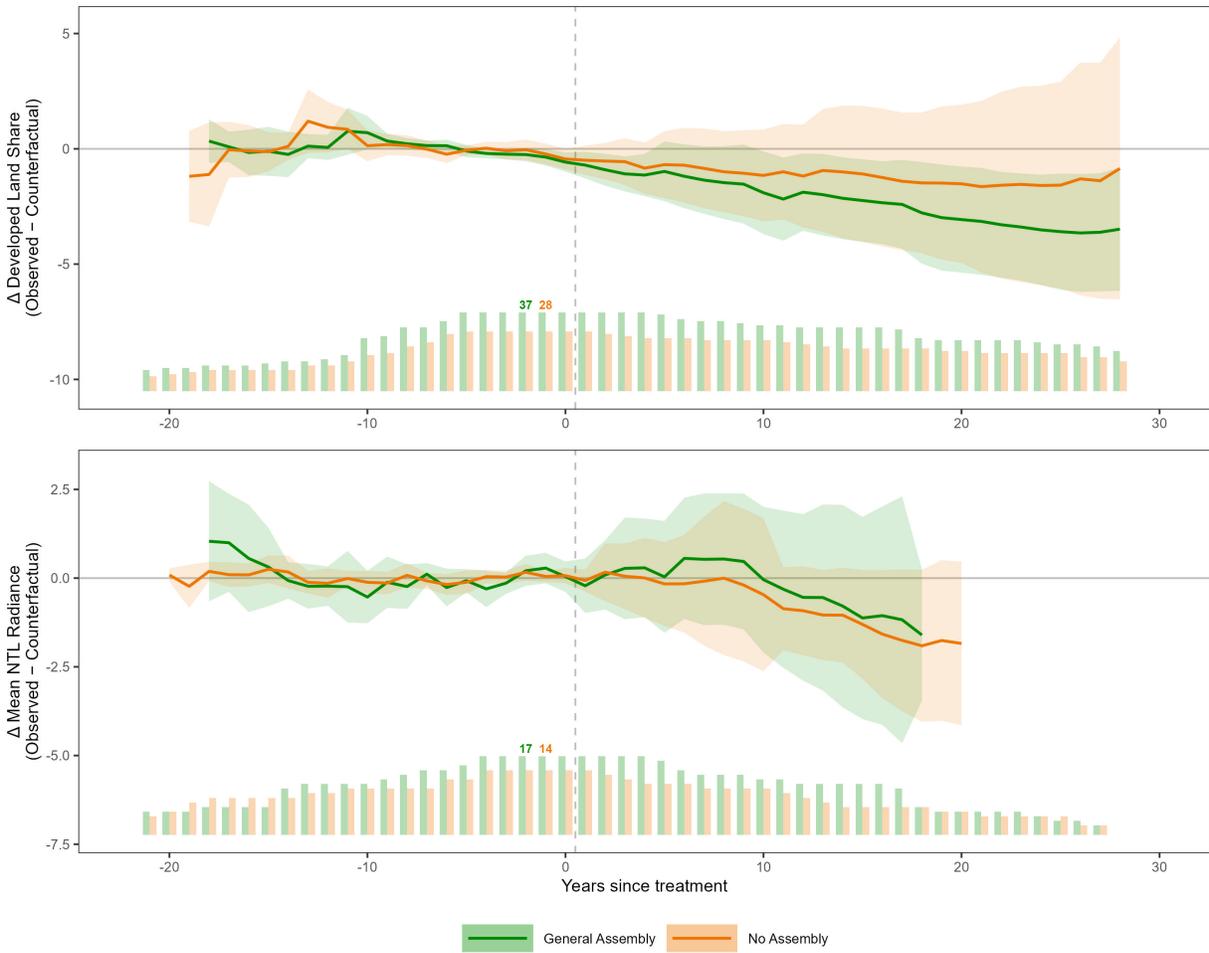


Figure G7: Dynamic treatment effects by direct democracy (general assembly vs. representative government). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	General Assembly	No Assembly
<i>Panel A: Developed Land Share</i>		
ATT	-2.157** (0.853)	-1.034 (1.352)
ATT _{t=1}	-0.707* (0.292)	-0.495 (0.320)
ATT _{t=6}	-1.191 (0.706)	-0.710 (0.827)
ATT _{t=12}	-1.881* (0.856)	-1.183 (1.136)
ATT _{t=18}	-2.779* (1.124)	-1.478 (1.557)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	-0.366 (0.718)	-0.715 (0.660)
ATT _{t=1}	-0.216 (0.391)	-0.067 (0.160)
ATT _{t=6}	0.557 (0.871)	-0.159 (0.701)
ATT _{t=12}	-0.544 (1.196)	-0.918 (0.639)
ATT _{t=18}	-1.603 (0.945)	-1.907 (1.092)
Latent Factors (<i>r</i>)	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

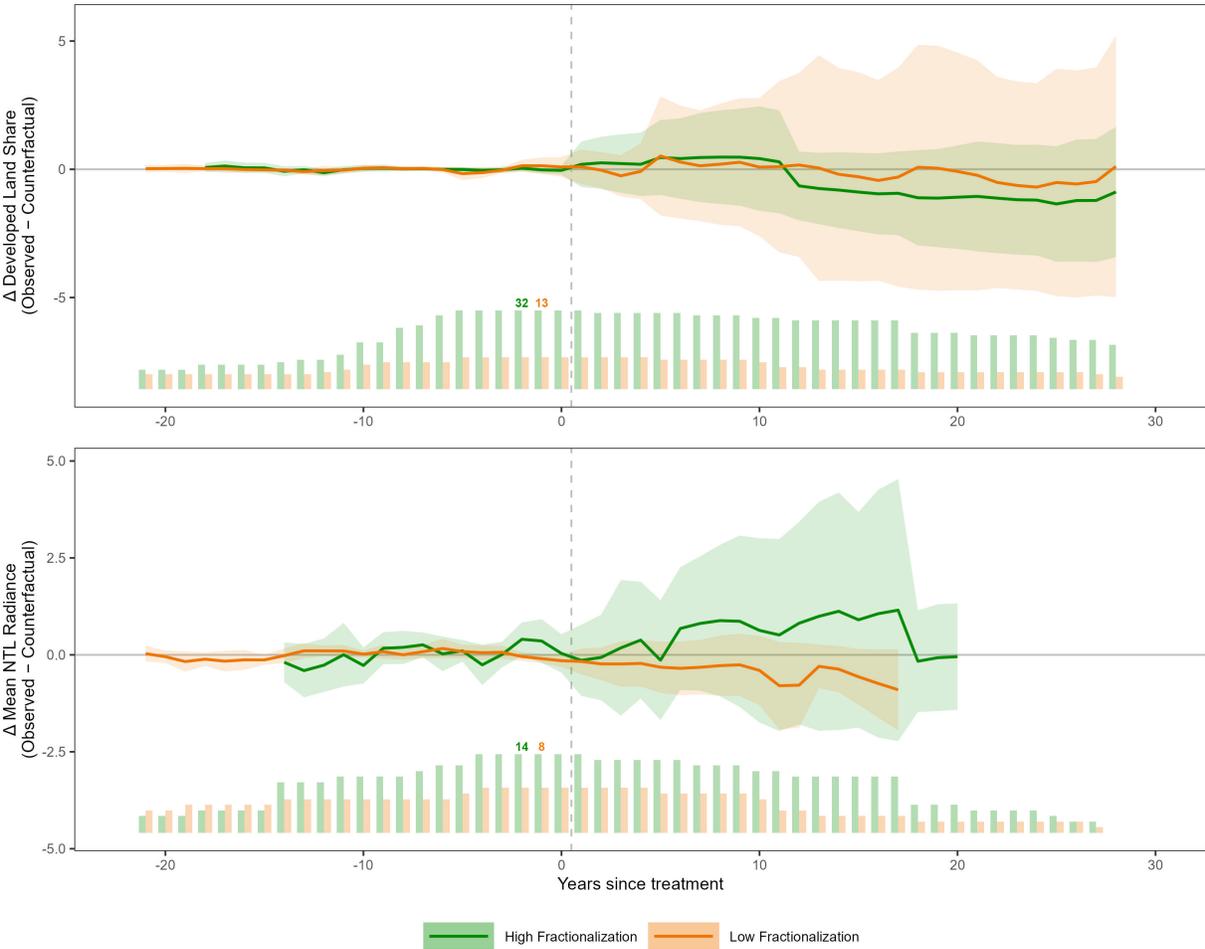


Figure G8: Dynamic treatment effects by ethnic fractionalization (median split). Top panel: Developed Land Share. Bottom panel: NTL Radiance.

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

	High Fractionalization	Low Fractionalization
<i>Panel A: Developed Land Share</i>		
ATT	-0.428 (0.780)	0.042 (1.507)
ATT _{t=1}	0.194 (0.451)	0.097 (0.342)
ATT _{t=6}	0.418 (0.798)	0.284 (1.124)
ATT _{t=12}	-0.647 (0.684)	0.168 (1.829)
ATT _{t=18}	-1.110 (0.945)	0.080 (2.434)
Latent Factors (<i>r</i>)	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
<i>Panel B: NTL Radiance</i>		
ATT	0.468 (0.693)	-0.524 (0.322)
ATT _{t=1}	-0.142 (0.467)	-0.170 (0.169)
ATT _{t=6}	0.680 (0.807)	-0.347 (0.354)
ATT _{t=12}	0.816 (1.332)	-0.783 (0.559)
ATT _{t=18}	-0.164 (0.668)	-1.283* (0.612)
Latent Factors (<i>r</i>)	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. * $p < 0.05$; ** $p < 0.01$.