

Land Titling Policies and Economic Growth: An Empirical Analysis of the First Nations Land Management Act in Canada

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Abstract

This paper seeks to gauge the economic impact of the First Nations Land Management Act in Canada. The legislation, passed in 1999, allowed First Nations in Canada to develop their own land title policies, whereas previously such policies had been developed for them by the Canadian federal government. Theory predicts that a more localized land policy, backed by the federal government but created by the First Nation, will lead to greater economic growth as opposed to land policies that are poorly defined by the federal government. The paper seeks to contribute to the literature in institutional economics while avoiding some of the omitted variable problems associated with cross-country institutional comparisons.

I Introduction

Recent literature in institutional economics employing cross-country analysis has shown positive correlation between a nation's per-capita income and its security of property and contractual rights (Keefer and Knack, 1997). However, Glaeser et al. (2004) conclude that commonly used measures of institutional capacity fail to establish causality between institutions and growth. Pande and Udry (2006) note the greatest problem with recent cross-country literature on institutions and growth is the statistical challenge of isolating causal pathways. To avoid the problems associated with cross-country comparisons, some recent studies have focused on reservation economies within the United States. Anderson and Parker (2008; 2009) studied US reservations, focusing specifically on the economic effects of reservations placed under state jurisdiction in the 1950s. In treating the legislation as a natural experiment, they use it to gauge institutional effects.

A similar natural experiment exists in Canadian reservations. The First Nations Land Management Act passed in 1999, allows First Nations (essentially identical to the "tribe" in the American experience) to devise their own land policies rather than the Canadian federal government having this responsibility. As the economic effects of the Act have not been measured, I seek to determine whether First Nations adopting the First Nations Land Management Act saw greater economic growth as compared to those who did not.

If such legislation proves successful, it could greatly help the poverty stricken First Nations. As Anderson and Parker (2009) note, "Most American Indian reservations and Canadian First Nation reserves are islands of poverty in a sea of relative wealth." Aboriginals living on First Nations are among the poorest citizens in Canada, with living conditions mimicking and sometimes worse than Third World conditions. In Canada, the per-capita income of Aboriginals living on reserves in 2000 was Can \$9,257, compared to Can \$14,258 for Aboriginals living off reserves and Can \$23,712 for all Canadian citizens. The income of indigenous people living on reserves lags well behind those living off, while the income of those living off reserves still lags behind that of non-Indians. If we wish to see these populations increase their economic wealth and welfare, then we must analyze the causal pathways that lead to such success. This paper is an attempt to understand the effects that institutions, specifically a localized, First Nation specific land policy, can have on economic growth. Future First Nations adopting the Land Management Act will benefit from the knowledge of which adopted land policies were most effective in increasing economic growth. Developing countries seeking to improve economic growth will also benefit from knowledge of how specific institutional changes can affect growth.

The first part of the paper reviews recent literature on cross-country comparisons of institutions. The problems of such comparisons are detailed, and an analysis of institutions on American Indian reserves follows. The third section explains the current titling policies on the Canadian First Nations, and how the First Nations Land Management Act can change them. My data sources are explained in the third section, followed by my hypothesis and a detailed explanation of my research design. The results of my regressions come in the sixth section, followed by my conclusions in the final section.

II Background and Literature Review

i. Definition of institutions and a cross-country comparison

Although there is no single, conclusive definition of an institution, Douglass North provides the best explanation: “Institutions are the rules of the game in a society, or, more formally, are the humanly devised constraints that shape human interactions. In consequence they structure incentives in human exchange, whether political, social, or economic” (North, 1990). Institutions are essentially the “rules of the game,” consisting of both formal and informal rules and norms that govern human interaction and structure individual behavior. That institutions affect the performance of economies is “hardly controversial,” according to North.

Recent literature comparing institutions across countries readily concludes that institutional quality is a significant determinant of a country’s growth performance. Keefer and Knack (1997) seek an explanation for the divergence between rich and poor countries that can be derived from institutional performance. They hypothesize that an inadequate political, legal, and regulatory framework the “institutional environment” - may reduce investment, as well as a country’s ability to adopt technological advances from abroad. Their analysis employs various measures of institutional quality, including the strength of the rule of law, the pervasiveness of corruption, and the risk of expropriation and contract repudiation.

Keefer and Knack use several sources to measure the security of property and contractual rights. Two independent international investor risk services, the International Country Risk Guide and Business Environmental Risk intelligence, evaluate bureaucratic quality and corruption, as well as the rule of law, the risk of expropriation, and contract enforcement. A third source of data, which measures the constraint of the executive branch, comes from the Polity II Codebook (Gurr et al., 1989).

As Keefer and Knack note, one way in which countries commonly secure property rights and avoid arbitrary policy changes is by constraining the decision-making

powers of the executive branch of government. To measure these constraints, they use both the Rule of Law variable included in the Country Risk report, as well as the Executive Constraints indicator from the Polity II Codebook. The Rule of Law variable is higher when a country exhibits sound political institutions, a strong court and provisions for an orderly succession of power. The Executive Constraints variable is low when “constitutional restrictions on executive action are ignored, the constitution is frequently revised at the executive’s initiative, rule by decree is repeatedly used, and the executive appoints and removes at will the members of any group meant to oversee the executive” (Keefe and Knack, 1997).

Bureaucratic quality is also used as a measure of institutional quality. Bureaucracies are more likely to act arbitrarily (and therefore increase the insecurity of property and contractual rights) when there are few institutional restrictions on them and when the quality of the administrative officials is poor. Two subjective variables are used to measure bureaucratic quality: Bureaucratic Quality (from Country Risk) and Bureaucratic Delays (from Business Risk). Countries score high on these categories when the bureaucracy has the strength and knowledge to govern without drastically changing policy or interrupting government services.

The standard model explaining rates of convergence includes growth as the dependent variable, with the log of per-capita income at time zero, human capital, physical capital, labor, and political instability as independent variables. Keefe and Knack modify this model by substituting institutional variables for political stability, and allowing for an additional term interacting institutional quality and initial income. In running the regression with two different measures of institutional quality, Rule of Law and Contract Enforceability, the institutional and interacting variable were shown to be statistically and economically significant. Both growth and convergence are faster when institutions are better. The explanatory power of the regression is also increased with the inclusion of the institutional and interaction terms: with contract enforceability, the adjusted R squared term rises from .295 to .406; in the case of rule of law, it rises from .256 to .398.

Keefe and Knack consider the robustness of their results, focusing on whether good institutions are the product of reverse causality - that is, whether countries growing rapidly choose to spend more resources to develop better institutions. If causality operated from growth to institutions, then regressions employing end of period values of institutional indicators should provide larger coefficients than regressions relying on older data. This does not appear to be the case, as when 1992 values are substituted for 1982 values in the Country Risk Index, the results are similar. Overall, the paper shows that institutions are powerful determinants of the ability for poor countries to catch up with rich.

ii. Problems with cross-country comparisons

Glaeser et al. (2004) have questioned whether institutions do in fact cause growth. They find that most indicators of institutional quality used to establish the proposition that institutions cause growth are conceptually unsuitable for that purpose. Additionally, they find that some of the instrumental variable techniques used in the literature are flawed. They examine three measures of institutions currently found in economic growth literature: risk of expropriation by the government, government effectiveness, and constraints on the executive. The first two measures by construction do not describe political institutions, but rather are outcome measures that primarily reflect past government restraint from expropriation. These measures cannot differentiate between “dictators who choose to respect property rights and democratically elected leaders who have no choice but to respect them” (Glaeser et al., 2004). As an example, dictators who chose to respect property rights - in the U.S.S.R. or Singapore - received high scores, an indicator of “good institutions.” In the short-term, dictators who choose to respect property rights might be as equally effective as institutions that protect property rights. But if we are concerned with durable, long term institutions that constrain action, a dictator choosing a policy that respects property rights cannot be considered a good institution.

Glaeser et al. consider the third measure, constraint on the executive, to be reflective only of the outcomes of the most recent election and not principally linked to constraints on government. When countries have inconsistent electoral experiences, their scores fluctuate wildly. Take Haiti as an example: “Haiti gets the worst score of 1 under the dictatorship during 1960-1989, jumps up to 6 when Aristide is elected in 1990, goes back to 1 when he is ousted during 1991-1993, rises again to 6 and even a perfect score of 7 during 1994-1998 as Aristide and his party return to power (even though the elections had been widely criticized), but falls down all the way to 3 during 2000-2001” (Glaeser et al., 2004). It is clear that this is a measure of the rapidly changing assessments of electoral outcomes, and not a measure of executive constraint. Therefore, in richer countries, where elections are more orderly and defined, “constraints on the executive” may well be the consequence of economic development.

Overall, Glaeser et al. conclude that commonly used measures of institutions cannot be used to establish causality. They do not reflect permanent features of the political landscape, nor constraints on the executive. Research on institutional economics and the consequences of alternative institutions must focus on actually enforced rules, not conceptually ambiguous assessments of institutional outcomes. These actual rules and laws could be assessed and manipulated by policy makers to see what actually works.

Pande and Udry (2006) note that the greatest problem with recent cross-country literature on institutions and growth is the statistical challenge of isolating causal pathways. The exogenous variables used to measure the character of institutional development are limited, and their broad definitions prevent precise analysis to understand how institutions lead to growth. At best, the literature supports the fact that institutions and economic growth jointly cause each other, although the direction of causation is still not well defined. The scope for using cross-country data for identifying channels of influence is limited.

Pande and Udry argue that due consideration must be given to the appropriate unit of analysis when considering the relationship between institutions and growth. They suggest that a unit smaller than a country may provide a more homogeneous environment for a certain institution, and therefore reveal more about the causal role associated with that institution. Empirical research based on micro-data in development economics may be a better source to explain the relationship between institutions and growth. Country-specific policies that implement institutional change are an opportunity for such research because they allow for examination of a more homogeneous environment as well as provide an opportunity to obtain instruments which can help isolate the effects of specific institutions.

iii. Institutions on American Indian reservations

Recent literature focusing on American Indian reservations and economic growth associated with institutions has readily taken advantage of Pande and Udry's suggestions. Reservations' economies provide a good source for testing the effects of institutions because the data are free from some of the reverse-causation and omitted-variable problems found in cross-country comparisons. The federal government often imposes institutions on reservations, rather than tribes endogenously selecting these institutions. Additionally, reservations operate broadly within the framework of the United States, and are therefore impacted by the same exogenous shocks that affect people living in adjacent locations. For these reasons, researchers using reservation data face an easier task in explaining causal mechanisms from institutions to growth (Parker, 2009).

Papers by Parker (2009) and Anderson and Parker (2008) examine the institutional difference between reservations that have tribal jurisdiction over criminal offenses and civil suits, and reservations that have state courts which handle such jurisdiction. The variation is due to federal legislation, Public Law 280, passed in 1953, which required that jurisdiction over major criminal offenses committed on reservations be transferred from the federal government to the state containing the

reservation land. PL 280 also transferred jurisdiction over minor criminal offenses and civil disputes from tribes to the state. Today, state legal systems that are anchored in the British common law tradition have authority on some reservations and tribal systems of dispute resolution have authority on other reservations. Furthermore, the selection of reservations for state jurisdiction was not based on any economic criteria, which makes the legislation a quasi-natural experiment in legal institutions (Parker, 2009). Tribes did not self-select state jurisdiction, which reduces the likelihood that the legislation was enacted on those reservations best positioned to benefit.

Reservations that retain tribal courts and tribal jurisdiction suffer from the dilemma of the sovereign (Anderson and Parker, 2008). Tribal courts have the ability to enforce property rights and contracts, as well as to break those rights and contracts. A sovereign power thus has the ability to contribute to public goods and increase productivity or pursue rent-seeking behavior. There are two ways in which a tribe can succumb to the dilemma of the sovereign. First, a tribe can change a contract after the fact without repercussion by claiming sovereign immunity. Second, a tribal court can selectively enforce contracts and property rights between non-members and members. In each of these two ways, a tribe can lose its credible commitment to enforce property rights and contracts.

To escape the problems of credible commitment a tribe can relinquish its sovereign judicial authority to a sovereign power with reputation capital, such as state courts. This was done under PL 280, and assuming state courts have a stronger reputation for enforcing contracts, such a change would increase the willingness of outsiders to contract with Indians. PL 280 can essentially be seen as a credible commitment mechanism. It gives non-Indian plaintiffs the right to file suits over contract disputes with Indian defendants in state courts, as well as enable parties contracting with tribes the ability to sue tribal businesses in state courts. The hypothesis follows that American Indians under PL 280 jurisdiction will experience greater economic prosperity because of the credible commitment. Investors are more willing to invest in physical capital on reservations and to provide credit to Indians on reservations that are under state jurisdiction.

Anderson and Parker (2008) use data for reservations where the 1999 American Indian population exceeds 1,000. This limits the analysis to 81 of the 327 reservations in the United States, but accounts for over 90 percent of the 512,431 Indians living in reservations. As Parker and Anderson note, this approach has the advantage of preventing results from being strongly influenced by the incomes of a small number of Indians living on small reservations. The primary dependent variable in their analysis is the 1969 to 1999 per-capita income growth of American Indians. They begin measuring income growth in 1969, even though PL 280 was passed in 1953,

for three reasons. First, many optional PL 280 states (that is, states that had the option of enacting the legislation) did not enact it until the 1960s. Second, it took time for courts to sort out the full civil jurisdictional effects. Finally, PL 280 was amended in 1968 to allow state-initiated retrocession, although tribes were given no mechanism for securing such retrocession. By 1968 it was clear that the jurisdiction of PL 280 could not be changed by tribes.

The model of growth used by Parker and Anderson is as follows:

$$\begin{aligned}
 \text{Indianpcigrowth} = & \beta_0 + \beta_1(\text{statejurisdiction}) + \beta_2(\text{beginningperiodincome}) \\
 & + \beta_3(\text{resourceendowments}) + \beta_4(\text{humancapital}) \\
 & + \beta_5(\text{economicconditionsinsurroundingcounties}) + \varepsilon
 \end{aligned}$$

Letting X denote the matrix of controls, they assume that $\text{Cov}[X, \varepsilon] = 0$ and that the $\text{Cov}[\text{statejurisdiction}, \varepsilon] = 0$, meaning that both the controls and court jurisdiction are exogenous to income growth. This assumption is most likely appropriate, since tribes did not chose state jurisdiction, nor were the tribes selected on the basis of potential economic growth. Reservation-level controls are used to account for differences in natural resource endowments, land tenure, levels of education, geographic remoteness, and economic conditions in counties surrounding the reservation.

Anderson and Parker conclude that “across all specifications, the effect of state jurisdiction on income growth is positive, statistically significant, and robust to the inclusion of control variables and to different definitions of state jurisdiction. The regression coefficients across the columns imply that state jurisdiction increased Indian per-capita incomes by at least 30 percent between 1969 and 1999.” Anderson and Parker also analyze the effects by decade, and find that state jurisdiction was shown to have its largest impact on Indian incomes in the decade period most immediately following the uniform implementation of PL 280.

Anderson and Parker provide a template for studying how a stable rule of law affects Indian reservation economies by focusing on per-capita income, an aggregate measure of economic activity. To more precisely analyze the causal pathways from institutions to growth, and shed light on what it would take to lift reservations out of poverty, Parker (2009) examines if (and why) state jurisdiction improved credit-related outcomes on reservations. Relying on microfinance literature that suggests access to credit is crucial for development, Parker focuses on how moving from tribal to state court jurisdiction over debt contracts affects the incentives of potential lenders and borrowers. Lending institutions are necessarily reluctant to make loans to Indian operators because foreclosure procedures may lie with tribal jurisdiction. Regression estimates of 1951-1970 data indicate that state jurisdiction increased

per-capita credit from customary sources to American Indians by at least 66 and 96 percent in the regions where PL 280 was enacted. Additionally, data from 2004-2006 home loans showed that the average per capita amount received by American Indians on reservations under state jurisdiction was 164 percent greater than the average amount received on reservations under tribal jurisdiction. Parker's conclusions are consistent with the theory that lending conditions are improved under state jurisdiction because creditors, usually non-Indians, are less certain about the enforcement of debt contracts under tribal law. Predictable legal institutions are critical to growth on reservations, and better access to credit is a key channel in this process.

III Canadian First Nations and the First Nations Land Management Act

Canadian Indian reservations provide a further evaluation of the effect of institutions on economic growth. In 1999, the Canadian federal government devised the First Nations Land Management Act (FNLMA). The FNLMA allows First Nations to develop and implement their own policies for administering their lands, based on their own rules and customs, whereas before the lands were managed by the Canadian federal government. Under the Canadian federal government, specifically the Indian Act (1876), all lands reserved for Indians are held by the Crown to be used by Indian bands to occupy and live on. In practice, the bands, through organized band councils¹, exercise primary usage and occupancy rights over all reserve lands (Alcantara and Flanagan, 2006).

Alcantara and Flanagan (2002; 2006) analyze the three different land titling policies used under the Indian Act. The majority of bands employ customary land rights, which usually exist in two forms. The first is when the band council allots individual tracts to band members who then occupy and use the lands. The second is when community members and/or the band council informally recognize one's traditional occupation of a tract of reserve land. Since customary rights have no statutory basis,

¹Band councils operate under the authority of the Indian Act Band councils and are similar to municipal governments. They are governed by a chief and council who are elected by band members through customary rules established by the bands themselves, or under the rules set out in s. 74 of the Indian Act. Band councils have a wide range of powers, and are able to control membership rules, economic development, zoning, housing, and the administration of reserve lands. Most band councils delegate the administration of their reserve lands to band council created land departments, although final authority remains with the band councils. (Alcantara, 2007)

each band develops their own customary rights regimes. Overall, most customary rights tend to be poorly documented and enforced. Canadian courts have declined to enforce customary property rights on reserves. Such property rights over land suffer from high transaction costs, limited security of tenure, and susceptibility to political favoritism.

The second form of land titling is the Certificate of Possession (CP). A CP is proof of lawful possession issued under the authority of the Indian Act by the Minister of Indian Affairs after approval by the band council (Alcantara and Flanagan, 2002). Once granted, a CP is a stronger form of property right than customary ownership. Land held by a CP can be subdivided, left to an heir, and sold to another person who has a right to reside on the reservation. Canadian courts will enforce the rights generated by a CP. The biggest limitation is that a CP can only be transferred within the band, making it harder for businesses and individuals to construct housing and other economic development projects on the reserve. Since the largest First Nation reserves are relatively small, real-estate markets on reserve tend to suffer from lack of potential buyers.

The final form of land titling is leases. Leases can be granted on the band's collective land as well as on any type of individually controlled reserve land. Short-term leases, called permits, suffer from the fact that the Minister of Indian Affairs must approve leases to non-Indians for residential purposes, even if the band council recognizes that such a lease would be beneficial. Long-term leases, referred to as designated lands, suffer from the fact that terms incorporated in the agreement may make the lease land considerably less valuable than its off-reserve equivalent. The restriction on sale and use inherent in reserve land, coupled with the power of the band council to levy property taxes and to pass by-laws such as zoning laws, greatly reduces the current value of designated land. Thus, the amount of rent collected for leased reserve land will be lower than what could be earned from the land if it were not on a reserve. Overall, titling on land in Canadian Indian reservations exhibits various forms of quasi ownership conferred by political authorities, where owners are frequently confused about their rights, and where rights can be withdrawn on short notice without compensation (Alcantara and Flanagan, 2006). This in effect reduces the economic profitability of the lands.

With a thorough understanding of the three types of land titling, it is now appropriate to return to the First Nations Land Management Act. For a land code to come into effect under the FNLMA, a First Nation must submit a developed and drafted land code to a jointly appointed verifier, negotiate a funding agreement with Indian and Northern Affairs Canada, and then hold a community vote on both the land code and the funding agreement. If it is approved by the community, the verifier

certifies the land code and the First Nation takes over all land management responsibilities from the Crown. When the land code takes effect, it becomes enforceable in Canadian courts (Alcantara, 2007). A completed land code will address a number of issues, including: “(a) the use and occupancy of First Nation’s land, including licenses, leases, and allotments under s. 20(1) of the Indian Act; (b) the transfer of land interests and the revenues from natural resources obtained from reserve land; (c) requirements for accountability to First Nation members for land management and moneys derived from reserve land; (d) community consultation processes for the development of rules respecting matrimonial property issues, use, occupation and possession of First Nation land and the division of interests in First Nation land; (e) publication of First Nation laws; (f) conflicts of interest in the management of First Nation land; (g) the establishment of a forum for the resolution of disputes in relation to interests in First Nation land; (h) granting or expropriating interests in First Nation land; (i) delegation by the council of its authority to manage land; (j) approvals of an exchange of First nation land; and (k) amending the land code” (Issac, 2005) According to the Framework Agreement on First Nations Land Management website, the supposed benefits of the FNLMA include more efficient management, recognition of the right to receive revenue from interests, protection against arbitrary expropriation, removal of the need to obtain Ministerial approval for First Nation law, and the ability to create a local dispute resolution process.

A preliminary analysis of the effects of the FNLMA has been carried out by Alcantara (2007). He argues that properly designed property rights can significantly improve economic development conditions on contemporary Indian reserves. Conversely, poorly designed property rights can create significant drag. Alcantara admits that, for the time he is researching, large-n and time series statistical data are unavailable to gauge the effects of the FNLMA. Alcantara pursues an empirical study, relying on a close textual analysis of the land codes and key informants published in secondary sources, for both the Mississaugas of Scugog Island in Ontario and the Muskoday First Nation in Saskatchewan.

The Scugog Island land code attempts to create a more formalized and efficient system of customary rights. All interests and licenses in Scugog Island lands must be documented and filed with the band before they can receive legal recognition and enforcement. Written documentation is also necessary for land transfers, wills, subdivision of land, and allotments of land (Alcantara, 2007). The holder of an allotment is entitled to the “exclusive use and occupancy of that lot for residential purposes,” while also being entitled to any resources or revenue arriving from the sale of resources on that lot. Additionally, the lot holder can transfer his land to any band member without the need for band council approval. The band council

cannot expropriate any customary allotment, whereas before the band council could cancel rights at any time. The Scugog Island land code eliminates the involvement of the federal government from all land transactions, allowing the band council to act quickly on business proposals. The administration of leases and CPs is much more efficient since the band no longer has to wait for applications to go through approval at the federal level.

The Muskoday First Nation has been less successful in formalizing and strengthening customary rights. Although the land code does require that all interests and licenses in Muskoday lands be documented in writing and registered with the band council, it does not specify what ownership rights an individual acquires when he is allotted a land title. The land title does not guarantee the member any benefits arising from the interest in his land. Moreover, the Muskoday land code requires the written consent of the council in order to transfer land. The band council can also expropriate reserve lands as long as the expropriation is for community purposes. The Muskoday land code, while reducing transaction costs by eliminating federal involvement, fails to strengthen individual property rights to land.

Minimally, a land code allows a First Nation to reduce transaction costs by eliminating the involvement of the federal government in administering lands. A First Nation can manage its lands according to local knowledge and needs, while avoiding the inefficiencies and inaccuracies generated by the involvement of a central authority (Alcantara 2007). However, there is no guarantee that First Nations will develop a land code that strengthens their individual property rights, since it is not required by the FNLMA.

IV Data

It is my goal to build on Alcantara's paper, and test the economic impact of the FNLMA using recently released census data. This examination is important for reasons outlined by Glaeser et al. (2004), in that it focuses on actual rules and not conceptually ambiguous assessments of institutional outcomes. Furthermore, it follows Pande and Udry's (2006) suggestion that a unit smaller than a country may provide a homogeneous environment for a certain institution, and therefore reveal more about the causal role associated with that institution. Canadian First Nations provide an opportunity for empirical research based on micro-data, making it easier to isolate the effects of specific institutions. My research will hopefully build on that provided by Parker (2009) and Anderson and Parker (2008) by providing an additional "natural experiment" for institutional testing.

As indicators to gauge economic growth, I will use median income, average earnings, average earnings for those working full time, unemployment, and net growth in dwelling units.² Although the Canadian census began collecting data aggregated by individual First Nations in 1995, the data measurements in 1995 are inconsistent with those in 2000 and 2005. The 1995 Census includes average income measurements for individual First Nations, whereas the 2000 and 2005 Census contain median income statistics. For this reason, income measurements from 1995 are not used. Additionally, the 1995 Census lacks any data on earnings aggregated by individual First Nations, so earnings data from only the 2000 and 2005 Census will be used. The 1995 Census is valuable, however, in calculating the net growth in dwelling units variable. This measurement is not provided directly in the Census, but can be calculated for the years 2000 and 2005 by taking the difference in dwelling units between 2000 and 1995, and again between 2005 and 2000.³ Overall, I assume these measurements to be an accurate representation of housing construction within each First Nation.

The treatment group, those eight First Nations adopting the FNLMA, will be compared to a control group. For each adopter, four control First Nations were selected based on similar population numbers and beginning period income, as well as geographic location.⁴ With the four original adopters, four later adopters, and 32 control First Nations, this will give me a total of 40 First Nations. Since I am collected data for two years, this will give me 80 total observations.⁵

The summary statistics for the treatment and control First Nations are provided in Table 3. The income and earnings statistics are quite similar for both groups. The mean median income for the treatment First Nations was \$13638, while it was \$12374 for the control First Nations. The standard deviation was similar, while the range for the control First Nations was slightly larger than the range for the treatment First Nations.⁶ The standard deviation for net growth in dwelling units was larger for the treatment First Nations, 44.865 as compared to 19.208 for the treatment First Nations. The range was also quite different between groups: 180 for the treatment

²See Table 1 for a list of variable descriptions.

³Note that the Canadian census rounds the number of dwelling units to the nearest 5.

⁴To control for geographic location, control First Nations within the same province as the First Nation adopting legislation were chosen.

⁵A list of treatment and control First Nations are presented in Table 2.

⁶The number of observations for net growth in dwelling units is 12 for the treatment First Nations because 2 treatment First Nations did not have data for dwelling units for all years. The number of observations for control First Nations is 62 because two control First Nations did not have dwelling units data for the year 1996. Unemployment data is missing for one control First Nation for the year 2000.

First Nations and 110 for the control First Nations. The mean unemployment rate was slightly higher for the control First Nations, and the maximum was higher as well.⁷

V Hypothesis

The null hypothesis is that those First Nations adopting the FNLMA have shown similar economic growth as compared to those First Nations who have not adopted the Act. The alternative hypothesis is that those First Nations adopting the FNLMA will show greater economic improvement as compared to those who did not adopt the Act. Based on the findings in previous literature, it is my expectation that the null hypothesis will be rejected in favor of the alternative hypothesis. Alcantara (2007) shows that, at a minimum, the FNLMA will reduce the inefficiencies associated with passing land titling through the federal government. At the local level, First Nations can respond more quickly in establishing land titles according to local knowledge and needs. Furthermore, First Nations that also establish secure property rights in their land codes will show an even greater economic improvement.⁸ My hypothesis is further backed by an extensive literature that shows the economic benefits of more secure and decentralized property rights (Boudreaux, 2005, 2006; Boudreaux and Aligica, 2007). Although such a hypothesis may seem paradoxical in light of the conclusions of Anderson and Parker (2008), I believe the land code satisfies the necessary conditions of a credible commitment. A land code creates a contract enforcement mechanism, as the land code is known and protected by the federal government. As such, outside investors may be more willing to invest on Canadian First Nations.

VI Research Design

The variable of interest, whether the First Nation adopted the legislation, will be a dummy variable. The control group will be labeled 0 for each of the two time periods. Two assumptions are used regarding the timing of the effects of the treatment group. For the level dependent variables, which include median income, average earnings,

⁷Unemployment data is missing for one control First Nation for the year 2000.

⁸A measurement of the security property rights may be gained from a close textual analysis of each First Nations land policy. However, such a measure would be quite difficult to derive quantitatively, and for this reason I have left it out of the analysis.

average earnings full time, and unemployment, it is assumed that effects of the legislation emerge 2 years after adoption.⁹ So, the First Nations adopting the Act in 1999 will be coded 0 for 2000, and 1 for 2005. The 2003 adopters will be labeled the same, 0 for 2000 and 1 for 2005.

The variable for net growth in dwelling units allows a different assumption. Housing starts in year t can be represented by:

$$\alpha + \beta T_t$$

where T stands for the treatment dummy variable and will be coded 0 or 1. Total new housing starts in a 5 year period (the length of time between Censuses) can be represented by:

$$5\alpha + \beta \sum_{i=1}^5 T_i = 5\alpha + \beta Y$$

where Y is the number of years the reserve is treated. Alternatively, this can be rewritten as:

$$5 \left(\alpha + \beta \frac{Y}{5} \right) = 5\alpha + \beta S$$

where in the final equation 5α is a constant and S represents the share of years that the First Nation is treated. Under the assumption that there is no lag in the effect of the legislation, the adopters in 1999 would be coded 1/5 for the 1995-2000 census period, and 5/5 for the 2000-2005 census period. Similarly, S would equal 0 for the 2003 adopters in the 1995-2000 period, and equal 2/5 for the 2000-2005 period.¹⁰

To control for variables specific to individual First Nations, which may include beginning per-capita income, economic conditions in the surrounding community, and human and physical capital endowments, a fixed effects model will be used. The generalized fixed effects model controlling for changes between time periods is:

$$Y_{it} = a + bx_{it} + c_i + u_{it}$$

⁹This is done both on theoretical and practical grounds. Theoretically, information concerning the change in land policies will take some time to reach potential investors. Two years is a reasonable lag, as presumably after 2 years both individuals within the First Nation and outside investors will have knowledge of the new policy, and change their behavior accordingly. Practically, this assumption will allow both the 1999 and 2003 adopters to be considered as treated in the 2005 census. For robustness, assumptions will also be tested when the lag is one year and when it is between three and six years.

¹⁰For consistency, I will also examine net growth in dwelling units under the assumptions that effects emerge immediately after one year, two years, and between three to six years.

Y_{it} will be one of our dependent variables (median income, average earnings, average earnings for those working full time, net growth in dwelling units, or unemployment), where i signifies each individual First Nation, and t signifies the time period (2000 or 2005). x_{it} is the treatment dummy variable for First Nation i at time t , and c_i is the fixed effect for each First Nation i . d_t will be a dummy variable for time, in our case d2005, which will equal one for 2005 and zero otherwise. The fixed effects estimator is unbiased under a strict exogeneity assumption on the explanatory variables (Woolridge, 2009).

In analyzing the FNLMA as a “natural experiment” similar to what occurred to American Indian reservations under PL 280, the Canadian experience suffers from the fact that First Nations actively chose to enact the legislation, rather than having it randomly forced upon them. In this sense, such legislation is endogenous rather than exogenous. However, by using reservation fixed effects, time-invariant heterogeneity can be removed from the estimates.

There are a few problems in the research design that should be addressed. First, the time lapse between when the land code was enacted (1999) and the last census data (2005) is quite short. It may be the case that any effects as a result of the new land code will not have come to fruition, as outside businesses and individuals have yet to recognize a more secure property rights environment. However, I think there are few reasons why the current design is justifiable. First, I feel the economic effects from eliminating the federal government from the land process will be felt almost immediately. Business deals can progress much more rapidly, so economic changes should be readily apparent. Furthermore, in their study of PL 280 on American Indian reservations, Anderson and Parker found state jurisdiction had its largest impact in the decade immediately following implementation. The effects of P.L. 280 were greatest when it was first implemented, suggesting that credible commitments are quickly integrated into the institutional environment. When new investment opportunities appear on reservations, a stable contracting environment with strong property rights allows Indians to better capitalize on those opportunities (Anderson and Parker, 2008).

VII Results

The results from two OLS regression estimations, one using only 1999 adopters and their controls, and one using the 1999 adopters and controls and the 2003 adopters

and controls, can be seen in Tables 4 and 5.¹¹ The results for the two regressions are quite similar. The adoption of the FNLMA has positive, economically significant effects on median income, average earnings, and net growth in dwelling units. For the 1999 adopters and controls, the estimated effects of the FNLMA on average earnings for full time workers and unemployment is negative, while it is positive when the 2003 adopters and controls are included. However, the estimated effects of the FNLMA are insignificant at the 5% level for median income, average earnings, average earnings for those working full time, and unemployment.

In both regressions, the coefficient on FNLMA is statistically significant at the 5% level when the outcome measure is net growth in dwelling units. In the second regression, the estimator predicts that, *ceteris paribus*, First Nations adopting legislation will have on average about 56 more newly built dwelling units as compared to non-adopters. The fact that construction shows the greatest statistical significance may be indicative of how it responds to changes in property rights and contracts. If property rights and contracts are more secure, construction may be the first variable we see increasing. There may be a greater lag on income, earnings, and unemployment, and thus with only six year and two year differences between when the Act was adopted and the latest Census, we may see no statistically significant change yet in those variables.¹²

In the first regression, the estimated effects of the FNLMA on average earnings for those working full time is negative. This means that First Nations adopting the FNLMA have lower average incomes when working full time as compared to those not adopting the act. However, when the dependent variable is the log of average earnings for those who worked full time, the OLS estimator drops to an economically insignificant -0.072. The R squared term is about the same, 0.773 in the log form and 0.771 in the linear form. Therefore, the negative coefficient on average earnings for those working full time may not be important, as it is economically insignificant when the log of the independent variable is used instead. The negative coefficient on

¹¹The results of OLS regression under the assumption of a one year lag are presented in Table 6, and under the assumption of a three to six year lag (since a three year, four year, five year, and six year lag would be coded identically in our equation) in Table 7. The OLS regression under the assumption of two year lag when net growth in housing units is the dependent variable is presented in Table 8.

¹²However, when we regress treatment on the log of new dwelling units (to give it a more normal distribution by eliminating some of the skewness), any statistical significance is lost. For the second regression, the t-value drops to 0.10, and the p-value increases from .022 to 0.92. The R2 term has also increased, from 0.5629 to 0.7546, showing that the log form better fits the data.

unemployment in both regressions is as expected: those adopting legislation would show a decrease in the unemployment rate.

By examining the confidence intervals for the regressions, we may better understand the impact of the legislation. Using the second regression, it can be predicted with 95% confidence that the Act will not decrease median income by more than \$1264, nor increase median income by more than \$3432. As the average median income for all First Nations was \$12,627, this predicts that with the legislation median income would not decrease below \$11,363 nor increase above \$16,059. The 95% confidence interval for the net growth in dwelling units is between 7.97 and 96.22, predicting that First Nations adopting legislation will show a growth in dwelling units between 21.35 and 109.60¹³. The 95% confidence interval for log of net dwelling units is between -1.44 and 1.59. As the average log of net growth in dwelling units was 2.85, it is predicted that after the Act is passed the log of the net growth in dwelling units would be between 1.41 and 4.44.

VIII Conclusions

The results of my regressions dictate that, with the possible exception of new residential construction, I accept the null hypothesis, that First Nations adopting legislation have shown no greater economic growth as compared to First Nations not adopting legislation. However, I believe that my data suffers from defects acknowledged by Alcantara (2007): a small number of observations and limited time series data. Although the conclusion at this point is that adopting the FNLMA will show no greater economic improvement, it may still be considered a good idea for empirical reasons observed by Alcantara (2007). In creating their own land codes, First Nations have the ability to strengthen property rights and reduce transaction costs. Additionally, this paper has shown that the FNLMA was not a complete disaster. First Nations adopting the Act did not show any significant decreases in the dependent variables as compared to those First Nations who did not adopt the act. Further analysis using the forthcoming 2010 Canadian Census may allow us to reject the null hypothesis, and support the empirical study carried out by Alcantara.

¹³The average net growth in dwelling units for both treatment and control First Nations is 13.38.

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Table 1 – Treatment and Control Tribes

Treatment Tribe	Control Tribes
Tsinstikeptum 9, British Columbia (1999)	Bella Bella 1 (B.C.) Campbell River 11 Chehalis 5 Gitanmaax 1
Muskoday, Saskatchewan (1999)	Canoe Lake 165 Buffalo River Dene Nation 193 Cowessess 73 Muskeg Lake Cree Nation 102
Opaskwayak Cree 21E, Manitoba (1999)	Shamattawa 1 (MB) Bloodvein 12 Chemawawin 2 Ebb and Flow 52
Nipissing, Ontario (1999)	Aroland 83 Curve Lake First Nation 35 Whitefish Lake 6 Serpent River 7
Tsaout, East Saanich 2, British Columbia (2003)*	Gitanyow 1 Woyenne 27 Coldwater 1 Nak'azdli (Necoslie 1)
Kinistin, Saskatchewan (2003)	Witchehan Lake 117 Chitek Lake 191 Carrot River 29A Moosomin 112B
Garden River, Ontario (2003)*	Aroland 83 Christian Island 30 Wapekeka 2 Sagamok
Whitefish Lake, Ontario (2003)	Alderville First Nation Pikwakanagan (Golden Lake 39)

* No data for *net growth in dwelling units* for both 2000 and 2005

Table 2 – Variable Descriptions

variable	description
treatment	Dummy variable for the adoption of the FNLMA
d2005	Dummy variable for year
tribeid	Number assigned to each tribe (ranging between 1 and 40)
income	Median income, for persons 15 years and older with income
avg_earnings	Average earnings, for population 15 years and over
lavg_earnings	log of average earnings
avg_earningsfulltime	Average earnings, for population 15 years and over who worked full year, full time
netdwellingunits	Net growth in dwelling units
lnewdwellingunits	log of newly constructed dwelling units
unemployment	Unemployment rate - refers to the unemployed expressed as a percentage of the labor force

Table 3 – Summary Statistics

Variable		Obs	Mean	Std. Dev.	Min	Max
Median income	treatment	16	13637.65	4150.17	6568.00	20902.27
	control	64	12374.49	3844.21	4153.43	23136.65
Average Earnings	treatment	16	20572.26	3956.18	12670.88	26461.2
	control	64	19366.74	3316.33	12807.15	28207
Average Earnings Full time	treatment	16	32925.19	5457.78	23698.63	42462.25
	control	64	31214.07	4519.03	21598.00	44184.00
Net dwelling units	treatment	12	14.167	44.865	-100	80
	control	62	13.226	19.208	-55	55
Unemployment Rate (%)	treatment	15	20.02	10.21	7.7	38.9
	control	64	25.4125	9.448784	7.5	51.7

Table 4: Including 1999 adopters and controls

VARIABLES	(1) Net Dwelling Units	(2) log Net Dwelling Units	(3) Median Income	(4) Average Earnings	(5) Average Earnings Full Time	(6) log Average Earnings Full Time	(7) Unemployment Rate (%)
treatment	56.250** (21.061)	-0.062 (0.512)	930.513 (1,508.520)	964.628 (1,751.917)	-2,139.270 (2,344.516)	-0.072 (0.091)	-2.462 (3.973)
d2005	-23.750** (9.419)	-0.520** (0.209)	-1,343.524* (674.631)	-1,129.770 (783.481)	277.438 (1,048.499)	0.010 (0.033)	-1.512 (1.777)
Constant	20.000*** (5.957)	3.354*** (0.120)	13,938.921* (426.674) **	20,395.144*** (495.517)	32,199.466*** (663.129)	10.373*** (0.021)	24.930*** (1.124)
Observations	40	30	40	40	40	40	40
R-squared	0.669	0.817	0.842	0.755	0.773	0.771	0.860

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Including both 1999 adopters and controls and 2003 adopters and controls

VARIABLES	(1) Net Growth Dwelling Units	(1) Log Net Growth Dwelling Units	(3) Median Income	(4) Average Earnings	(5) Average Earnings Full Time	(6) Unemployment Rate (%)
treatment	52.099** (21.712)	0.074 (0.729)	1,083.735 (1,160.063)	505.646 (1,402.640)	152.950 (1,783.288)	-2.332 (3.064)
d2005	-10.511* (6.141)	-0.254 (0.192)	-1,362.040** (518.796)	-514.165 (627.280)	-225.821 (797.511)	-1.625 (1.298)
Constant	14.833*** (4.041)	2.972*** (0.117)	13,199.768* ** (328.115)	19,814.361*** (396.726)	31,653.906*** (504.390)	25.397*** (0.824)
Observations	74	58	80	80	80	79
R-squared	0.563	0.755	0.865	0.747	0.782	0.866

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Assumption of a one year lag (using 1999 adopters and controls and 2003 adopters and controls)

VARIABLES	(1) Net Growth Dwelling Units	(2) Log Net Growth Dwelling Units	(3) Median Income	(4) Average Earnings	(5) Average Earnings Full Time	(6) Unemployment Rate (%)
treatment	10.294 (26.602)	0.872 (0.597)	1,112.994 (1,553.958)	623.489 (1,870.649)	2,123.689 (2,352.859)	-2.047 (4.434)
d2005	-5.294 (6.270)	-0.322* (0.176)	-1,256.592** (491.405)	-475.385 (591.551)	-407.600 (744.039)	-1.886 (1.230)
Constant	14.706*** (5.183)	2.905*** (0.120)	13,088.468*** (364.435)	19,752.012*** (438.706)	31,441.537*** (551.794)	25.605*** (0.942)
Observations	74	58	80	80	80	79
R-squared	0.491	0.777	0.863	0.747	0.786	0.865

Table 7: Assumption of a three to six year lag (using 1999 adopters and controls and 2003 adopters and controls)

VARIABLES	(1) Net Growth Dwelling Units	(2) Log Net Growth Dwelling Units	(3) Median Income	(4) Average Earnings	(5) Average Earnings Full Time	(6) Unemployment Rate (%)
treatment	41.875** (18.056)	-0.367 (0.622)	813.647 (1,558.834)	275.437 (1,872.849)	-1,851.779 (2,358.897)	-2.152 (3.890)
d2005	-9.375 (6.019)	-0.215 (0.183)	-1,226.658** (492.947)	-440.580 (592.247)	-10.053 (745.949)	-1.823 (1.246)
Constant	15.929*** (4.041)	2.977*** (0.115)	13,199.768*** (330.679)	19,814.361*** (397.291)	31,653.906*** (500.398)	25.397*** (0.827)
Observations	74	58	80	80	80	79
R-squared	0.559	0.758	0.863	0.746	0.785	0.865

Table 8: Assumption of a two year lag (using 1999 adopters and controls and 2003 adopters and controls)

VARIABLES	(1) Net Growth Dwelling Units	(2) Log Net Growth Dwelling Units
treatment	33.667** (15.335)	0.279 (0.462)
d2005	-10.333 (6.261)	-0.295 (0.193)
Constant	15.955*** (4.070)	2.973*** (0.115)
Observations	74	58
R-squared	0.552	0.759