Do better property rights improve local income?: Evidence from First Nations' treaties

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Abstract

This paper examines the effect of an improvement in property rights on a local economy using the case of First Nations' modern treaties. These treaties are an important institutional reform that clarifies ownership of land and natural resources near Aboriginal communities. Using confidential micro-data, I find evidence of a positive impact of modern treaties on real income. The effect is driven by employment income and spreads across workers in industries not directly affected by the reform. I also find an increase in real wages and housing costs. The effects are similar in neighboring communities outside Indian reserves. These results are consistent with property right reforms creating a positive demand shock that affects the whole local economy. This is a yet understudied mechanism through which better property rights can generate positive local spillovers.

Keywords: property rights, institutions, local labor markets, First Nations, treaties.

1 Introduction

Well-defined property rights are considered an important element of economic development (North, 1990; Besley and Ghatak, 2010). Cross-country studies link better property right institutions to higher national income and economic growth (Acemoglu et al., 2001; Acemoglu and Johnson, 2005). Using within-country variation, several empirical papers also find evidence of a positive effect of property rights on investment, and other economic outcomes, such as labor supply, agricultural productivity, and land use (Lin, 1992; Besley, 1995; Johnson et al., 2002; Banerjee and Iyer, 2005; Field, 2007; Goldstein and Udry, 2008; Hornbeck, 2010; Galiani and Schargrodsky, 2010).[1] Not surprisingly, there have been several policy initiatives aimed to reform, and improve, property rights.

In practice, property right reforms usually target some specific local population.[2] At local level, however, it is not clear whether, or how, improvements in property rights ultimately affect income and living standards. Do the increase in investment or economic activity associated to better property rights translate into higher real income?, are these benefits limited to a specific population, such as land-owners, or do they extend to the rest of the local economy?. The answers to these questions are not straightforward due to possible general equilibrium effects associated to property right reforms, such as increase in demand for local labor, migration, and change in local prices.

This paper examines these questions using the case of First Nations' modern treaties.[3] These treaties have had a profound effect on defining ownership over land and natural resources near Aboriginal communities. While formally owned by the government, some of this land is subject to collective rights held by Aboriginal communities.[4] In many cases, however, neither the scope of these rights nor the territory involved is defined. This feature creates lack of clarity about ownership over vast tracts of land rich in natural resources.

Modern treaties clarify property rights over these lands and resources. They do so in several ways. First, they delimit the territory subject to Aboriginal rights. Second, they specify

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[1]Recent work also explores the mechanisms linking property rights and investment, such as access to credit, reduction of expropriation risk, or facilitation of trade (Besley and Ghatak, 2010; Besley et al., 2012).

[2]For example, in Mexico, the 1992 land certification program targeted only people living in *ejidos* (de Janvry et al., 2013). Similarly, Operation Barga in West Bengal improved security of tenure mostly among rural farmers (Banerjee et al., 2002).

[3]The term First Nations refers to the largest Aboriginal group in Canada. The other two are: Métis and Inuit.

[4]These rights, called Aboriginal rights, arise from the traditional use and ancestral occupation of land.

who owns land and natural resources, and describe in detail how these rights will be exercised. Finally, they clarify the scope of Aboriginal rights to harvest wildlife and use land for traditional purposes. Note that treaties' main objective is to clarify property rights in dispute, not to reduce inequality in land redistribution. In that sense, they are not similar to standard land reforms.[5] By clarifying property rights, however, modern treaties have the potential to reduce transaction costs, especially for extractive activities.[6] In turn, this can facilitate economic transactions and affect local economic conditions.

In order to examine the economic impact of modern treaties, I use confidential Census micro-data of individuals living on reserves held by First Nation bands.[7] The richness of the data allows me to observe key economic variables (such as employment and household income). and to construct indexes of local prices. This is crucial to obtain measures of real income and real wages. The main empirical challenge is dealing with omitted variables that may affect both economic outcomes and treaty making. To address this identification concern, I use a difference in difference approach exploiting the timing of treaty implementation. This quasi-experimental approach effectively compares the evolution of outcomes in reserves held by treaty bands, relative to non-treaty bands, before and after a treaty was implemented.

I find evidence that modern treaties increase real household income by around 9%. Results are robust to several specifications, such as inclusion of band fixed effects, and time-varying band-level controls. I also find similar results using as a control group First Nation bands that started, but not yet completed, treaty negotiations. This group is likely to be more comparable to treaty-implementing bands.

Having established this result, I examine alternative explanations for the increase in real income. There are two main candidates: (1) changes in population composition due, for instance, to selective migration, and (2) confounding institutional changes associated to treaties, such as expansion of the public sector, financial compensation, or implementation of self-government agreements. The evidence suggests, however, that these two explanations are unlikely to fully

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[5]See Besley and Burgess (2000) and Ghatak and Roy (2007) for a review of the effects of redistributive land reforms on poverty and agricultural productivity.

[6]In this context, the main transaction costs faced by extractive firms, such as mines, is associated to obtaining an operating license. This requires public consultation with local communities, and plans to mitigate or compensate parties whose rights are affected. Lack of clarity of ownership and the extent of these rights makes this process more cumbersome.

[7]First Nation communities are officially referred to as bands. A band usually has lands set apart for its own use and benefit, called Indian reserves. Reserves are similar to U.S. Indian reservations.

explain the observed effects. There is no significant change in observable population characteristics, such as age, migration history, or education. There is also no increase in real income of public workers, nor differences in the effect of treaties that did not implemented self-government agreements. I also find that most of the increase is not driven by government transfers, but by employment income.

An important question is: why would employment income increase? To answer this question, I analyze treaties as a reduction in transaction costs to develop extractive activities. This is a reasonable starting point given the role of treaties on clarifying property rights over land and natural resources, and the need of consulting with local communities before starting new projects in Aboriginal lands. In this view, treaties have the potential to facilitate new extractive operations and increase the demand for local labor. In order to analyze the equilibrium effects of a local demand shock, I use the analytical framework developed by Moretti (2011). In this framework, a positive shock to the demand for local labor has a first order effect of increasing wages in the affected sector. There are, however, other general equilibrium effects. First, to the extent that workers are mobile between industries, the increase on wages would spread to other workers. Second, the increase in the local budget constraint would also increase the demand, and price, of non-tradable goods, such as housing. Finally, in the presence of imperfect labor mobility, we could expect a positive effect on workers' real income.[8]

With this framework in mind, I explore the effects of treaty implementation on the local economy. First, I find a positive relation between treaty implementation and mining agreements. These are private contracts between mining firms and Aboriginal communities in order to start new mining operations. I interpret this finding as evidnece that treaties have indeed reduce transaction costs. Second, I find that real income increases for workers in extractive industries but also for workers in manufacturing, trade and non-public services. Third, consistent with a local shock on labor demand in presence of inelastic labor and housing supply, I also find an increase in real wages, and house prices.[9] Finally, I present evidence of geographical spillovers. In particular, I document an increase in real income and real wages in neighboring, off-reserve, communities within commuting distance of bands that implemented a treaty.

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[8]If labor were perfectly mobile, as in the Rosen-Roback model, then immigration and the increase in housing prices would offset the increase in wages. This would keep real wages (and workers' real income) similar between locations.

[9]Similar response of local economies to demand shocks has been documented in previous studies such as Greenstone and Moretti (2003) and Aragón and Rud (2013).

This paper relates to a literature studying the economic effects of property rights institutions. The contribution to this literature is twofold. First, this paper documents the effect of better property rights on real income. This outcome has been neglected in previous studies using within-country variation. Importantly, the richness of the data allows the construction of local price indexes. This is important in order to account for the change in cost of living associated to local shocks. Second, this paper highlights a potential local spillover associated to better property rights. By fostering investment or facilitating economic transactions, better property rights can create a positive demand shock in a local economy. This can spread the benefits, in terms of real income, to individuals not directly linked to the assets whose property rights have improved. This result depends, however, on the elasticity of labor and housing supply.

This paper also relates to a literature studying the causes of economic underperformance of Aboriginal peoples. This literature, mostly using the case of North American aboriginals, emphasizes the importance of institutions, governance and property rights (Cornell and Kalt, eds, 1992; Alcantara, 2007b; Anderson and Parker, 2008; Anderson and Parker, 2009; Akee, 2009; Akee et al., 2012). There is also evidence of the importance of other factors such as forced political integration (Dippel, 2013), cultural assimilation (Kuhn and Sweetman, 2002) or demand shocks from new industries, such as casinos (Evans and Topoleski, 2002). Recent work also explores the long term effect of assimilation policies, such as Indian residential schools, on cultural and economic integration (Feir, 2013). This paper contributes to this literature by examining the economic effects of a large, and on-going, institutional reform in Aboriginal communities.

2   Background

2.1   First Nations, Aboriginal rights, and modern treaties

First Nations are the largest group of Aboriginal people in Canada.[10] As of 2006, there were around 1.17 million people identified as Aboriginals, of which around 60% consider themselves as First Nations (Statistics Canada, 2010). First Nation communities, officially referred to as bands, have lands set apart for their collective use or benefit. These lands, called "reserves",

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[10]The Aboriginal people in Canada are classified in three groups: First Nations, Métis and Inuits. In Canada, the term First Nations refers to indigenous Indians (AANDC, 2010).

are formally owned by the Crown and are held in trust for bands by the federal government. Around 40% of First Nations peoples live on reserves.[11]

Besides reserve lands, First Nation bands also hold title, and the rights that go with it, over vast tracts of land and resources outside their reserves.[12] These rights, called *Aboriginal rights*, are enshrined by the 1982 Canadian Constitution and derive from the historic occupation and use of ancestral lands by Aboriginal people. These rights exists whether there is a treaty or not.[13] But, without a treaty there is uncertainty about how and where these rights apply (BC Treaty Commission, 2012). For instance, without treaties, it is not clear what is the territory of Aboriginal lands, or even who is the owner since there are cases of overlapping claims between Federal Government and Aboriginal communities, and also between several Aboriginal communities. Similarly, it is not clear which specific rights of use or wildlife harvesting the community may hold.

Modern treaties, also called Comprehensive Land Claim Settlements, address this issue by clarifying property rights in Aboriginal lands. They do so in several ways.[14] First, they delimit the territory subject to the Aboriginal rights of a First Nation band. Second, they recognize and specify the property rights to the land and natural resources of the involved parties, and describe in detail how these rights will be exercised. Finally, they define the scope of Aboriginal rights to harvest wildlife and use land for traditional purposes.

This clarification of property rights has the potential to reduce transaction costs for development of extractive industries, such as mining, in the vicinity of First Nation communities. These transaction costs arise due to the need to consult with Aboriginal communities before starting any project that may affect their rights. Since 1990, a series of Supreme Court decisions have clarified the nature of Aboriginal Rights and outlined the requirements with regards to consulting Aboriginal populations.[15] In practice, this has lead to an increased need to consult with Aboriginal communities before starting new projects on or near their Aboriginal lands,

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[11]First Nations peoples living on reserves are one of the poorest groups in Canadian society. (Royal Commission, 1996; AANDC, 2004). This situation is similar to the economic under-performance of U.S. Native Americans living on reservations.

[12]Similar Aboriginal title is recognized in Australia, New Zealand and U.S.

[13]Section 35 of the 1982 Canadian Constitution states that "(1) The existing aboriginal and treaty rights of the aboriginal peoples of Canada are hereby recognized and affirmed. (3) For greater certainty, in subsection (1) "treaty rights includes rights that now exist by way of land claims agreements or may be so acquired."

[14]See AANDC (2009) for a profile of four typical modern treaties. The full text of all treaties is available at http://www.aadnc-aandc.gc.ca/eng/1100100030583/1100100030584 (retrieved on November 20, 2012).

[15]Some of these decisions are R. v. Sparrow (1990), R. v. Gladstone (1996) and Delgamuukw v. British Columbia (1991).

and to arrange mitigation or compensation actions.

This consultation process is likely to be more cumbersome without clarity of who owns the rights over land and resources, and the scope of these rights. While I cannot measure transaction costs directly, latter I show that contracts between mining companies and Aboriginal communities are positively affected by treaties. This is suggestive evidence that treaties may have indeed lowered transaction costs.

In addition to clarifying property rights over Aboriginal lands, treaties may also introduce other institutional reforms. First, some treaties eliminate Indian reserves, and transfer title of the land in fee simple to the tribal council.[16] This feature, however, is not widely spread. In the sample, only one third of treaties include provisions to eliminate reserves. Moreover, this feature of treaties was not implemented during the period of analysis.[17] Second, modern treaties also involve transfer of funds from the federal government to the signatories, and devolution of land and resource management responsibilities to the band, such as management of land and natural resources. Finally, many treaties also include self-government provisions that devolve First Nations jurisdiction over their own affairs. These are important confounding factors that may affect the interpretation of results. I discuss these issues in more detail in Section 5.

There is not a comprehensive evaluation of the fulfillment of treaty terms. Some reports (based on few case studies) suggest that they have been successfully implemented, at least in terms of transfer of funds, establishment of public bodies, and recognition of rights to land (AANDC, 2009, p. i). However, other reports suggest that implementation is not exempt of difficulties and in some cases the government may have not fulfilled its treaty obligations (Auditor General of Canada, 2007; Standing Senate Committee on Aboriginal Peoples, n.d.). Some issues raised in these reports refer to: discrepancies in interpretation of implementation plans, reluctance of federal agencies to refer matters to arbitration, difficult coordination, and lack of funding of the implementation process. Note that limited implementation of treaties may create an attenuation bias and thus make the estimates more conservative. Similarly, lengthy

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[16]There are other on-going institutional reforms partially addressing the issue of limited property rights on reserves, such as the "lawful possession" tenure system. This use of this system, however, is limited: only around 2.9% of reserve land is held as a lawful possession (Brinkhurst and Kessler, 2013). Similarly, recent institutional changes, such as bilateral agreements and the First Nations Land Management Act, offer alternative arrangements to First Nation communities to secure property rights over their Aboriginal and reserve land, respectively (Alcantara, 2008) .

[17]The first transfer of former reserve lands to private individuals (in fee simple) happened in the Nisga'a Nation in 2013 (CBC, 2013).

implementation process may create lags between the beginning of treaty implementation and its economic effects. I present evidence of these lags in Section 4.2.

There were previous treaties between Aboriginal peoples and either the British Crown or the Government of Canada. These treaties, called historic treaties, were signed between 1701 and 1923, and addressed similar issues as modern treaties, such as ownership of land and resources, and financial compensation. Treaty making, however, stopped in 1927 when the federal government made it a criminal offense for a First Nation to hire a lawyer to pursue land claims.[18] Negotiation of modern treaties re-started in 1973, after the Supreme Court recognized the existence of Aboriginal rights.

As of 2012, there were 26 modern treaties ratified and implemented. These treaties cover around 50 percent of Canada's land mass and involve 96 Aboriginal communities (AANDC, 2012). Treaty making is still politically relevant, especially in resource-rich provinces where uncertainty over ownership of land and resources is hindering the development of extractive industries. In British Columbia, for instance, there are currently 111 First Nations bands, or 70% of its Aboriginal people, participating in the treaty process.

2.2 Analytical framework

Based on the previous discussion, the improvement in property rights associated to treaties can be analyzed as a reduction in transaction costs for extractive industries. This is an example of the Coase theorem: well-defined property rights can facilitate market transactions and improve economic efficiency. In this case, clarification of property rights may reduce the costs associated with public consultation, and facilitate the development of new extractive operations, such as mines.

A possible first order effect of the expansion of extractive activities is an increase in the demand for local labor. There might be, however, general equilibrium effects that would transmit the benefits to the rest of the community, even if they were not directly engaged in extractive industries. What are the general equilibrium effects of this shock on demand for local labor? A suitable analytical framework for studying these shocks to local labor markets, and how they propagate to the rest of the economy, is provided by Moretti (2011).

In this framework, there are competitive local economies that use labor to produce a tradable

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[18]This restriction on land claims was eventually lifted in 1951.

good. Workers are mobile so, in equilibrium, workers must be indifferent between different locations. They have, however, heterogeneous preferences over locations. These preferences define the degree of labor mobility and the supply of labor in a given location.[19] There is also a housing market. Demand for housing depends on city size, while its supply is exogenously given by geography and land use regulations.

The direct effect of the labor demand shock is to increase wages in the affected industries, i.e., extractive industries. To the extent that workers are substitutable between industries, this initial shock would also increase the wage of workers in the rest of the local economy. The increase in the local budget would increase the demand, and price, of housing and other non-tradable goods. In turn, these price changes would partially offset the increase in nominal wages.[20] Given an inelastic housing supply, the final effect on real wages depends on the degree of labor mobility. If workers were perfectly mobile (i.e., perfectly elastic supply), then real wages would not change. In contrast, if workers were less mobile (i.e., inelastic supply), the initial shock of demand would translate into an increase in real wages, and worker's real income.

In the case of First Nation communities, a plausible assumption is that labor supply is far from being perfectly elastic.[21] This is due to the membership requirements to access band services (such as tribal housing) and transfer property, which may deter immigration of non-Indian population. For that reason, we could expect that the improvement in property rights over land and resources associated to modern treaties would have a positive effect on house prices, wages, and workers' real income. This increase in income would benefit workers in extractive industries, but could also spread to other workers participating in the local labor market.

This conceptual discussion provides some guidance about the direct and indirect effects of a reform of property rights on a local economy. I explore these empirical predictions in detail in Section 6.

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[19]If workers are indifferent between locations they become perfectly mobile. In contrast, if preferences over location are important, then workers would be less willing to move to arbitrage away real wage differences. In this latter case the supply of labor would be upward sloping.

[20]Note that the increase in house prices depend on the assumption of a non-perfectly elastic supply of housing supply.

[21]In Section 6.3, I examine empirically the validity of this assumption.

3 Methods

3.1 Data

The empirical analysis uses data on modern First Nations treaties, collected from the Aboriginal Affairs and Northern Development Canada (AANDC), and confidential micro-data from the long-form Canadian Census.

Modern treaties   I focus only on modern treaties that: (1) were implemented by First Nations bands, (2) involved land claims, and (3) whose implementation started between 1973 and 2005.[22] This excludes treaties signed by other Aboriginal groups, such as Métis and Inuit, two treaties that only dealt with self-government issues[23], and treaties signed recently, such as the ones signed by Tswaassen and Malnuuth First Nations. For each treaty, I obtain the list of signatory bands and the year when the treaty implementation started.[24]

Table 1 presents the list of treaties used in this study, and some of their characteristics, such as whether they include provision for self-government or eliminate Indian reserves. The list includes 16 modern treaties implemented by 36 First Nation Bands. There are three observations relevant for the empirical analysis. First, all treaties were implemented by bands located in the following provinces and territories: British Columbia (BC), Quebec (QC), Northwest Territories (NT) and Yukon (YK). For this reason, in the baseline regressions, I restrict the sample to bands in these areas. Second, the earliest treaty was implemented in 1990. Since then, there has been an increasing number of bands implementing treaties. I exploit this differential timing of treaty implementation in the identification strategy. Finally, not all treaties include self-government provisions or eliminate Indian reserves. I later use this cross-sectional variation to explore the importance of these treaty features as alternative explanations.

I also obtain information of treaties that, during the period of analysis, were still under negotiation, i.e., without an implementation plan. In the period of study, there were 20 treaties

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[22]This is due to the availability of Census data from 1991 to 2006 only.

[23]These are the treaties signed by the Sechelt and Westbank First Nations.

[24]There are five main stages in the process of treaty negotiation. First, parties commit to the treaty negotiations signing a "Memorandum of Understanding". Second, parties agreed on a "Framework Agreement" that defines the issues to be discussed. Third, parties reach an "Agreement-inPrinciple" that contains all the major elements of the final agreement, but it is not legally enforceable. Third, parties ratify the "Final Agreement". This is the main outcome of treaty negotiations. Finally, parties need to agree to an "Implementation Plan". This plan is a crucial element of the Final Agreement since it specifies what must be done to put the agreement in effect, assign parties responsible for each implementation activity, and describes when and how these activities will be done. All the information about treaties is available at AANDC (2013).

under negotiation, involving 99 First Nations bands. Later, I use this information to perform a robustness check that uses as a control group bands that started, but not yet completed, treaty negotiations (see column 5 in Table C.1).

Table 1: List of implemented modern treaties

Nr.	Treaty name	Prov./ Territ.	Year implemen- tation plan	Self- gov.	Elim. Indian reserves
1	James Bay and Northern Quebec Agree- ment and the North Eastern Quebec Agreement	QC	1990	Yes	Yes
2	Gwich'in Comprehensive Land Claim Agreement	NT	1992	No	Yes
3	Sahtu Dene and Metis Comprehensive Land Claim Agreement	NT	1994	No	Yes
4	First Nation of Nacho Nyak Dun Final Agreement	YK	1995	Yes	No
5	Vuntut Gwitchin First Nation Final Agreement	YK	1995	Yes	No
6	Teslin Tlingit Council Final Agreement	YK	1995	Yes	No
7	Champagne and Aishihik First Nations Final Agreement	YK	1995	Yes	No
8	Little Salmon/Carmacks First Nation Fi- nal Agreement	YK	1997	Yes	No
9	Selkirk First Nation Final Agreement	YK	1997	Yes	No
10	Tr'ondëk Hwëch'in Final Agreement	YK	1998	Yes	No
11	Nisga'a Final Agreement	BC	2000	Yes	Yes
12	Ta'an Kwach'an First Nation Final Agreement	YK	2002	Yes	No
13	Tlicho Agreement	NT	2003	Yes	Yes
14	Kluane First Nation Final Agreement	YK	2003	Yes	No
15	Carcross/Tagish First Nation Final Agreement	YK	2005	Yes	No
16	Kwanlin Dun First Nation Final Agree- ment	YK	2005	Yes	No

Note: BC: British Columbia, QC: Quebec, NT: Northwest Territories, YK: Yukon.
Source: AANDC (2013).

Census data The empirical analysis uses micro-data from four rounds of the long-form Cana- dian Census (years 1991, 1996, 2001 and 2006). I use the confidential version available through the Research Data Centers program. This version of the long-form Census contains detailed information, at household and individual level, on income, socio-economic characteristics, and

geographical location for a sample of the population.[25] Normally, the survey sample is 20% of the population. However, in the case of many Indian reserves and rural communities the whole population was surveyed.[26]

A main data challenge is to match individual records to First Nation bands.[27] To do so, I obtain linkage tables from the AANDC.[28] These tables provide a mapping of Indian reserves, and bands, to Census Sub-Divisions (CSDs). These tables, however, are only available for 2006 and 2001, so I reconstruct the mapping for previous years. Simply extrapolating the mapping from 2001 or 2006 to previous years would be wrong, since CSDs may change over time: they split, merge, or change coding. To address this issue, I use geographical concordance tables to trace back the changes to CSDs (Statistics Canada, 2012). This allows me to create a reliable mapping of CSDs between 1991 and 2006, so I can identify which CSDs correspond to Indian reserves. These concordance tables are, however, available only since 1991. Due to this data limitation, I restrict the main analysis to the period 1991 to 2006.[29]

A second issue is the incomplete enumeration of some Indian reserves that refused to participate in the Census. However, the magnitude of this issue is not significant and seems unlikely to affect the results. For instance, only 1.96% of observations correspond to bands that have at least one incompletely enumerated reserve between 1991 and 2006. Moreover, the results remain basically unchanged when excluding these observations.

The final dataset is a repeated cross-section of individuals 15 years and older living on CSDs classified as Indian reserves in years 1991, 1996, 2001 and 2006. As discussed above, in the baseline results, I restrict the sample to provinces and territories that have implemented at least one modern treaty. The dataset represents a population of almost 144,000 households who lived in around 250 Indian bands during the period of analysis. Table 2 presents the mean of the main variables used in the empirical analysis.[30] Note that due to the confidential nature of the dataset, I cannot report unweighted statistics —such as actual number of observations— and I round ratios and frequencies.

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[25]In contrast, the publicly available dataset (Public Use Micro Files - PUMF) has a smaller sample, just 2.7% of the population, and only includes geographical identifiers for large areas, such as provinces and metropolitan areas.

[26]In the dataset I use, the likelihood of being surveyed is around 80%.

[27]I also checked the Census codebooks and re-define some variables to guarantee comparability over time.

[28]These tables are available upon request from AANDC's Statistical Office.

[29]I do, however, use data from 1986 to evaluate pre-trends and check robustness of the results. For 1986, I simply extrapolate the mapping of First Nation Band and CSDs of 1991.

[30]See Appendix A for a detailed description of variables.

Table 2: Mean of main variables

Variables	Whole sample	Non-treaty bands	Treaty bands
Panel A: Household level			
Treaty implemented	0.119	-	-
Ever implemented a treaty	0.150		
Nominal household income (CAD)	40,384	38,545	50,791
Real household income	34,853	33,302	43,389
Household size	3.1	3.0	3.9
CSD population	1,206	1,195	1,268
Owns house	0.565	0.605	0.334
Dwelling needs major repair	0.235	0.224	0.292
Nr. rooms	6.0	6.0	5.0
House price (CAD)	115,172	116,236	104,313
Urban	0.245	0.271	0.131
Panel B: Individual level			
Nominal individual income (CAD)	13,043	13,002	13,220
Real individual income	16,920	16,667	18,066
Age	31.9	33.0	27.5
High school or above	0.496	0.509	0.436
Female	0.490	0.491	0.484
Primary maintainer	0.321	0.335	0.256
Registered as status Indian	0.724	0.690	0.868
Live in same CSD	0.822	0.812	0.868
Labor force	0.454	0.440	0.521
Employed	0.962	0.963	0.961
Hours worked (per week)	33.1	33.1	33.1
Real wage	13.0	12.0	13.0

Notes: Means are calculated using sampling weights and rounded due to confidentiality requirements. CAD = Canadian dollar. Samples includes bands in provinces and territories that have at least one treaty band (BC, QC, NT and YK). Real values are measured in 1991 CAD.

3.2 Empirical strategy

The aim of the empirical analysis is to explore the effect of modern treaties on economic outcomes, such as real income and wages, of population living on Indian reserves.

The main empirical challenge is to find a suitable counterfactual, i.e., what would have happened among treaty bands in the absence of a treaty. A simple cross section comparison of bands with and without a treaty would be insufficient because there may be unobservable differences between both groups —such as degree of internal cohesion, potential for extractive industries, or quality of local politicians— that would affect both treaty implementation and economic outcomes.

To address this concern, I use a difference-in-difference (D-i-D) approach exploiting the timing of treaty implementation. This quasi-experimental approach uses treaty implementation as a treatment, and compares the evolution of outcomes in treaty bands relative to non-treaty bands.

Figure 1 illustrates the basic idea behind the identification strategy. It plots the number of treaties implemented by a given year, and the conditional mean of real household income in treaty and non-treaty bands.[31] Note that in 1986, real income in both groups was similar. Between 1986 and 1991, they also followed similar trends. Since 1991, however, there has been a divergence, with income in treaty bands increasing at a faster rate. This change in the evolution of household income parallels the pattern of treaty implementation, which started in 1990 (see Figure C.1).

The validity of the D-i-D approach relies on the assumption that economic outcomes in both groups, treaty and non-treaty bands, would have followed the same trend in the absence of treaties. The similarity of trends before 1991, when treaty implementation was at its beginnings, is thus a necessary condition for the validity of this approach.[32]

Treaty and non-treaty bands are, however, not identical even before treaties are implemented. Using a cross-section of bands in 1991, I find that bands with smaller, rural, populations, and with larger extractive industries are more likely to implement a treaty (see Table C.2 in the Appendix).[33] While these initial differences are controlled by the D-i-D strategy, a
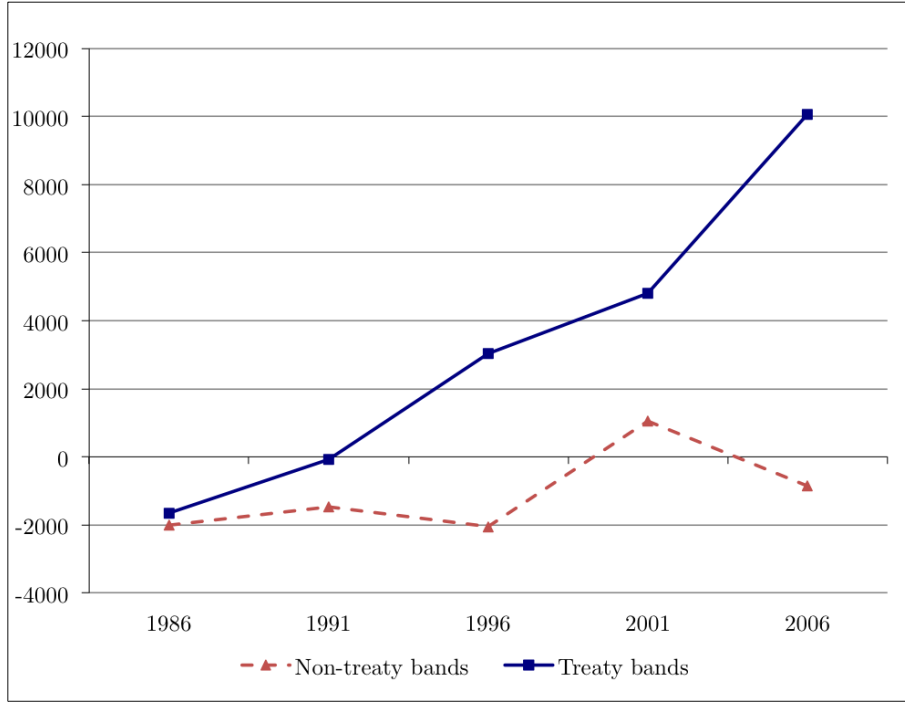
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[31]The mean is conditional on age, age^2, and education level of the principal maintainer; and household size.

[32]I formally test the similarity of pre-trends in Section 4.2.

[33]However, initial income, inequality, education are not significant determinants of treaty implementation.

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Figure 1: Conditional mean of real household income, by Census year

concern is that they could be associated to different paths of economic growth. Similarly, there may be several unobserved time-varying factors that affect differentially treaty and non-treaty bands. In either case, these omitted variables could invalidate the identification strategy. I address these concerns in more detail in Section 4.

To formally implement the D-i-D approach, I estimate the following regression:

$$y_{ijt} = \beta treaty\ implemented_{jt} + \gamma X_{ijt} + \delta W_{jt} + \rho_t + \eta_j + \epsilon_{ijt}, \tag{1}$$

where the unit of observation is household (or individual) i, living on reserve of band j and year t. y_{ijt} is the outcome variable, such as real income, or house value. In some specifications, I also use other outcomes such as real wages, hours worked, or employment status.

To obtain measures of real income and real wages, I deflate nominal values using a band-specific consumer price index (CPI). This local CPI allows for housing costs to vary across reserves held by different bands, and for non-housing prices to vary across provinces. The methodology to construct this index closely follows Moretti (2013).[34]

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[34]See Appendix B for further details on construction of CPI.

The main regressor, *treaty implemented$_{jt}$*, is a dummy equal to one if the band has already implemented a treaty. The omitted category are non-treaty bands and bands that had not implemented a treaty yet. In this specification, parameter β captures the effect of treaty implementation.

All regressions include year (ρ_t) and band (η_j) fixed effects, as well as controls at individual, X_{ijt}, and band level W_{jt}. I estimate the regression using sampling weights and cluster the errors at band-year level.[35]

4 Main results

4.1 Effect on real income

Table 3 reports the main results. Panel A uses as outcome variable the log of real household income, while Panel B displays results using instead the log of real individual income. Column 1 displays the baseline results using the preferred sample of bands in provinces and territories that implemented at least one treaty. This specification includes band and year fixed effects, and several covariates and household and individual level (see notes of Table 3 for details). Columns 2 and 3 check the robustness of the results to alternative sample definitions. In column 2, I estimate the baseline regression including observations from year 1986. Note, however, that geographical matching of CSDs to bands in 1986 is less precise than in most recent years. This may introduce measurement error. In column 3, I use the sample of all provinces in Canada for period 1991 to 2006. In both cases the estimates are similar to the baseline regression.

The relation between treaty implementation and real income is positive and significant.[36] Under the assumption that the evolution of income would have been similar in treaty and non-treaty bands in the absence of treaties, we can interpret these results as evidence of a positive effect of treaties on real income. The magnitude of the effect is economically significant. The estimate suggests that after the beginning of treaty implementation, real household income increases by around 9 percent.[37]

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[35]I cluster the errors at this level for two reasons: (1) to account for possible spatial correlation among individuals, and (2) to recognize the level of variation of the regressor of interest (*treaty implemented$_{jt}$*).

[36]Note that these results are in real terms, i.e., above any increase in local prices that may be associated to treaty implementation. Using nominal income produces similar positive results but, as expected, the point estimates are greater (see Table C.3).

[37]The estimates using real individual income are greater because there are several cases with non-positive individual income. These cases are, by contruction, excluded from the estimation. In contrast, the number of

The estimated regression controls for all time-invariant band characteristics —such as location or historical background— as well as for common time variation. A main concern, however, is that there may be time-varying confounding factors correlated to income differences and treaty implementation. As I mentioned before, bands with small, rural, populations or larger extractive industries are more likely to implement a treaty. These initial differences are controlled for by band fixed effects. They may be, however, associated to different growth paths and confound the effect of treaties. Similarly, other institutional reforms —such as change in electoral system— may affect income and also be associated to treaty implementation.[38]

I address this concern in two ways. First, I include several time-varying covariates (column 4). Specifically, I add non-parametric trends interacted with indicators of province, urban status, population size, employment share of extractive industries, and band electoral system.[39] These control variables account for heterogeneous trends associated to these observable characteristics.

Second, I change the sample to include only bands that, during the period of analysis, (1) have implemented a treaty, or (2) have a treaty under negotiation (column 5).[40] This specification effectively uses bands that have treaties under negotiation, but have not implemented them yet, as a control group for treaty-bands. This reduces concerns of selection bias since this control group is likely to be more comparable to treaty-bands that bands that have not started treaty negotiations. Columns 4 and 5 present the results of these checks. In both cases, the results are similar to the baseline regression.

A related issue is whether the timing of treaty implementation is endogenous. A main concern is that completion of treaty negotiations (or beginning of its implementation) are correlated to some factors that also affect the trend of economic development. For instance, influential band leaders may be more able to complete treaty negotiations and also implement policies or reforms that foster economic growth. Similarly, treaties may be more likely to be completed during economic booms. Failing to account for these factors may confound the effect of treaties.

A review of factors that affect the outcome of treaty negotiation suggest, however, that this identification concern might not be very important. Existing qualitative studies suggest that

cases with non-positive household income is negligible.

[38]First Nations bands have two basic types of electoral systems: a system based on the Indian Act, and a custom-based system. All treaties modify the electoral system to a custom-based type. However, it is not a pre-requisite to have a treaty to modify the electoral system.

[39]I discretize continuous band-level variables, such as population size and employment share in extractive industries, by creating a dummy equal to 1 if its value is above the median, and 0 otherwise.

[40]See table C.1 for a list of treaties under negotiation.

the main factors for failing to complete a treaty include: lack of political will from provincial and federal governments, differences in governmental and Aboriginal world-views and goals, confrontational negotiation tactics, internal divisions in Aboriginal groups, and negative perception of Aboriginal groups (Alcantara, 2007a; Alcantara, 2008; Alcantara, 2013). To the extent that these factors are time-invariant or related to broader (i.e provincial or national) circumnstances, they are already controlled for by the set of band, year and province-year fixed effects.

4.2 Exploring dynamic effects

A relevant question is when the effect of treaty on real income occurs. There could be a lag in the effect if, for instance, the institutional changes require some time to mature or to be implemented. The effect could also occur *before* implementation if local markets change in anticipation to the institutional reform, or if treaty and non-treaty bands follow different growth paths. This last case is relevant since it would shed doubts on the validity of the identification assumption

To explore these issues, I extend the baseline regression (1) by including lags and forwards of *treaty implemented*. In particular I estimate the model:

$$y_{ijt} = \sum_k \beta_k \, treaty \; implemented_{j,t+k} + \gamma X_{ijt} + \delta W_{jt} + \rho_t + \eta_j + \epsilon_{ijt}, \tag{2}$$

where k is a time period relative to the beginning of treaty implementation. Due to data limitations, I define k as a range of years before or after treaty implementation.[41] Using this notation, *treaty implemented*$_{j,t+k}$ represents a dummy equal to 1 if we observe a treaty band k years from the beginning of treaty implementation, and 0 otherwise. The parameters of interest are β_k which capture the difference between treaty and non-treaty bands in periods before and after treaty implementation, relative to a initial period.[42]

Figure 2 presents the estimates of β_k and their 95% confidence interval. There are two relevant observations. First, there is no significant difference between treaty and non-treaty bands *before* the beginning of treaty implementation. This similarity of pre-trends increases confidence

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[41]These periods are: -4 to -6, -1 to -3, 1 to 3, 4 to 6, 7 to 9, and 10 and more. Thus, $k = $ (-4 to -6) means a period 4 to 6 years before the beginning of treaty implementation.

[42]Given the definition of k, this initial period corresponds to 7 and more years before treaty implementation

Table 3: Treaties and real income

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable = ln(real household income)					
Treaty implemented	0.087**	0.070*	0.085**	0.084*	0.123***
	(0.041)	(0.041)	(0.043)	(0.043)	(0.040)
Sample	BC, QC, NT and YK	Including 1986	All Canada	BC, QC, NT and YK	Treaty impl. or under negot.
Band fixed effects	Yes	Yes	Yes	Yes	Yes
Non-parametric trends	No	No	No	Yes	No
Weighted nr. of obs.	137,105	142,840	280,625	128,955	81,720
R-squared	0.287	0.265	0.319	0.291	0.351
Panel B: Dependent variable = ln(real individual income)					
Treaty implemented	0.353***	0.209**	0.304***	0.306***	0.291***
	(0.107)	(0.092)	(0.104)	(0.103)	(0.105)
Sample	BC, QC, NT and YK	Including 1986	All Canada	BC, QC, NT and YK	Treaty impl. or under negot.
Band fixed effects	Yes	Yes	Yes	Yes	Yes
Non-parametric trends	No	No	No	Yes	No
Weighted nr. of obs.	284,835	295,740	611,800	268,280	178,215
R-squared	0.310	0.309	0.349	0.313	0.332

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects, and a set of household and individual controls. Regressions in Panel A use ln(real household income) as outcome variable, and include as controls: characteristics of the principal maintainer (age, age^2, gender, Indian status, indicators of education level) and household size. Panel B uses ln(real individual income) as outcome variable and include as controls: age, age^2, gender, Indian status, indicators of education level, indicator of being principal maintainer, indicator of being employed, and household size. Column 1 corresponds to the baseline specification. It uses the sample of bands in provinces and territories with at least one treaty band. Columns 2 add to the baseline regression observations from 1986, while Column 3 expands the baseline sample to include all provinces in Canada. Column 4 includes non-parametric trends by province, urban status, type of electoral system, population size, and employment share of extractive industries. Column 5 uses the sample of bands that, between 1973 and 2006, implemented a treaty or had a treaty under negotiation. BC: British Columbia, QC: Quebec, NT: Northwest Territories, YK: Yukon.

on the validity of the identification assumption. Moreover, it rules out possible changes in economic conditions in anticipation of treaty implementation. Second the effect of treaties seems to increase over time. In the first 3 years after the beginning of treaty implementation, the effect is small and only marginally significant. From year 4 onwards, however, the magnitude of the effect increases. This finding is consistent with the gradual implementation of treaties.

Figure 2: Effect of treaty on real household income, by period
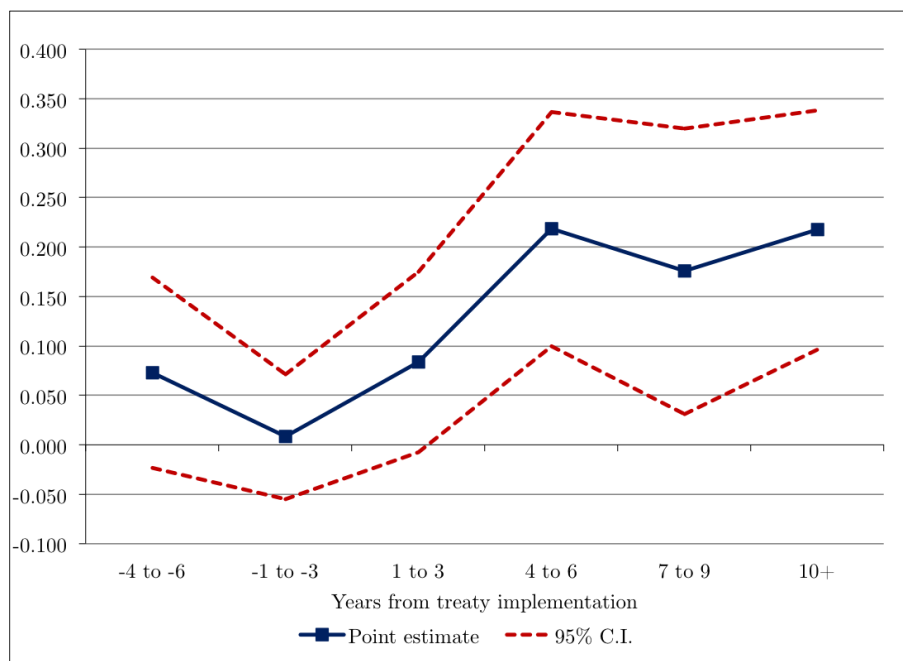
Note: Figure displays estimates of β_k from equation 2. The omitted category is a period 7 years and more before treaty implementation.

5   Alternative explanations

The previous results yield support to the hypothesis that treaties have a positive effect on household income. This paper argues that this effect is driven by changes in property rights associated to modern treaties.

There are, however, at least two alternative explanations of the observed results. First, treaties involve other institutional changes in First Nations communities. They are usually accompanied by financial compensation, increased participation of local governments on land management, and even self-government provisions that devolve local responsibilities to tribal councils. Some treaties also include provisions to eliminate Indian reserves and transform them

into private land. The presence of these confounding institutional changes would not alter the basic finding of a positive impact of treaties on income, but would affect the interpretation of the results.

Second, the institutional changes associated to treaties may affect migration patterns. A main concern is that the institutional reform would have increased net migration of relatively more productive workers. In that case, the results would just reflect compositional effects not an increase in household income. In this section, I explore the relevance of these two alternative explanations.

5.1 Confounding institutional changes

The confounding institutional changes associated to treaties suggest several alternative channels for treaties to affect income. Firsts, devolution of responsibilities (either through participation in land management or self-government) could expand the local public sector or improve provision of public goods. Second, financial compensation associated to treaties may increase governmental transfers to local residents. Finally, the results may be driven by elimination of Indian reserves instead of improvement of property rights in Aboriginal (off reserve) lands.

Expansion of the local public sector To assess the importance of this alternative explanation, I explore the effect of treaties on income of public workers, and public employment. To do so, I replicate the baseline regression splitting the sample between public and non-public workers. Then, I examine the effect of treaties on the likelihood of being a public worker.[43]

The results suggest that treaties only had a positive effect on real income among non-public workers (columns 1 and 2 in Table 4) . Moreover, workers' likelihood of working in the public sector has decreased. This is the opposite of what we could expect if the baseline results were driven by an expansion of the local public sector.[44]

Financial compensation I explore the role of financial compensation in two steps. First, I replicate the baseline results splitting the sample between households with and without band

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[43]I classify an individual as a public worker if she works in any of the following industry divisions: government services, education, or health and social services. The results are similar using a narrower definition of public workers, i.e., workers in government services. Industry divisions are classified according to the Standard Industrial Classification (SIC) 1980.

[44]A possible explanation for this finding is that nominal wage of public workers is set at national or regional level, not in local markets. In fact, I find no significant effect of treaties on nominal income of public workers.

Table 4: Treaties, public workers, and band members

	ln(real individual income)		Works in public sector	ln(real household income)	
	(1)	(2)	(3)	(4)	(5)
Treaty implemented	0.596***	-0.107**	-0.093***	0.107**	0.084*
	(0.161)	(0.045)	(0.022)	(0.047)	(0.048)
Sample	non-public workers	public workers	All workers	Band members	non-Band members
Weighted nr. of obs.	193,470	91,365	142,840	88,430	48,680
R-squared	0.297	0.377	0.214	0.308	0.225

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The set of controls and sample is similar to the baseline regression in column 1 of Table 3.

members. Note that band membership is required to access several band benefits, such as housing and share of band's assets, as well as to participate in the election of the tribal council.[45] Columns 3 and 4 in Table 4 display the results. If the results were mechanically driven by distribution of band resources, we would expect that income increase only among households with band members. The effect of treaties on real income is, however, positive for both types of households.

Second, I examine the effect of treaties on different income sources. Given the data availability, I can decompose individual income in four broad sources: employment income, unemployment benefits, government transfers, and other income. Note that transfers associated to financial compensation or additional federal funds would be recorded as government transfers, or other income.[46] I also distinguish employment income of public and non-public workers.

Table 5 presents the estimates of baseline equation (1) using as outcome variables the dollar values of individual income by different sources.[47] Note that, in average, real income increases

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[45]Access to federal programs, such as income support, require having Indian status. The baseline regressions already control for this variable. Moreover, the results are similar splitting the sample by Indian status. Note that having Indian status does not automatically guarantee being member of a band.

[46]Employment income includes wages and salaries. Unemployment benefits includes benefits managed by the federal Employment Insurance Program. Government transfers include all transfers from government sources, except for pensions, income supplements and unemployment benefits. Other income includes all remaining sources of income, such as pensions, income supplements, child benefits, investment income, payment from retraining programs, etc. Employment income is the most important income source, accounting for almost 68% of household income. In contrast, government transfers and unemployment benefits together account for around 10%.

[47]I use dollar values, instead of logs, to facilitate exposition and to avoid dealing with zero values for some income sources. The results using logs, however, are similar.

by around CAD 2,360 for non-public workers, but decreases CAD 2,900 for public workers (columns 1 and 2). This result is in line with the findings in Table 4. Most of the change is driven by employment income, which, in the case of non-public workers, increases by almost CAD 1,900 (column 3). Government transfers and unemployment benefits also increase, but the magnitude is relatively small (columns 6 and 7). Taking together, these results weaken the argument that the observed increase in average income is driven by financial compensation associated to treaties.

Self-governance and elimination of Indian reserves   Finally, I examine whether the effect of treaties on income is driven by the inclusion of self government provisions, or by clauses that eliminate Indian reserves. These are important institutional changes implemented by some treaties, in addition to the clarification of property rights.

To do so, I replicate the baseline regression (1) splitting the sample of treaty bands between bands with and without a give treaty provision (i.e. self governance or elimination of Indian reserves). As a control group I include bands that started, but not yet completed, treaty negotiations. This approach basically allows for heterogeneous effects of treaties based on this characteristics.[48]

Table 6 displays the results. The results suggest that there are not significant differences driven by these treaties' characteristics. There are, however, two important caveats. First, the number of treaties without self-government provisions is small (2 out of 16). This may reduce the statistical power of the analysis. Second, during the period of analysis, former reserve lands were not privatized, but remained property of the tribal councils. This may explain the lack of importance of provisions that eliminate Indian reserves.[49]

While not conclusive, this evidence support the interpretation that improvement in property rights, and not other confounding institutional changes, drives the observed effect of treaties on real income.

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[48]Alternatively, I can include an interaction term of *treaty implemented* with indicators of treaty characteristics. The results using this approach are similar.

[49]The first case of private ownership of former reserve lands occurred only in 2013.

Table 5: Treaties and real income, by income source

| | Real individual income | | | | Sources | | |
	non-public workers (1)	public workers (2)	Non public employment (3)	Public employment (4)	Unemploy. benefits (5)	Government transfers (6)	Other income (7)
Treaty implemented	2,365.2**	-2,917.8***	1,921.7**	-2,348.1***	52.9	161.1**	-207.1**
	(1,045.4)	(681.3)	(975.3)	(717.0)	(56.0)	(67.7)	(99.0)
Mean value	15,012	16,542	9,875	11,668	573	1,058	3,423
Weighted nr. of obs.	213,085	97,710	213,090	97,710	310,800	310,800	310,800
R-squared	0.195	0.333	0.216	0.408	0.052	0.079	0.288

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The set of household controls and sample is similar to the baseline regression (column 1 in Table 3). All outcome variables are measured in 1991 Canadian dollars.

Table 6: Treaties and real income, by treaty characteristic

	ln(real household income)			
	(1)	(2)	(3)	(4)
Treaty implemented	0.234***	0.108**	0.155**	0.134***
	(0.058)	(0.047)	(0.069)	(0.045)
Treated group includes bands whose treaties:	Do not implement self government	Implement self government	Do not eliminate reserves	Eliminate reserves
Weighted nr of obs.	62,310	78,670	64,670	77,690
R-squared	0.325	0.352	0.323	0.357

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The set of controls and sample is similar to the regression in column 3 of Table 3.

5.2 Compositional changes

A second relevant concern is the possible change in composition of local population. This may happen, for instance, in the presence of selective migration.

I explore this alternative explanation by examining whether treaties are associated to changes in observable characteristics of the population. To do so, I estimate the baseline equation (1) using population characteristics as outcome variables. I use measures of (1) population demographics, such as gender, age and household size, (2) migration (i.e. an indicator of whether the individual has lived in the same CSD 5 years ago), (3) education, and (4) band membership.

Table 7 presents the results. Note that in almost all cases, there is no change in observable characteristics. Treaty implementation is only associated to an increase in band membership. This is expected given the (potential) increase in benefits associated to band membership, such as access to band transfers and assets.

Table 7: Treaties and population characteristics

	Female	Age	Household size	Live in same CSD	High school or above	Band member
	(1)	(2)	(3)	(4)	(5)	(6)
Treaty implemented	-0.001	0.023	-0.061	-0.006	0.003	0.044*
	(0.003)	(0.395)	(0.067)	(0.013)	(0.011)	(0.023)
Weighted nr. of obs.	450,910	450,910	144,870	409,050	326,700	450,910
R-squared	0.002	0.111	0.139	0.061	0.093	0.482

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The sample includes all households and individuals in provinces and territories with at least one treaty implemented.

6   Exploring the mechanism

The previous results suggest that treaties increase household income, mostly from employment sources. A relevant question is: why would employment income increase? To examine this question, I use the analytical framework discussed in Section 2.2.

In this framework, I treat treaties as a reduction in transaction costs that facilitate development of extractive activities, such as mining. In turn this can generate a positive shock to the demand of local labor. A direct effect of this shock is to increase wage, and income, of households working in extractive industries. This local demand shock would spread to the rest of the local economy, not directly engaged in extractive activities, via increases in wages and prices of non-tradables, such as housing. To the extent that labor and housing supply are not perfectly elastic, these changes would increase workers' real income.

I evaluate these predictions in four steps. First, I explore whether treaties are indeed associated to an increase in contracts that facilitate development of extractive activities. This can be indicative of reduction of transaction costs. Second, I examine whether the effect of treaties on income spread to workers in other industries. Third, I study the effect of treaties on employment and local prices. Finally, I explore possible geographical spillovers to neighbouring communies. In particular, I examine whether treaties have affected economic outcomes in populations living *outside* Indian reserves but within commuting distance.

6.1 Do treaties reduce transaction costs? Treaties and mining agreements

Ideally, we would like to evaluate how treaties affect a measure of transaction costs faced by extractive industries, such as number of days to obtain a license or monetary cost of public consultations. Unfortunately, this information is unavailable. Instead, I examine the effect of treaties on mining agreements.

Mining agreements are contracts between mining companies and Aboriginal communities, with or without treaties, to explore or develop new mining operations (Natural Resources Canada, 2013). They specify the obligations of each party regarding mitigation actions, as well as the benefits to the local community (such as employment opportunities, training, or revenue-sharing). These agreements are negotiated in almost all new mining projects in Canada and are regarded as a best practice by the mining industry (Sosa et al., 2001). They arise as a response to the duty to consult with Aboriginal people that may be affected by mining projects in or near their traditional lands. The number of mining agreeements has steadily increased since mid 1990s, specially among treaty-bands (see Figure C.2 in the Appendix). In 2012, there were 280 mining agreements which involved around 24% of First Nations bands.

Mining agreements provide a way to examine whether treaties have facilitated economic transactions over natural resources. In this case, the contract is between a mining company and a First Nation community.[50] To do so, I construct a panel dataset of First Nations bands with annual observations for the period 1988-2012, and estimate the following regression:[51]

$$mining\ agreement_{jt} = \phi treaty\ implemented_{jt} + \rho_t + \delta_j + \mu_{jt}, \qquad (3)$$

where the unit of observation is band j in year t. $mining\ agreement_{jt}$ is the number of mining agreements signed by the band up to year t, and $treaty\ implemented_{jt}$ is an indicator of whether the band has implemented a treaty. This specification includes band and year fixed effects, and cluster standard errors at band level to account for possible serial correlation. Note that this specification exploits within-band variation, hence it already controls for time-invariant band characteristics, such as location or initial mining potential.

Table 8 presents the results. Similar to the baseline regressions, column 1 restricts the

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[50]This relies on the assumption that mining agreements are more likely to occur when transaction costs are lower.

[51]Data on mining agreements was obtained from Natural Resources Canada (2013).

sample to bands in province and territories with at least one treaty-band. Column 2 uses a sample of bands that have implemented a treaty or have one under negotiation, while column 3 extends the sample to all bands in Canada. In all cases, treaty implementation is associated to a significant increase in the number of mining agreements. The most conservative estimate suggests an increase of 0.371, or almost two times the mean.[52]

Taken together, these results support the hypothesis that treaties indeed facilitated contracts related to natural resources, and the development of extractive industries. A main caveat is, however, that they are only informative about the effect on mining, not about other extractive industries, such as a logging or fishing.

Table 8: Treaties and mining agreements

| | Nr. active mining agreements | | |
	(1)	(2)	(3)
Treaty implemented	0.390***	0.371***	0.402***
	(0.121)	(0.117)	(0.121)
Sample	Bands in BC, QC, NT, YK	Treaty implem. or under negot.	All bands
Mean outcome var.	0.156	0.194	0.123
Nr. Bands	282	137	634
Observations	6,768	3,288	15,216
R-squared	0.127	0.184	0.121

Notes: Robust standard errors in parentheses. Standard errors are clustered at band level.* denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects.

6.2 Effect on income by industry

I then examine whether the increase in income is circumscribed to workers in extractive industries, or whether it spreads to workers in other industries. To do so, I split the sample of workers by main industry of occupation. The industry classification is based on industry divisions from

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[52] I also estimate equation (3) including lags and forward values of *treaty implemented*$_{jt}$. In particular, I estimate:

$$mining\ agreement_{jt} = \sum_{k=-5,-3,0,3,5,7} \phi_k treaty\ implemented_{jt+k} + \rho_t + \delta_j + \mu_{jt}.$$

This specification allows me to explore when treaties affect mining agreements. The estimates of ϕ_k are displayed graphically in Figure C.3. They suggest that mining agreements increase *after* treaties are implemented.

the SIC 1980.[53] I group these industry divisions into fewer groups, but the results are similar using a finer disaggregation.[54] Then, I replicate the baseline regression (1) using real individual income as outcome variable.

Table 9 displays the results. Note that real income increases for workers in extractive industries, but also for workers in other industries not directly engaged in extractive activities, such as non-public services, trade, and manufacturing.

Taken together, these results suggest that the initial effect of treaties spreads among the whole local economy. This is consistent with the spillover effects from a positive shock to the local demand for labor in presence of a relatively inelastic labor supply. However, to further explore this interpretation, we also need to examine what happens with local employment and prices.

Table 9: Treaties and real income, by industry

	ln(real individual income)				
	(1)	(2)	(3)	(4)	(5)
Treaty implemented	0.299**	0.159**	0.161**	-0.107**	0.318***
	(0.120)	(0.079)	(0.079)	(0.045)	(0.090)
Industry	Extractive industries	Manufacturing and others	Trade	Public services	Non-public services
Weighted nr of obs.	23,260	38,605	19,855	91,365	46,885
R-squared	0.229	0.266	0.330	0.377	0.346

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects, and individual control variables. The set of individual controls and sample are similar o the baseline regression in column 1 of Table 3. See main text for definition of industry groups.

6.3 Effect on employment and local prices

The analytical framework suggests that, if labor is inelastically supplied, then an increase in local demand of labor would translate into an increase in real wages, and worker's income. In

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[53]Note that the 2006 Census used the NAICS classification, while previous Censuses used the SIC 1980. I use data from the 2001 Census (which reported both NAIC and SIC 1980) to construct concordance tables between both classification systems.

[54]I group industry divisions in five groups: (1) extractive industries (such as mining, logging, agriculture and fishing) (3) manufacturing and others (such as construction, transportation and communication, and utilities), (4) trade (retail and wholesale), (5) public services (i.e, government services, education, health and social services) and (6) non-public services, which include the rest of service industries.

addition, the price of inelastically supplied non-tradable goods, such as housing, would also increase due to the expansion of the local budget constraint.

To explore this prediction, I first examine the relation between treaties and labor outcomes. I use several indicators of labor supply in the extensive and intensive margin, such as population size, participation rates, employment rates, and number of hours worked.[55] Table 10 displays the results. Column 1 uses data aggregated at CSD level and restrict the sample to areas whose geographical definition has not changed since 1991.[56] Columns 2 to 4 use micro data at individual level. In all cases, however, the relation is not significantly different than zero. This evidence yields support to the assumption that treaties have not significantly affected local labor supply.

Table 10: Treaties and local employment

	ln(CSD pop.) (1)	Labor force (2)	Employed (3)	ln(hours work) (4)
Treaty implemented	0.073 (0.067)	-0.012 (0.011)	0.001 (0.007)	-0.017 (0.016)
Sample of:	Census Sub-divisions	individuals age 15+	Individuals in labor force	workers
Weighted nr of obs.	1,024	326,700	148,440	137,620
R-squared	0.837	0.240	0.033	0.093

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level, except for column 1, in which they are clustered at band level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects, and include only bands in BC, QC, NT and YK. Column 1 uses data aggregated at CSD level, and does not include any additional control variable. The weight for each observation is equal to one. Column 2 to 3 includes as controls: age and it square, gender, Indian status, indicators of education level, indicator of being principal maintainer, and household size. Column 4 also adds an indicator of being employed.

Second, I examine the relation between treaties and local prices (see Table 11). Columns 1 and 2 explore the effect of treaties on real wages for both public and non-public workers. This specifications controls for workers characteristics —such as education, age, gender, and industry of occupation. Columns 3 and 4 estimate hedonic regressions using self-reported house values

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[55]Note that in Moretti (2011)'s original model, labor supply changes only due to migration. This would imply changes in population size.

[56]This regression uses data from the public version of the Census aggregated at CSD level. Results are similar using information from all bands.

and monthly gross rents.[57] Note that these measures of prices are in nominal terms.

The evidence suggests that treaties increase house prices and real wages, except for public workers. This change in local prices is consistent with the general equilibrium effects of a positive shock to a local economy. Moreover, they shed light on the mechanism linking this positive shock to an increase in real income.

Table 11: Treaties and local prices

	ln(real wage) (1)	ln(real wage) (2)	ln(house price) (3)	ln(rent) (4)
Treaty implemented	0.159*** (0.051)	-0.107** (0.049)	0.333*** (0.082)	0.197*** (0.054)
Sample of:	non-public workers	public workers	home owners	tenants
Weighted nr of obs.	71,000	41,375	81,190	31,870
R-squared	0.201	0.219	0.259	0.368

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects, and use same sample as in baseline regression. Column 1 and 2 include as controls: age and its square, gender, Indian status, indicators of education level, indicator of being principal maintainer, household size, industry dummies, indicator of being employed, and indicators of labor force activity. Columns 3 and 4 include as controls: number of rooms and its square, indicators of need for repairs, indicator of urban area, log of CSD population, and home owner's Indian status and educational attainment.

6.4 Geographical spillovers

I interpret the previous results as evidence that treaties have created a local positive shock on demand for local labor. So far I have assumed that local labor markets correspond to Indian reserves only. Local labor markets, however, may be larger and extend to areas outside Indian reserves. If that is the case, then neighbouring *off-reserve* communities may also be affected by treaties. In this sub-section, I explore this possible geographical spillovers.

To do so, I identify a sample of people living outside Indian reserves hold by treaty bands but within commuting distance.[58] In particular, I select all individuals living in Census Sub-

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[57]I also examine the relation between treaties and housing conditions (see Table C.5 in the Appendix). Similar to labor supply, there is not a significant effect of treaties on housing outcomes.

[58]In 2001, the average commuting distance for individuals living within 50km of an Indian reserve was 11.2 km. The average commuting distance for all individuals in provinces and territories with at least one treaty band was very similar, 11.8 km.

Divisions (CSDs) with at least some part within 10 km of Indian reserves' boundaries.[59] Then, I replicate the baseline regression (1) using this new dataset. The main difference is that I include CSD fixed effects instead of band fixed effects and cluster the errors at CSD-year level instead of band-year level.

Tables 12 and 13 present the results. These results suggest that treaties also increase real income and real wages in neighbouring communities. Moreover, the increase in real income is driven mostly by workers in extractive industries and non-public services. These results are similar to the ones documented for populations living on Indian reserves. The main difference is that there is no increase on housing costs.[60] Importantly, they are consistent with the presence of geographical spillovers expected from localized demand shocks.

Table 12: Treaties, real income and prices in neighbouring areas

	ln(real household income)	ln(real individual income)	ln(real wage)	ln(house value)	ln(rent)
	(1)	(2)	(3)	(4)	(5)
Treaty implemented	0.116***	0.075*	0.200***	-0.088	0.015
	(0.033)	(0.043)	(0.062)	(0.093)	(0.044)
Weighted nr. of obs.	16090	31435	18425	13885	6045
R-squared	0.209	0.316	0.188	0.344	0.261

Notes: Robust standard errors in parentheses. Standard errors are clustered at Census subdivision-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. The sample includes individuals living in Census subdivisions within 10 kilometers of Indian reserves hold by treaty bands. It excludes individuals living on Indian reserves. All regressions include census subdivision and year fixed effects, and a set of control variables. Columns 1 and 2 use the same set of controls as column 1 in Table 3. Columns 3 use the same set of controls as column 2 in Table 11. Columns 4 and 5 use the same set of controls as columns 3 and 4 in Table 11.

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[59]Ideally I should focus on individuals whose residence is within commuting distance of reserves. Data on geographical location, however, is only available at the Census Sub-Division level. Given the large size of CSDs outside Indian reserves, this data limitation may introduce mesurement error since I will include individuals that actually are outside the local labor market.

[60]This last result may reflect a more elastic supply of housing outside Indian reserves.

Table 13: Treaties and real income in neighbouring areas, by industry

| | ln (real individual income) | | | | |
	(1)	(2)	(3)	(4)	(5)
Treaty implemented	0.507***	0.019	-0.085	-0.036	0.155*
	(0.095)	(0.085)	(0.079)	(0.060)	(0.083)
Industry	Extractive industries	Manufacturing and others	Trade	Public services	Non-public services
Weighted nr of obs.	4,390	6,245	3,555	7,195	6,280
R-squared	0.285	0.296	0.331	0.335	0.327

Notes: Robust standard errors in parentheses. Standard errors are clustered at Census subdivision-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. The sample includes individuals living in Census subdivisions within 10 kilometers of Indian reserves hold by treaty bands. It excludes individuals living on Indian reserves. All regressions include census subdivision and year fixed effects, and individual control variables. The set of individual controls and definition of industry group are similar to regressions in Table 9.

7 Final remarks

This paper study the local economic effects of First Nations modern treaties, an important institutional reform that clarified ownership over lands and resources near Aboriginal communities. I find robust evidence that modern treaties have increased real household income. This benefit spreads to other workers in the local economy. The results are driven by increase in wages and employment income, not by other changes associated to treaties, such as financial compensation or expansion of the local public sector.

The main contribution of the paper is to document the effect of better property rights on real income and show how general equilibrium effects can transmit the benefits throughout a local economy. This insight enrich existing evidence already linking property rights to improvements on investment, productivity and other economic outcomes.

The policy implications of these findings are important not only in Canada, where treaty making is still an unfinished business, but also in the context of less developed economies. First Nations communities share important similarities to populations in less developed societies: they are mostly rural, relatively immobile and with ill-defined property rights. This paper suggest that, in these cases, the benefits of property right reforms may spread beyond the direct beneficiaries, through its indirect effects on local labor markets.

Some words of caution are, however, necessary. The treaty bands studied are likely to be the ones with higher expected benefits from this institutional reform. Thus, the effect on real income is likely to be an upper bound value. Second, as suggested by the analytical framework, the results depend on a relatively inelastic labor supply. With more mobile workers, the effect on real income may be smaller. These caveats should be taken into account when using the results of this paper to predict the economic impact of other property right refoms.

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A   Description of variables

Note: I use bold and italics format (e.g. ***varname***) to refer to variables from the Census raw microdata.

- age: Age (in years), same as ***age***.

- CSD population: Population size of CSD of residence, same as ***pop***.

- dwelling needs major repairs: Dummy equal to 1 if dwelling needs major repairs: ***rpair***==3.

- educational attainment: Highest education level. This variable is based on ***hcdd*** for 2006, and in ***hlos*** for previous years. This variable identifies the minimum comparable categories of ***hcdd*** and ***hlos*** to make them comparable over time. Categories are: 0 "Non applicable", 1 "None (less than high school certificate)", 2 "High school certificate", 3 "Above High school but below Univ bachelor degree (incl trades, college , etc)", 4 "University bachelor level or above" and 5 "Masters and PhDs".

- employed: Dummy variable equal to 1 if individual is employed, conditional on being in labor force. Individual is considered employed if ***lf71*** is between 1 and 5.

- employment income: Real income from employment sources (***empin***/local CPI).

- ever implemented a treaty: Dummy equal to 1 if CSD corresponds to an Indian reserve held by a band that implemented a treaty between 1973 and 2006.

- female: Dummy equal to 1 if individual is female: ***sex***==1.

- government transfers: Real income from government transfers (***govti***/local CPI).

- high school or above: Dummy equal to 1 if individuals highest education level is high school or above: educational attainment>= 2 (see above for definition of education attainment).

- hours work: Hours per week worked for pay or in self-employment, same as ***hours***

- house value: Dwelling value, same as ***value***.

- house rent: Gross house rent, same as ***grosrt***.

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– <u>household size</u>: Number of household members in survey (i.e. 15 years and older), equal to maximum of **persno** by household.

– <u>labor force</u>: Dummy equal to 1 if individual is part of the labor force (i.e., employed or seeking a job).

– <u>live in same CSD</u>: Dummy equal to 1 if individual lived in same CSD 5 years ago: **mob5** $>= 4$ **mob5** $<= 5$.

– <u>industry of occupation</u>: Industry division, based on the SIC 1980. This variable is equal to **inddivf** for year 2001. For 1991 and 1996, I obtain industry divisions from variable **ind80**, which also uses SIC 1980. For year 2006, I use variable **naics97**. I use information from 2001 to construct a translator from NAICS 1997 to SIC 1980.

– <u>Nr. rooms</u>: Number of rooms in dwelling, same as **rooms**.

– <u>labor force activity</u>: Same as **lf71**. Categories are: 1 "Employed - absent in reference week - Armed Forces", 2 "Employed - absent in reference week - Civilian", 3 "Employed - unpaid family worker", 4 "Employed - paid - Armed Forces", 5 "Employed - paid - Civilian", 6 " Not in labor force - Excluding institutional residents", 7 " Not in labor force - Institutional residents", 8 "Unemployed - Looked for work - experienced", 9 "Unemployed - Looked for work - inexperienced", 10 "Uenmployed - On temporary layoff".

– <u>other income</u>: Real income from sources other than employment, government transfers, or unemployment benefits = (**totinc** - **empin** - **govti** - **uicbn**)/local CPI.

– <u>owns house</u>: Dummy equal to 1 if dwelling is owned by a member of the household: **tenur** $==3$

– <u>primary maintainer</u>: Dummy equal to 1 if individual is primary maintainer: **hmain**$==3$

– <u>real household income</u>: **hhinc**/local CPI.

– <u>real individual income</u>: **totinc**/local CPI.

– <u>real wage</u>: Implied hourly wage:(**wages**$/52)/$**hours**) divided by local CPI.

- registered Indian: Dummy equal to 1 if individual is registered Indian, i.e,. has official Indian status: **rgindr**==2.

- treaty implemented: Dummy equal to 1 if the CSD corresponds to an Indian reserve held by a band that implemented a treaty before the Census year.

- unemployment benefits: real income from employment insurance benefits (**uicbn**/local CPI).

- urban: Dummy equal to 1 if individual lives in urban area.

B   Local consumer price index

I construct a band-specific local consumer price index using information on local housing costs. The index resembles the local CPI 1 in Moretti (2013).

To construct the index, I first obtain measures of housing costs at band level. Following Moretti, my preferred measure of housing costs is house rents. In particular, I take the average gross rent paid by tenants in a band and normalize it to be 1 in 1991. There are, however, several cases with no information on rental costs over time. In those cases, I use self-reported house prices instead. Note that the change in housing costs does not reflect individual changes but instead the average conditions in local housing markets.

Second, I obtain measures of non-housing costs using consumer price indexes at province level. In particular, the non-housing cost for a band in province p in year t is $\frac{CPI_{pt}-w_p H_{pt}}{1-w_p}$, where CPI_{pt} is the aggregate consumer price index, and H_{pt} is the price index of shelter costs at province level. w_p is the expenditure share in shelter. The consumer price indexes and expenditure shares correspond to the 2009 basket.[61] I also normalize this measure of non-housing costs to be equal to 1 in 1991.

Finally I construct the band-specific local price index by taking a weighted average of housing and non-housing costs. As weights I use the province-level expenditure share of shelter costs. This weight varies for years 1986, 1992, 1996, 2001 and 2006 (Statistics Canada, 2013c).

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[61]The information on province-level price indexes and expenditure shares comes from Statistics Canada (2013a) and Statistics Canada (2013b).

C Additional figures and tables

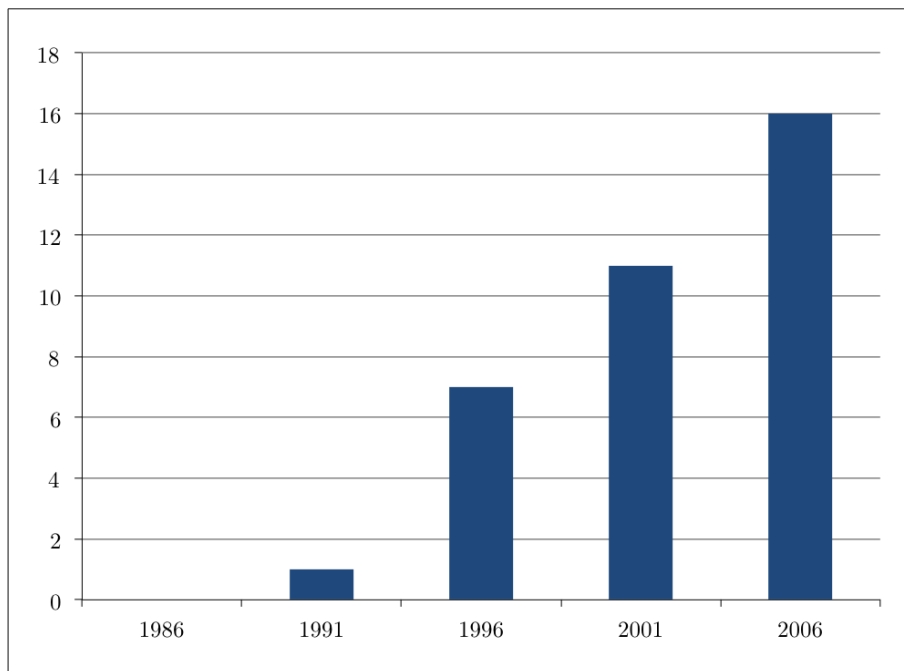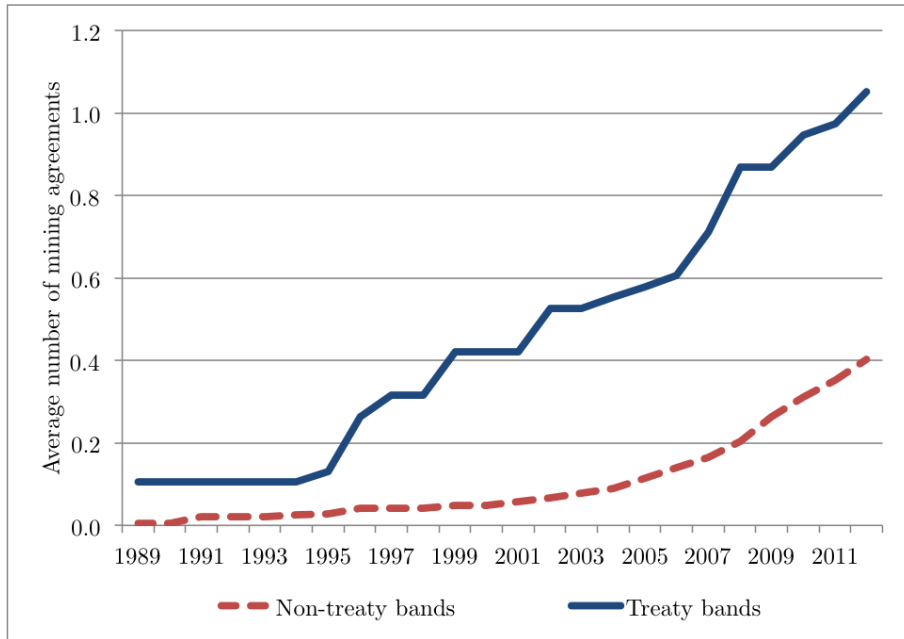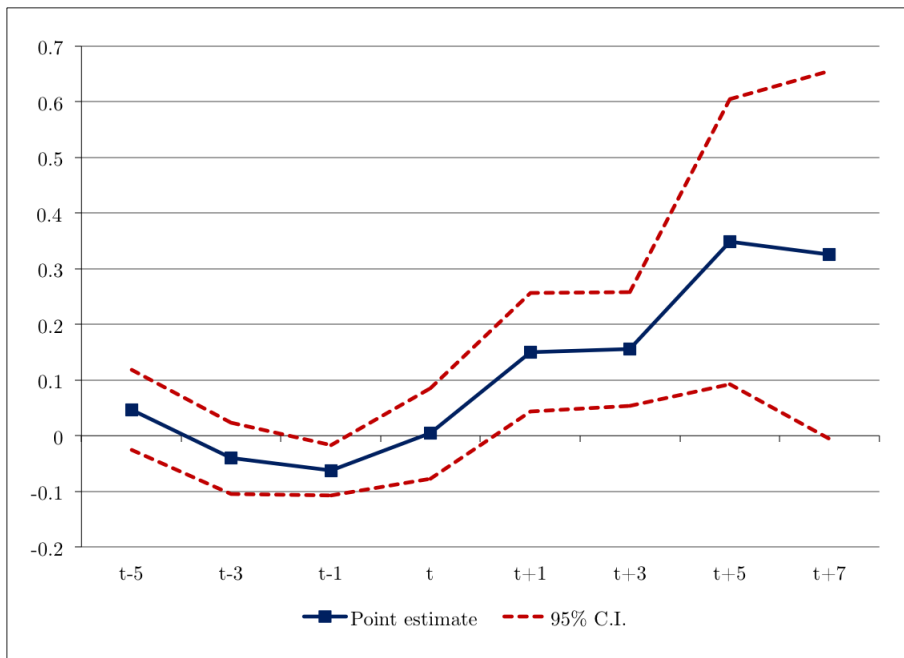Figure C.1: Cumulative number of treaties implemented, by Census year

Figure C.2: Average number of mining agreements

Note: Sample include only bands in provinces and territories with at least one treaty-band (BC, QC, NT and YK).

Figure C.3: Effect of treaties on mining agreements, by year since treaty implementation

Note: See footnote 52 for further details.

Table C.1: List of modern treaties under negotiation

Nr.	Treaty name	Province
1	Blood Tribe Governance and Child Welfare Agreement-in-Principle	AL
2	Lheidli T'enneh	BC
3	Maa-nulth First Nations Final Agreement	BC
4	Nuu-chah-nulth Tribal Council	BC
5	Sliammon First Nation	BC
6	Tsawwassen First Nation Final Agreement	BC
7	Yekooche First Nation	BC
8	Denesuline	MB
9	Sioux Valley Dakota Nation	MB
10	Mi'kmaq - Nova Scotia - Canada	NS
11	Deh-Cho First Nations	NT
12	Akwesasne	ON
13	Algonquins of Ontario	ON
14	Anishinabek Nation/Union of Ontario Indians	ON
15	Anishnaabe	ON
16	Mamuitun and Nutashkuan	QC
17	Micmac Nation of Gespeg	QC
18	Mohawks of Kahnawake	QC
19	Athabasca Denesuline	SK
20	Meadow Lake First Nations	SK

Table C.2: Determinants of treaty implementation

	Treaty implemented	
	(1)	(2)
ln(mean household income)	0.168	0.350
	(0.109)	(0.242)
Log of income ratio 90th and 10th percentile	0.007	0.017
	(0.005)	(0.017)
% urban households	-0.216**	-0.228
	(0.085)	(0.140)
ln(population)	-0.003	-0.080**
	(0.025)	(0.033)
% registered Indians	0.096	0.138
	(0.140)	(0.198)
% high school or above	-0.132	0.076
	(0.412)	(0.756)
% workers in extractive industries	0.029	1.770***
	(0.264)	(0.667)
% workers in public services	0.684	0.957
	(0.555)	(0.684)
% workers in non-public services	-0.171	-0.764
	(0.334)	(0.523)
Sample	BC, QC, NT, and YK	Treaty impl. or under negot.
Year	1991	1991
Nr. of obs.	238	104
R-squared	0.072	0.298

Notes: Robust standard errors in parentheses. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. The unit of observation is a First Nation band in 1991. To obtain this dataset, I aggregated micro-data at band level. The aggregation was done using sample weights. The regression is estimated using the number of original observations as analytical weights. *Treaty implemented* is a dummy equal to 1 if band implemented a treaty in period 1991-2005. Columns 1 uses the sample of bands in provinces and territories with at least one treaty band. Column 2 restricts the sample to bands that, between 1991 and 2006, implemented a treaty or had a treaty under negotiation. BC: British Columbia, QC: Quebec, NT: Northwest Territories, YK: Yukon.

Table C.3: Treaties and nominal income

	ln(nominal household income)		
	(1)	(2)	(3)
Treaty implemented	0.141***	0.121**	0.117***
	(0.047)	(0.047)	(0.044)
Sample	BC, QC, NT, and YK		Treaty impl. or under negot.
Band fixed effects	Yes	Yes	Yes
Non-parametric trends	No	Yes	No
Weighted nr. of obs.	144,165	135,195	87,075
R-squared	0.276	0.280	0.318

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The set of controls and sample definition are similar to the baseline regression (see notes of Table3).

Table C.4: Treaties and real income, using sample of less mobile population

	ln(real household income)	ln(real ind. Income)
	(1)	(2)
		[t]
Treaty implemented	0.111**	0.408***
	(0.043)	(0.118)
Sample	Households in which all members lived in same CSD 5 years ago	Individuals who lived in same CSD 5 years ago
Weighted nr. of obs.	102,035	230,215
R-squared	0.303	0.312

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. All regressions include band and year fixed effects. The set of controls and sample is similar to the baseline regressions (see notes of Table 3).

Table C.5: Treaties and housing characteristics

	Owns house (1)	Dwelling needs major repairs (2)	Number of rooms (3)	Persons per room (4)
Treaty implemented	0.014 (0.018)	-0.014 (0.015)	-0.048 (0.062)	-0.018 (0.017)
Weighted nr of obs.	144,500	144,500	144,500	144,500
R-squared	0.324	0.100	0.223	0.644

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. All regressions include band and year fixed effects. The set of household controls and sample is similar to the baseline regression (see notes of Table 3).

Table C.6: Treaties and real income, excluding two major treaties

	ln(real household income)			
	(1)	(2)	(3)	(4)
Treaty implemented	0.142*** (0.045)	0.104** (0.041)	0.162*** (0.046)	0.172*** (0.045)
Sample	Excl. Nisga'a treaty	Excl. JBNQ agreement	Excl. both treaties	Treaty impl. or under negot. excl B.C.
Weighted nr. of obs.	134,945	127,530	125,370	75,835
R-squared	0.288	0.272	0.273	0.353

Notes: Robust standard errors in parentheses. Standard errors are clustered at band-year level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include band and year fixed effects. The set of controls is similar to the baseline regression (see notes of Table3). All regressions use the sample of bands in provinces and territories with at least one treaty band (BC, QC, NT and YK) but exclude bands that signed some treaties. Column 1 excludes signatory bands of the Nisga'a treaty, while column 2 excludes signatory bands of the James Bay and Northern Quebec (JBNQ) Agreement. Column 3 excludes both groups of bands. Column 4 restricts the sample to bands that, between 1973 and 2006, implemented a treaty or had a treaty under negotiation but excludes bands in British Columbia (BC).