University of Victoria | Economics

Department Discussion Paper DDP1703

ISSN 1914-2838

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We study the effects of Canadian Indian residential schooling on two anthropometric measures of health during childhood: adult height and body weight. We use repeated cross-sectional data from the 1991 and 2001 Aboriginal Peoples Surveys and detailed historical data on school closures and location to identify the effect of residential schooling on these outcomes. We find some evidence that on average residential schooling increases adult height and decreases adult body weight of those that attended. However, these effects are concentrated after the 1950s when the schools were subject to tighter health regulations and students were selected to attend residential school based partly on their need for medical care that was otherwise unavailable. Residential schooling is only one policy in Canada that impacted Status Indian peoples' health, so our results must be understood in the broader social context. We also document significant increases in height and body weight for Status Indian people born after the 1960s which is suggestive of non-trivial changes in diet and living conditions during this time period.

Keywords: Indigenous peoples, residential schools, health, stature, weight, identity, schooling.
JEL Classifications: I12, I14; I15; I18; N32

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The Effect of Indian Residential Schools on Height and Body Mass Post-1930

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Acknowledgments. We thank Statistics Canada for access to confidential data. Auld thanks the Center for Addictions Research of British Columbia (CARBC) for financial support. We are grateful for the helpful comments from numerous seminar participants at the 2014 Canadian Economics Meetings and for the constructive feedback from our colleagues at Queen's University, Simon Fraser, the University of British Columbia, and the University of Victoria. Any errors or omissions are our own.

1 Introduction

Up until the end of the twentieth century, governments in the United States, Canada, and Australia forcibly removed Indigenous children from their communities and placed them in racially segregated boarding schools. In Canada, these schools were known as Indian residential schools. The policies associated with these residential schools resulted in the largest class action settlement in Canadian history and the establishment of the Truth and Reconciliation Commission of Canada[1]. While previous research has demonstrated that attendance at Indian residential schools increased children's eventual educational and labor market outcomes on average, it came at the cost of significant losses in traditional languages and cultural practices.[2] Poor standards of living were also well-documented in residential schools, particularly in the middle to late 1800s and early 1900s (Bryce, 1922; Lux, 2001; Milloy, 1999; Mosby, 2013; Truth and of Canada, 2015). It has been suggested that Indian residential schools have played a significant role in the relatively poor average health outcomes of Indigenous people in Canada today (Akee and Feir, 2016; Gracey and King, 2009; King et al., 2009; Loppie and Wien, 2009; Smith et al., 2005; Truth and of Canada, 2015; Waldram et al., 2006). However, the ultimate effect of residential schools on the health of the children who attended is not obvious given the social context in Canada at the time, and is yet to be established by rigorous quantitative analysis.

During the era of residential schooling, Indigenous populations were coping with the fallout of European colonization more broadly, including the dramatic loss of traditional food sources, numerous exclusionary policies, and a severe lack of access to medical care and quality housing (Waldram et al., 2006). In addition, the environments within residential schools began to change in the late 1950s with a change in public opinion after the Second World War and the Federal approach to governing more generally; child labour, previously common in the schools, was prohibited and stricter health regulations were implemented as the federal government began to play a more active role in Indigenous education (Miller, 1996; Milloy, 1999). Deaths from communicable disease also fell sharply after the introduction of vaccines for tuberculosis, changing the disease environment both within residential schools and in Indigenous communities. Finally, more explicit regulations about which students were to attend residential schools were implemented in the 1960s. These selection procedures enrolled children in residential schools that

[1] See Riemer (2010) and the Truth and Reconciliation Commission (TRC) (2015)

[2] See Feir (2012) - published later as Feir (2016), Jones (2013), and Gregg (2016).

non-Indigenous authorities deemed to be the "most vulnerable" and explicitly included children with chronic conditions who otherwise would not have access to medical care (Armstrong, 1969, 1-2). Thus, the net effect of residential schooling on the physical health of the children that attended in this context is unclear.

In order to better understand the effects of residential schooling in the midst of the broader challenges faced by Indigenous communities in Canada at the time, we pool the confidential data files from the 1991 and 2001 Aboriginal Peoples Survey (APS) and study the relationship between residential school attendance and adult height and body weight for Status Indians.[3] Both height and body weight may be interpreted as proxy measures of the biological standard of living experienced during childhood such as nutrition, sanitation, exposure to disease, and workload (Steckel, 2008).[4] We interpret an effect of residential schooling on height as indicative of the biological adversity faced by children in residential schools relative to their counterfactual environments. While height is relatively cleanly linked to childhood environmental conditions, adult body weight partially reflects conditions in childhood and later life behaviors, acculturation, and conditions.[5] We begin to disentangle the extent to which residential school affects each of these dimensions. In addition, pooling the confidential data files from the 1991 and 2001 APS allows us to examine trends in Status Indian height and body weight during the mid–twentieth century. To our knowledge we are the first to map trends in Status Indian height and body weight over this time period. We find large increases in adult height for those born in the 1960s and 1970s, along with increases in mean BMI exceeding similar trends in the general population, suggestive of large changes in Status Indian living conditions over this period (Bozzoli and Quintana-Domeque, 2009; Katzmarzyk, 2002b).

We use detailed historical data on school opening, closing, and location as a source of plausible exogenous variation in residential school attendance to identify the local average treatment effect of residential schooling on height and body weight.[6] The use of residential school opening

[3]Status Indians are individuals identified as "Indian" under the *Indian Act* of Canada and were the primary population that could be compelled to attend residential schools.

[4]It is important to highlight that we only study the effects of residential schooling on those born after 1930. This implies that our results are conditional on the effects of residential schooling on generations born before 1930.

[5]Given the TRCs documentation of the mental health, trauma, loss of culture and suffering in residential schools, these later life behaviours might differ significantly for residential school attendees (e.g. substance abuse, marriage/divorce rates, institutionalization, etc.) all of which may have direct and indirect effects on outcomes such as body weight.

[6]Our identification strategy is most similar to Feir (2012) and Feir (2016), but the models that use school opening and closure also akin to those of Jones (2013) and Gregg (2016).

and closing as exogenous variation implicitly exploits the top-down institutional nature of the residential schooling system: the dates schools were opened or closed and the distance from the community to the school were not under control of the affected communities but rather dictated by governments and churches. We believe we are the first to investigate the long run consequences of residential schooling on adult height. We also go beyond previous literature on residential schooling and adult BMI in two significant ways.[7] First, by pooling the 1991 and 2001 confidential waves of the APS, we are able to disentangle age and cohort effects which are important in studying outcomes such as height and BMI. Second, we allow for the effect of residential school attendance to vary along personal characteristics, cohort, and location. Allowing for heterogeneous treatment effects is an important extension given the substantial changes in conditions within residential schools over time and the potentially significantly different counterfactual conditions across the country.

In line with historical accounts of student selection, we find that Status Indian children who would have otherwise been shorter and heavier as adults were more likely to be selected to attend residential schools (Miller, 1996; Milloy, 1999). Once this selection is accounted for, we find some evidence that residential schooling increased adult height by a half an inch to an inch and stronger evidence that it decreased mean BMI by about 0.8 units. These effects do not appear to be substantially mediated by education, income, and cultural attachment, suggesting that the increase in height and reduction in BMI resulted directly from conditions in residential schools affecting health. The smallest observed increase in height is roughly equivalent to half the increase in height observed among the average American man between 1900 and 1950 (Steckel, 1995).

Despite the uncertain effects of residential schools, the positive local average treatment effects on our measures of adult health are somewhat surprising. We explore these results further by relaxing the assumption of a constant treatment effect, and we find notable heterogeneity in the effects of attending residential school. Specifically, we find that the effect of residential school varies substantially by the era in which children attended. Any positive causal effect of residential schooling on height and BMI is concentrated after the 1950s and is essentially non-existent in earlier decades. This result is consistent with increased funding, tightened health

[7] Jones (2013) examines the effect of an additional year of having a residential school open within 500 kilometers of a community when the respondent is of schooling age on body weight and finds no effect.

regulations, increased monitoring, and increased access to medical care in residential schools starting in the late 1950s (Milloy, 1999). It is also plausible that changes over time in the unobservable characteristics of residential school students results in the effects of residential school being larger in later periods. For example, during the 60s and 70s the percentage of residential school students deemed as "neglected" by non-Indigenous authorities increased and was as high as 80 percent in some schools (Milloy, 1999).[8].

One concern is that our post-1950 results are driven by higher mortality in residential schools. We argue that this is unlikely based on mortality data collected by the Truth and Reconciliation Commission of Canada (2015). However, it is plausible we are unable to identify the effect of residential school before the 1950s not only due to plausibly higher rates of mortality in residential schools but also due to the importance of communicable diseases in the health of Status Indians before the development and distribution of tuberculous vaccines (Waldram et al., 2006). The prevalence of diseases like tuberculous before the 1950s likely impacted not just those that attended residential schools, but whole communities. If this is true, then comparing those who attended with those who did not will under-estimate the consequences of residential school due to the stable unit value treatment assumption implicit in our estimation strategy being violated. However, assuming that the timing of closure of residential schools after the 1950s is conditionally uncorrelated with within community trends in height and BMI, we can interpret our findings after 1950 as the local average treatment effect of residential schools on those who attended. This suggests that Status Indian health today would be significantly better had the federal government either improved health conditions in residential schools sooner or improved access to medical care and health conditions outside residential schools more rapidly during this time period.

In addition to understanding the legacy of residential schools in the broader history of Canada, our results also contribute to the literature on the role of childhood health in determining adult health by examining the effects of a *later* childhood policy intervention. Most of the literature in this area has focused on the role of *early* childhood health conditions and has found these conditions to be fundamental to adult health and thus educational and labor market outcomes. Notable contributions to this "early origins" hypothesis in economics include Cunha

[8]What non-Indigenous authorities deemed to be neglect was often tightly linked to material conditions like housing which the federal government was technically responsible for providing (TRC, 2015)

and Heckman (2007), who argue that health and human capital are strongly complementary and thus children who suffer early negative health shocks do not catch up to their peers, and Conti et al. (2010), who demonstrate that early health endowments are important causes of socioeconomic outcomes in early adulthood.[9] While the literature on the effects of later childhood health interventions on adult health is smaller, a notable contribution is Akee et al. (2013) who studies the effect of substantial cash transfers given to American Indian households with older children on the children's later life BMI. They find that among poorer households, interventions that start as late as adolescence have medium term consequences on BMI. We contribute to this literature by investigating the effects of relatively late shocks in childhood health, education, and acculturation on adult health outcomes. Like Akee et al. (2013), our findings provide evidence that, at least among the most marginalized children, later childhood interventions can have substantial long run effects on health.

In the next section we review a brief history of residential schooling in Canada and discuss the evidence on childhood environments and later life height and weight. In Section 3 and 4 we discuss our data and identification strategy. In Section 5 we provide the main results and conduct a number of robustness exercises. The last section concludes.

2 Background

2.1 Residential schooling in Canada

Between the early 19th and the late 20th centuries, an estimated 150,000 Indigenous children attended residential schools in Canada (TRC, 2015). The first long standing residential school was established in 1834 with the last residential school closing its doors in 1996 (TRC, 2015).[10]

Figure 1 shows the distribution of residential schools across the country during the peak of the system in 1930. The flags represent the locations of residential schools and the dots indicate the centroid of Indigenous communities included in the 1991 Census. It can be seen

[9] See Currie and Almond (2011) for a review of research in economics on the effects of early childhood health outcomes on later life health and socioeconomic outcomes.

[10] The final year of closure of the last residential school is a matter of debate. The date 1969 is used as the end date because it marks the end of formal Church involvement in the system. However, the federal government continued to run residential schools into the 1980s and did so at the behest of some First Nations into the 1990s. Some cite the date of closure of the system to be 1986 when the last school was transferred to the First Nations it served (**?**), while others use the date of closure of Gordon Residential in 1996 as the formal date of closure (TRC, 2012).

in Figure 1 that in 1930, residential schools existed in nearly every province in Canada with the exception of Quebec, Newfoundland, and Prince Edward Island. Although 139 residential schools are officially recognized, many more may have existed. During the system's peak, over 60 residential schools existed, and between 1930 to 1945 about half of Registered North American Indian students attended a residential school.

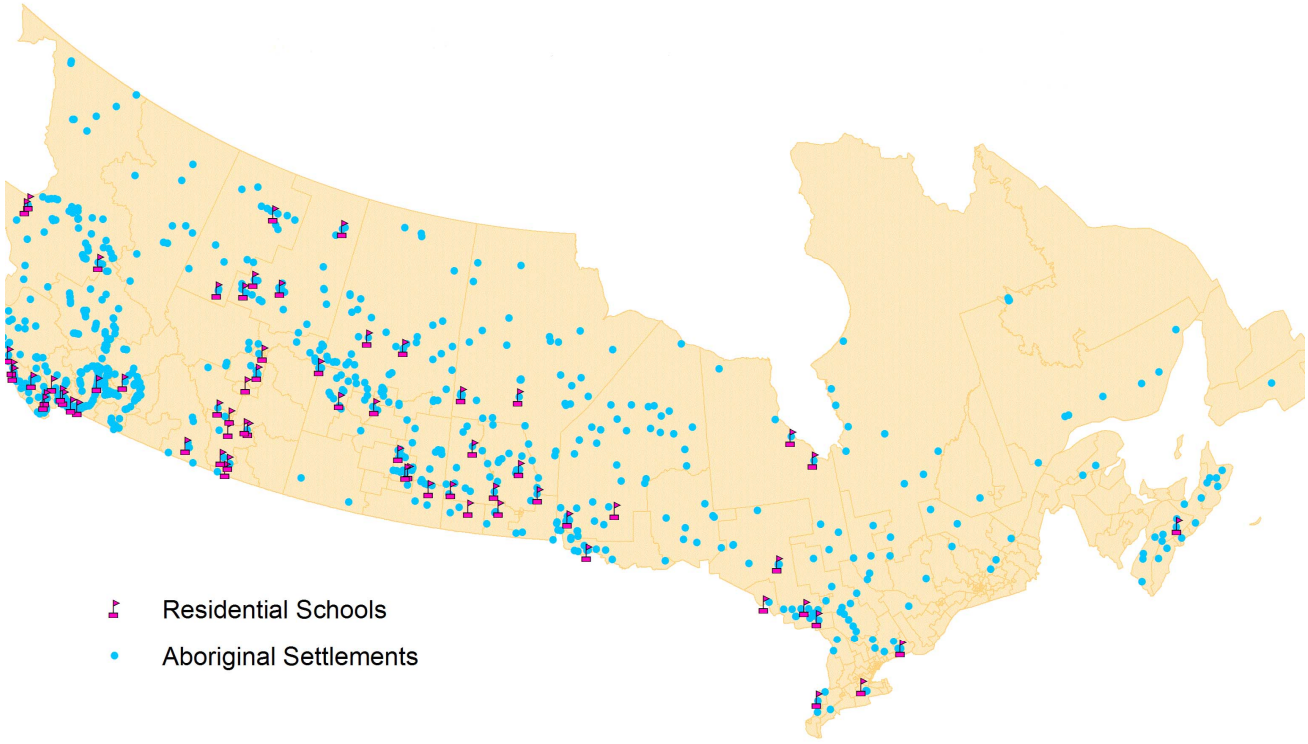

Figure 1: *Data on Indigenous settlements and positions of residential school locations compiled from geographic sources cited in the geographic references Section. Data on residential schools compiled from "Where are the Children" by the Legacy for Hope Foundation. This source can be found at http://www.wherearethechildren.ca/. Last Accessed September 28, 2012.*

Perhaps the most morally neutral description of the residential schooling system is "the institutionalized means by which a dominant society seeks to transmit a body of information, including both formalized subject matter content and social norms" (King, 1964). For most of the system's history, missionary organizations were responsible for the establishment and operation of residential schools. The Canadian federal government began its formal involvement in 1892 by providing funding to the schools on a per capita basis. In 1911, the government further involved itself in the system by setting out a basic set of regulations for the operation of the residential schools and enforced attendance at the schools (Milloy, 1999).

While some residential schools were located within Indigenous communities, some were hun-

dreds of kilometers away. Although children were permitted to return home for summer vacation starting in 1920, some children who were taken extraordinary distances did not see their families for years.[11]

Unlike day schools on reservations or public schools, the residential schooling system operated on a half day system until the 1950s. Half the day was spent in academics, culture and religion and the other half in manual labour (Grant, 1996; Gresko, 1979). Before 1951, student labour was used to make up for school budgetary short falls. In 1957, the federal government changed the funding scheme for the residential schools from a per capita system to a cost based system and increased funding more generally (Miller, 1996).

This increase in funding came after the gradual decline of the residential schooling system began. Figure 2 shows that after the end of the Second World War, there was a sharp change in government enforcement of attendance at residential schools. Figure 2 shows that while residential schools accounted for over 50 percent of enrollments in schools in 1945, they accounted for less than 20 percent by 1965. This change in enforcement was largely attributable to the findings of the Reestablishment and Reconstruction Commission designed to survey the state of Canadian affairs after the war. The Commission's recommendation that residential schools be shut down was based on the testimony of government officials and Indigenous representatives (Leslie, 2002). However the closure of the system was protracted. This was not due to slow moving government selective school closure, but rather the political resistance faced by the government from the religious organizations that ran the schools (Milloy, 1999). Especially intense resistance was encountered from the Catholic Church, which looked on non-secular and Protestant education with contempt (Hawthorn, 1967; Miller, 2004). The federal government believed the churches' attitudes "act[ed] as a brake on the development of Indian education through the stress they place[ed] on their own privileges and on the dangers which school integration presents to faith and morals," (Hawthorn, 1967, 62). There was major opposition from the Catholic Church to public integration and the closure of residential schools in provinces that did not provide for public Catholic schools (Milloy, 1999, 220).

The government forced the churches out of the official operation of the schools in 1969 and school closure rapidly increased (TRC, 2015, 72). Government enforcement of residential school

[11] See McFarlane (1999); Miller (1996) for examples of this. It is worth noting that we will not be able to identify the effect of attending residential school for children who were taken these great distances since closures of schools far away have little impact on attendance rates in communities on average.

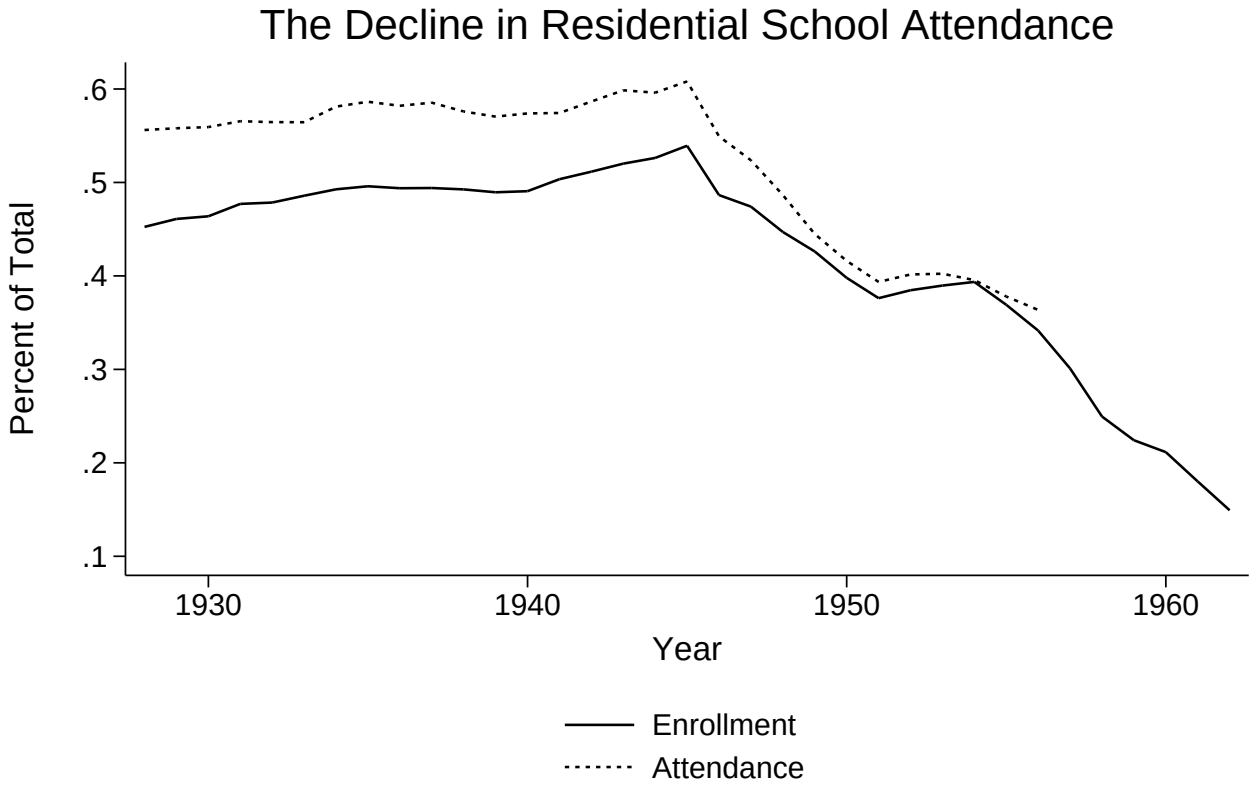

Figure 2: *These calculations were made using the 1941 to 1980 Indian Affairs Reports. This figure also appears in Feir (2012) and Feir (2016).*

attendance was never random, but the specialized selection of children to attend residential schools likely increased during the period of closure. Although the *Indian Act* made school attendance mandatory for all Indian children between the ages of seven and fifteen, children could be forced to attend residential schools if they were "of the kind required" (*Indian Act* 1920, Section 10). Before 1969, informal policy at the discretion of Indian Agents and eventual regulations resulted in the residential schools being primarily operated for "orphan children, children from broken homes and those who because of isolation or the migratory way of life of their families are unable to attend day schools" (Administration of Indian Affairs 1964, 44). In 1969, the director of the Indian Affairs Operations Branch set clear guidelines to schools operators and Indian Agents that only Indigenous students who met at least one of six requirements could be admitted to a residential school: 1) Home is isolated and removed from day school services; 2) Parents or guardians are migratory; 3) Problems in the home; 4) The handicapped student who has a chronic condition, but can live in a student residence and obtain regular medical follow-up which would be difficult to obtain in the home area; 5) Students who require a period of adjustment to urban living through living in a residence with peers who share his culture –

that is, a student who requires a gradual orientation to urban living before he can manage in a private boarding home in the community; 6) No suitable private boarding home is available in the area in which the appropriate school is located (Armstrong, 1969, 1-2). Categories 1, 2, 3, and 4 applied to students up to 14 years of age, while categories 3, 4, 5, and 6 applied to students 15 year of age and over. If children were not taken to a residential school in this time period, they either attended an integrated provincial public school from home, a school in their community, or lived in a white boarding home while attending a provincial public school. In the 1960s and 1970s, 50 to 80 percent of the children who attended these schools were deemed to be neglected (Milloy, 1999). Thus, particularly as of the 1960s, children selected to attend residential schools were likely in worse health or more likely subject to adverse health shocks, than children not selected into residential school.[12]

2.2 Health and nutrition in residential schools and in Indigenous communities

The causal effect of residential schooling on height and body weight depends on the biological standard of living in residential schools relative to the standard in which the children who attended would otherwise have lived. We briefly survey the sparse evidence that exists on nutrition and exposure to disease in both residential schools and Indigenous communities.

Residential schools were generally reported to have unacceptable health conditions. In 1907 and 1909, a special investigation of 35 residential schools in the Prairie Provinces found high death rates among children who had attended these schools. One of the most extreme cases described 75 percent of all individuals to attend a given residential school in its 16 year existence were dead at the time of survey (Bryce, 1922, 4). Follow up studies approximately 10 years later found as many as 75 percent of children at a given school were infected with tuberculosis and as many as 60 percent had aggregated scabies or itch which was commonly found in children in crowded and unhygienic living conditions (Milloy, 1999, 99). These sub-standard conditions were characteristic of living conditions in residential schools prior to our sample frame (Miller,

[12]The one exception to this is children who were from more culturally traditional households. These children who were selected to attend residential schools may have been in better health than their peers. It is also worth noting that children who were selected to attend residential school because of "problems in the home" could actually be the product of their earlier residential school system: early residential schools may have resulted in worse outcomes of future parents which may have lead their own children to be more likely to have attended residential school.

1996). At least up until reforms began to be implemented in 1957, children placed in residential schools were often subject to severe undernourishment; hunger was a "continual and systemic problem" (Milloy, 1999), and vitamin and iodine deficiency were not uncommon (Mosby, 2013). Before the reforms, the diets of children in one residential school were estimated at only 1,500 calories a day, most of which were from potatoes and bread (Milloy, 1999, 267). After the departmental reforms to supervision practices and funding of residential schools in 1957 there were improvements in health conditions. Indian Health Services dietitians inspected the schools more regularly than had ever been the case before and give detailed menu planing and advice and on-site training to staff based on Canada's food guide (Milloy, 1999, 277). There were also increases in funding in in 1957, 1962, and again in 1969 (Milloy, 1999, 273).[13]

The effect of residential schools on the health and development of children who attended depends on the alternate environment these children would have faced. Comprehensive quantitative evidence on nutrition and disease in Indigenous populations in Canada during this period is not available, but case studies of many communities suggest that conditions in Indigenous communities were often dire. During the late 18th to mid-19th century, Indigenous communities were subject to over-crowding, inadequate medical care, poor sanitation, nutritional deprivation, and inadequate clothing (Lux, 2001, 107). Hunger may have been even worse in many native communities than in residential schools. Moore et al. (1946) estimated that the average daily intake in the Norway House and Cross Lake First Nations was only 1,470 calories. This is about 100 calories per day less than consumed by men in the Minnesota Starvation Experiment. The previously healthy men included in this experiment suffered from effects such as anemia, lower extremity edema, muscle wasting, weakness, neurological deficits, dizziness, irritability, lethargy, and depression (Mosby et al., 2013). Further, the vast majority of these calories "were supplied by white flour, lard, sugar and jam." These foodstuffs are lacking in basic nutritional value and thus increase propensity for tuberculosis and other disease when consumed in isolation. Moore et al. (1946) reported that northern Cree communities were subject to tuberculosis death rates of 1,400 per 100,000, in comparison to 27 per 100,000 for the non-Indigenous local population. Overall, mortality rates in the Cree community were 39 per thousand, whereas in the general population they were eight per thousand. However, the evidence is not completely consistent.

[13]These improvements did not bring the residential school system up to the standard of funding of the public provincial school system for non-Indigenous children (TRC, 2015).

Some sources suggests that from 1919 to 1953 in Manitoba and Saskatchewan, children who entered residential schools were not any shorter than non-Indigenous Canadian children of the same age (Hackett et al., 2016).[14]

The conditions in Indigenous communities during the 20th century are often held in stark contrast to the conditions in early and pre-colonization (Kelm, 1998; Waldram, Herring, and Young, 2006). For example, Steckel and Prince (2001) argue that some Indigenous groups in North America were healthier than the average Europeans during the same time period. Thus it is important to keep in mind when interpreting the results regarding "the effect" of residential schools, that the alternative environments to children faced were already deeply impacted by colonization.

2.3 Childhood nutrition and adult body weight and height

In this section we briefly survey medical evidence on the effects of childhood malnutrition and other adversity on adult height and body weight.

Adult height is widely used as a marker of important aspects of standard of living.[15] In addition, Case and Paxson (2008) have argued that both height and cognitive ability result from common biological pathways, explaining the height-wage gradient. Schick and Steckel (2010) add that non-cognitive development may result from the same pathways.

These arguments are consistent with the medical literature documenting the relationship between early life development and adult health. Inadequate nutrition and exposure to infection early in life is associated with shorter adult stature (Li et al., 2003). This literature largely focuses on intrauterine and very early (birth to age two) growth stunting, but some evidence suggests that later childhood nutrition can induce "catch up" growth which counters early stunting (Victora et al., 2008). Nutrition, exposure to disease, and living conditions in residential schools may then have stunted or accelerated growth depending on the relative conditions in native communities at the time.

Early childhood nutrition and disease have a complex relationship with adult body weight. The medical literature proposes that early shocks may predispose the body to being shorter and overweight in adulthood (Barker, 1990). Early life under-nutrition may alter insulin secretion,

[14] However, this may be due to genetic differences between Status Indian children and Canadian children.

[15] Influential papers in the economics literature using height as a proxy for childhood standard of living include Case and Paxson (2008, 2010); Deaton and Arora (2009); Steckel (1995); and Strauss and Thomas (1998).

alter the number and size of fat cells and tissue function, and lead to changes in the regulation of appetite through central nervous system disruption (Martorell et al., 2001). Through this mechanism, the effect of early nutrition on the probability of being overweight or obese as an adult is thought to be U-shaped, with both undernourished and over-nourished children experiencing a higher probability of obesity as adults (Eckhardt, 2006). Communicable disease infections during childhood may also induce the effects found at the low end of the nutritional scales: net nutrition during childhood is the difference between intake and expenditures, and disease both increases demands and can reduce absorption of nutrients (Bozzoli and Quintana-Domeque, 2009; Silventoinen, 2003).

Evidence presented in both the social science and medical literatures, then, suggests that under-nutrition early in life may lead to both diminished stature and higher adult BMI. If residential schools had access to more resources and provided more nutrition than the child would have had available otherwise, attendance at a residential school would increase adult height and may decrease adult BMI.

3 The Aboriginal Peoples Surveys (1991 and 2001)

We study the effect of residential schooling on stature and body weight using the 1991 and 2001 confidential waves of the Aboriginal Peoples Surveys (APS) Adult Retrieval file. These data sets are the only data sets we are aware of that survey both the on-reserve and off-reserve population and include detailed questions on demographics such as height and body weight, geography, labor market and cultural outcomes, and residential school attendance.

The APS sample was derived from the census population that answered the long form questionnaire. The long form was given to 20 percent of households off reservation and 100 percent of people on reservation. All those that claimed Indigenous ancestry and/or that they were registered under the Indian Act were eligible to be surveyed for either the 1991 or 2001 APS. Responses to both surveys were voluntary and had response rates close to 80 percent.

Those in the 1991 APS sample were further required to "identify" with their Indigenous origins. The identification question was: "With Which Aboriginal group do you identify? North American Indian, Inuit, Métis, Another Aboriginal group?" If they didn't identify with an Abo-

riginal group, they were asked, "Are you a registered Indian under the Indian Act of Canada?"[16] If they said no, they were excluded from the survey.

The 2001 APS population differs from the 1991 sample in that it includes those who have Indigenous ancestry, but do not identify with an Indigenous group. However, the response rate for individuals with Indigenous ancestry who did not identify with an Indigenous group is much lower than for those that have Indigenous ancestry and do identify with an Indigenous group (68.6 percent compared to approximately 85 percent). The 2001 APS also differs in the Indigenous communities that were selected to participate in the survey. The 1991 APS was designed to be representative of all Indigenous communities, while the 2001 APS surveyed only 123 of the largest First Nations communities (reserves). The survey also focused on 52 Inuit communities, 38 communities with a concentration of 40 percent or more Indigenous peoples (28 of these communities are predominately Metis), and five additional communities with large numbers of Indigenous peoples (Prince Albert, North Battleford, Wood Buffalo, Yellowknife, and Whitehorse). While in most provinces these communities cover between 50 to 55 percent of the on-reserve population, there is notably less coverage of those living on reserve in British Columbia due to the large number of small reserves and the high cost of sampling. To deal with this, we restrict the number of observations per census division to be greater than 100 people and drop one division that is not present in both surveys.

A disadvantage to the 1991 APS is that separate residential schooling questions were asked to those between the ages of 50 and 64 and for those between 15 and 49. Comparability of the responses for these questions may be an issue. The question asked to those between 50 and 64 was, "Did you ever attend a residential school?" The question to those less than the age of 49 asked first whether an individual attended a single elementary school or multiple elementary schools. Then they were asked, "Where did you live while attending school: a) lived with family while at school; b) lived with a non-Indigenous family while at school c) lived at a residential school d) lived somewhere else." This process was then repeated for high school education if respondents attended high school. We use all of these sub-questions to create a single indicator of whether an individual ever attended a residential school. Anyone over the age of 65 was not asked any questions regarding their education. The question asked in the 2001 APS was consistent with the question asked of those between the ages of 50 and 64 in the 1991 APS.

[16] "Status Indian" and "Registered Indian" are equivalent.

We restrict our sample to include those individuals who are registered under the Indian Act, are members of an identified band, and live outside Quebec and the Atlantic provinces. We limit our sample to those who are registered under the Indian Act and are members of a band, since these are the individuals that the residential schooling system was designed for. We restrict the location of initial place of residence to Western Provinces and Ontario because residential schooling was established in Quebec, the Atlantic Provinces and the territories much later with different institutional histories. The sample is restricted to those younger than 65 since anyone older was not asked schooling questions in the 1991 APS. Finally, we drop observations with a reported BMI under 12 or over 80 since these reports seem implausible. This restriction results in a little over 10 percent of the sample being dropped.

Given the confidentiality rules for usage of the data, we are unable to present unconditional means of summary statistics. Instead, Table 1 contains conditional summary statistics for those who attended residential schools and those who did not using OLS regression. In each specification, the dependent variable is equal to one when the respondent attended residential school and zero otherwise. Approximately 25 percent of the sample in 1991 attended residential school. Conditional on other characteristics, respondents to the 2001 APS were somewhat less likely to report attendance than 1991 respondents, by about three to seven percentage points depending on covariates. This result may imply that the difference in survey questions could have resulted in different response rates. We address this potential issue by ensuring that we control for survey year in all specifications. On average, women attended residential school as often as men. Those whose mother tongue was an Indigenous language were far more likely to attend than those whose mother tongue was English or French. Those with only Indigenous origins (those who solely listed North American Indian as their ancestry rather than multiple ancestries) were also heavily selected into residential school. Individuals who attended residential school were also more likely to have lower total incomes later in life, and less education. These patterns are consistent with the historical literature documenting selection into residential schools. Migratory families were arguably more connected to traditional culture, while orphaned children and those from what were deemed to be "neglectful" homes from the perception of non-Indigenous authorities are also ex-ante more likely to be less educated and have lower incomes later in life.

4 Econometric methods

4.1 Single–equation descriptive estimates

We estimate the effects of residential schooling on height and body weight. Let y_{it} denote a measure of height for respondent i in year t, where $t \in \{0, 1\}$ denotes either the 1991 or 2001 APS. Let R_{it} be a dummy indicating the respondent attended residential school and let X_{it} denote a vector of "pre-treatment" exogenous determinants of physical health—variables which reflect the respondent's condition at the time of selection into residential schooling—including sex, birth year, a dummy for survey wave, geographic variables such as region and latitude, and whether the respondent's mother tongue is Indigenous, a measure of acculturation in the respondent's home. Let W_{it} denote a vector of adult outcomes, including measures of eventual educational achievement, labor market income, and acculturation as proxied by on-reserve status, whether a native language is spoken in the home, and whether the respondent visits a traditional healer. We begin by estimating models of the form

$$y_{it} = X_{it}\beta + \theta R_{it} + u_{it}, \tag{1}$$

by ordinary least squares (OLS), where β and θ are parameters to be estimated, to document partial associations between residential schooling and adult height. We also use OLS to estimate models which condition on later-life outcomes W. These later-life outcomes denoted by W may be influenced by residential schooling,

$$y_{it} = X_{it}\beta + W_{it}\pi + \theta R_{it} + u_{it}. \tag{2}$$

Changes in estimates of θ across these two specifications provide suggestive evidence on the degree to which the correlation between residential schooling and the dependent variable can be "explained" by early or late-life experiences; however, these estimates do not recover causal effects as selection into residential schools was not random conditional on X, and W are "bad controls", as they may be on the causal path from residential schooling to adult health outcomes.

4.2 Modeling selection into residential schooling

OLS and other single-equation estimates are likely to be inconsistent due to selection bias. As discussed above in the background section, unobserved determinants of adult height, u_{it}, also likely determine selection into residential schools. The historical evidence suggests this process is likely to manifest as negative selection bias: the children selected into residential schools in the 1930s through the 1980s, our sampling window, would likely have been in worse health as adults than observably identical children who were not selected into residential schools. OLS estimates will then tend to spuriously indicate that residential schooling is more deleterious, or less beneficial to health, than it actually was. To address selection bias, we instrument for the residential schooling dummy R using a dummy indicating that the closest residential school within 30km of the respondent's community was open when the respondent was of schooling age (on average, between age seven and fifteen).[17] Let Z_{it} denote this variable. Residential schooling outcomes are modeled,

$$\begin{aligned} R^*_{it} &= X_{it}\pi_0 + \alpha Z_{it} + \epsilon_{it} \\ R_{it} &= 1[R^*_{it} > 0], \end{aligned} \tag{3}$$

where R^*_{it} denotes the latent propensity to attend residential schools, and α is a parameter and π_0 is a vectors of parameters to be estimated. Note that X_{it} includes a full set of census division fixed effects as well as birth year fixed effects. These are important in order to control for unobserved differences across communities that may be correlated with residential school attendance and aggregate trends in health by birth year. We assume the error terms u and ϵ are jointly normal and estimate equations (1) and (3) simultaneously using full–information maximum likelihood (FIML). As with the OLS models, we also estimate variants in which we include adult educational, labor market, and cultural outcomes W as additional covariates, interpreting changes in the estimates of θ as reflecting mediation through these channels.

[17] "Schooling age" is defined by the compulsory schooling age legislated by the *Indian Act* which change over time. If the provincial compulsory schooling ages were more stringent than the ages in the Indian Act, the provincial compulsory schooling ages are used. The results are unchanged if constant schooling ages of seven and fifteen are used.

4.3 Modeling heterogeneous treatment effects

We relax the implicit assumption in the models presented above that the causal effect of residential schooling on weight or stature is common to all attendants. We estimate treatment effect specifications in which we model the effect of residential schooling as itself depending on observed and unobserved characteristics.[18] This model may be represented as,

$$\begin{aligned} y_{it}^1 &= \gamma_1 + [X_{it} - \mu_X]\beta_1 + u_{it}^1 \\ y_{it}^0 &= \gamma_0 + [X_{it} - \mu_X]\beta_0 + u_{it}^0 \\ R_{it} &= 1[X\pi_0 + Z_{it}\pi_1 + u_{it}^R > 0], \end{aligned} \tag{4}$$

where μ_X is the sample mean of X_{it}, $1[\cdot]$ is the indicator function, the γ are constants, y_{it}^1 denotes a continuous health outcome respondent i would experience if she was sent to residential school, and y_{it}^0 the outcome this respondent would experience if she was not sent to residential school, such that $\Delta_{it} = (y_{it}^1 - y_{it}^0)$ is the causal effect of residential schooling for respondent i, the u are outcome-specific unobserved causes of health, and as in previous models, R_{it} is a dummy indicating residential school attendance, modeled as depending on instruments Z as well as health determinants X, and unobservables u_{it}^R. We estimate these models by FIML assuming the error terms u are jointly normally distributed,

$$\begin{pmatrix} u^1 \\ u^0 \\ u^R \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_1^2 & \sigma_{12} & \sigma_{13} \\ \sigma_{12} & \sigma_0^2 & \sigma_{23} \\ \sigma_{23} & \sigma_{23} & 1 \end{pmatrix} \right). \tag{5}$$

From the model, we calculate the estimated average treatment effect (ATE) conditional on the realization of the covariates X_{it},

$$\widehat{ATE}(X_{it}) = (\hat{\gamma}_1 - \hat{\gamma}_0) + \sum_i (X_{it} - \mu_X)[\hat{\beta}_1 - \hat{\beta}_0], \tag{6}$$

and characterize how the effect of residential schooling varies with observed characteristics by calculating $(\hat{\beta}_1 - \hat{\beta}_0)$. The unconditional ATE is given by the estimated value of $(\gamma_1 - \gamma_0)$. For

[18] See for example Heckman et al. (2006) for an extended discussion of this class of model.

example, if it were the case that the only covariate was age and we estimated,

$$\begin{aligned} ATE(X_{it}) &= (\gamma_1 - \gamma_0) + (\text{age}_{it} - \mu_{\text{age}})[\beta_1 - \beta_0] \\ &= 2.3 - 0.03[\text{age}_{it} - \mu_{\text{age}}], \end{aligned} \tag{7}$$

where in this case β_1 and β_0 are scalars and μ_{age} is the sample mean age, then we would conclude that the average respondent's treatment effect is ATE = 2.3 units, and each year of age reduces that effect by 0.03 units. We calculate standard errors for these estimates analytically using the Broyden–Fletcher–Goldfarb–Shanno (BFGS) approximation to the asymptotic covariance matrix. All models were estimated using Stata 13.0.

4.4 Identification from school openings, closings, and location

We use an identification strategy similar to Feir (2012) when estimating the specifications (3) and (4), exploiting the top-down institutional nature of the residential schooling system: the dates schools were opened or closed and the distance from the community to the school were not under control of the affected communities but rather dictated by governments and churches. The econometric specifications above impose enough structure that we are able to recover estimates of average treatment effects even without an exclusion restriction. However, since these estimates are based on a natural experiment in the form of school openings, closings, and distances, our estimates should still be thought of as measuring local causal effects for children near the margin of attendance, specifically, children whose attendance outcome is most elastic to presence of the closest residential school within 30 km.[19] Note that we control for distance to the closest major city as well as latitude in our specifications in order to control for the availability of alternative schooling options that are not captured by our census division and cohort fixed effects. This strategy parallels research in labour economics studying college attendance, such as Card (1995), but the top–down placement of schools mitigates concerns over selection across distance to schools, and in addition we are able to exploit variation in dates of school closing, variation which is not typically available in the literature on college attendance.

It is important to acknowledge that the top–down nature of decision making regarding residential schools began to change after 1970 (Miller, 1996; Milloy, 1999). During this period,

[19] One implication is that if the impact of residential school is heterogeneous in the distance of the school from the community, we will only be able to estimate the effects of the closest schools.

Indigenous communities gained more authority and control over the schooling of their children. Thus while school closing and child attendance at residential schools was still ultimately controlled by the federal government, school closure is less obviously conditionally exogenous than in earlier time periods. This implies our results for periods after the 1970s should be viewed with caution. If school closure is correlated with changes in the health of children due to unobservable factors changing within communities, then our estimates of the effects of residential schools will be biased.

4.4.1 Data on residential school openings, closing, and locations

We use several data sources to construct the indicator of whether the closest residential school was open when an individual was of schooling age (see Feir (2012) for a more detailed description). We use information from the Aboriginal Healing Foundation on the dates of closure, opening, and location of residential schools[20] and combine it with data on the coordinates of CSDs provided by the Environmental Systems Institute and several provincial data sets from the Canadian Atlas Map Bundle on Canadian cities and towns. This allows schools to be matched with communities. Residential schools are matched to cities/towns and then ARC GIS is used to locate the closest residential school to a given community.[21] Using these files, the latitude and distance from the closest city are also calculated. Then, the closest residential school to each CSD is chosen using "as the crow flies" distance from the center of the CSD.[22] This distance is used as the main distance measure. By construction, all communities are tied to some residential school. The provincial schooling ages and their changes over time after 1945 is taken from Riddell and Song (2011) who expand upon the initial data collected by Oreopoulos (2006) on compulsory schooling laws and their changes in Canada. Before 1945, they are taken from the Indian Act. The provincial laws are used after 1945 because of the implementation of the Family Allowance at this time. Receiving the Family Allowance required obeying provincial schooling legislation and thus they are the more practically relevant schooling ages. However, if the Indian Act school age requirements were more stringent than the provincial requirements, the Indian

[20] These dates and locations can be found at http://wherearethechildren.ca/en/about/ahf.html (last retrieved: September 29, 2012). If the school was transferred to the band or group of bands before the school was ultimately closed, the date of transfer is given instead of the date of closure.

[21] The only schools included in the match are those that existed in 1928 or later since it is the meaningful time frame for our sample.

[22] Census sub-divisions in the context of the Indigenous population include Indian reserves, Indian settlements, and unorganized territories (Statistics Canada 2003).

Act ages are the ones we use. This gives us the appropriate age range over which children could be compelled to attend residential school.

Our identification strategy requires mapping individuals back to the communities they belonged to when they were young. The mapping of individuals to communities in the 1991 APS follows Feir (2012). She uses specific band membership information in the 1991 APS in conjunction with Statistics Canada and Aboriginal Affairs and Northern Development Canada records that legally link 2006 band names with census subdivisions which she converts back to 1991 names and geography. See Feir (2012) for more detail.

The matching process for the 2001 APS was unfortunately less complete due to the lack of information regarding band membership. To overcome this limitation we used the fact we know where an individual has resided one year ago and five years ago. If someone currently lives off-reserve, but they lived on-reserve either one year ago or five years ago, we match them back to the reserve in which they previously resided in and deem this their origin community. If they lived on two different reserves, they are matched to the one they resided in 5 years ago. If an individual did not reside on-reserve in any of these years, we assume that they lived on the closest reserve to their current geographic location.

Figure 3 shows predicted values from a probit model of attendance, controlling for cohort, sex, and region. The figure shows that the further away the closest residential school is, the less likely a child is to attend. The figure also shows that there is a substantial increase in the probability of attendance if the nearest school is open. As an instrument, we use an indicator equal to one if the nearest is school is within 30 kilometers and is open and zero otherwise. While this indicator does not fully leverage the distance from school measure, the results are robust to using variation in school distance interacted with whether the school was open as an instrument. We use the binary instrument for ease of interpretation.

Table 2 displays estimates of OLS models, along with marginal effects from the equivalent probit specifications deployed in subsequent modeling. Across specifications (1) and (2) we vary the province versus census division fixed effects. The estimated effects of the closest school being open, and the gradient between probability of attendance and geographic distance, are remarkably stable across the OLS and probit models and in specifications (1) and (2). A school closure decreases probability of attendance by 5 percentage points. These results are robust to the inclusion of a variety of individual–level covariates suggesting that school openings, closings, and

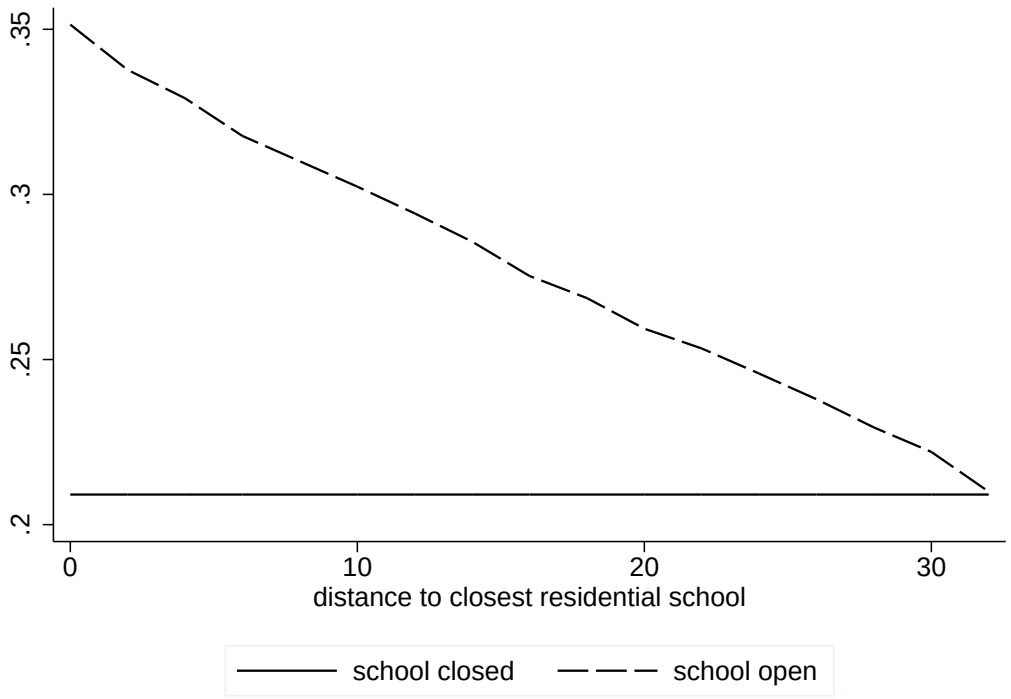

Figure 3: *The figure illustrates predicted probabilities of attending residential school when the nearest school to the respondent's community is open or closed against the geographic distance to that school, measured in kilometers. The probit regression controls for sex, province effects, and a full set of birth year effects; these covariates were evaluated at their sample means when constructing the figure.*

distances are not highly correlated with personal characteristics after conditioning on birth year and region, which in turn increases confidence that the instruments were effectively randomly assigned.

We assess the strength of the instruments using the F-statistics from linear versions of the first–stage models for residential school attendance, as the literature does not present formal diagnostic checks for instrument strength in the class of nonlinear models we estimate. The first–stage F–statistics range from 26.02 in the most restricted specification to 33.9 when all covariates are included. These values are higher than those suggested as flagging weak instruments for linear models by Stock et al. (2002).

5 Results

We first demonstrate that Indigenous body weight and height have both increased over time. To the best of our knowledge, both of these results are new to the literature, as previous work on BMI has limited attention to documenting obesity prevalence in single cross-sections in the

Canadian Indigenous community (for example, Katzmarzyk (2008)). Increases in age-adjusted BMI across cohorts in the Indigenous community parallel increases in mean BMI in the Canadian general population (Katzmarzyk, 2002a), but increases in height over our sampling window imply much larger increases in the biological standard of living in the Indigenous population over this window than in the general population.

Figure 4 depicts the height and body weight of each cohort by birth year adjusted by OLS for a quadratic in age, sex, and region. Height is reported in inches. The data in the figure indicate substantial plasticity in these outcomes at the population level between cohorts. The figure demonstrates that significant changes and height occurred in the Indigenous population between 1930 and 1980, which lends credibility to the hypothesis that residential schooling may have causally affected height and, possibly, some of the increases we observe in the Indigenous population may be due changes in residential schooling policy. In the late 1940s residential schools began to close and continued to do so over the 50s and 60s. Whether the decline of the residential schooling system or the changes within it contributed to the increased height of the population is a question we address in this paper. However, the results in this figure also make a separate contribution to the literature. We believe that we are the first to document that Status Indian height increased substantially over this sampling window possibly implying large increases in the biological standard of living. These observed increases are larger than increases in height among the general population in North America over this time period (Batty et al., 2009).

Having demonstrated the plasticity of both body weight and height over time within the Indigenous community, we now turn to broader determinants of height. First consider OLS models of the form (1). OLS estimates of height are reported in Table 3 and of BMI are reported in Table 4. A bare-bones specification, (1), only includes a dummy indicating survey wave, the residential schooling dummy, birth year and province fixed effects, and "pre–treatment" covariates. In (2), we add measures of income and education (3), Indigenous identity measures (4), specification with all these covariates, and (5) specifications with all these covariates and census division fixed effects instead of province fixed effects.

Across specifications, attendance at residential school is associated with lower height. Attendants were about 0.11 inches shorter than non–attendants (t=2.26). When conditioning on adult income, labor market, and identity outcomes, the effect varies modestly between about -0.12 and

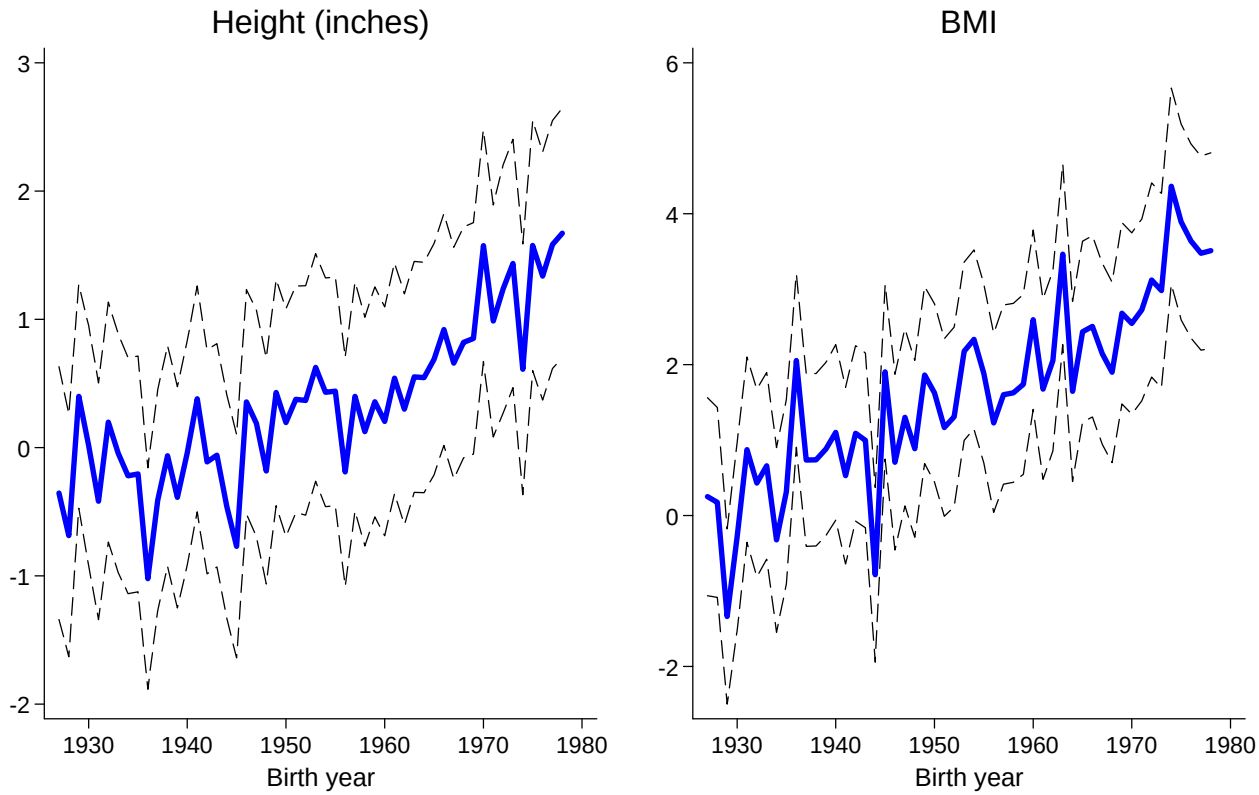

Figure 4: Variation in age, province, and sex adjusted height and BMI by birth cohort. *Figure shows estimated birthyear effects from OLS regressions also included a quadratic in age, a sex dummy, and province effects. 1991 and 2001 Aboriginal Peoples Surveys. Dashed lines display a 95% confidence interval.*

-0.17. These results are consistent with either a negative causal effect of residential schooling or with negative selection into residential schooling. Estimates on other covariates suggest greater height is associated with higher latitudes, male respondents, higher income, higher education, some non-Indigenous lineage, and not speaking an Indigenous language in the home.

In Table 4 we show that residential school attendance is also associated with lower BMI. In the most parsimonious specification, (1), BMI is about 0.22 units lower for attendants which is relatively stable across specifications (3), (4), and (5). Again, these estimates are consistent with either a negative causal effect of residential schooling on BMI or of selection of the lowest weight children into residential school. Higher BMI is also associated with higher latitudes, greater distance to major cities, only Indigenous lineage, speaking an Indigenous mother tongue (but negatively with speaking an Indigenous language in the home as an adult). The education gradient for weight is fairly flat, except for higher BMI at very low education levels. Unexpectedly and unlike in the general population, higher BMI is associated with higher incomes, with each $10,000 increase in income associated with about 0.13 units higher BMI (t=2.9 in specification

5).

Now consider estimates that correct for selection into residential schooling. Table 5 displays estimates of height models of the form in equation (3), instrumenting for residential schooling using the school open dummy and distance to the nearest school. After correction for selection bias, we find that residential schooling caused increases in adult height. When correcting only for exogenous pre-treatment covariates in specification (1), the estimates suggest that exogenous assignment to a residential school increased height by 1.12 inches (t=2.8). The estimated effect does not appear to be substantially mediated by income, educational, or Indigenous identity measures included in specifications (2), (3), and (4), yielding estimates ranging from 0.82 to 0.1.19 inches. However, in specification 5, where we control for census division fixed effects, the estimated effect falls to 0.43 inches and becomes statistically insignificant. Despite the estimate in (5) being noisy, the magnitude is still meaningful. An increase in height of about one inch is substantial but similar to increases in stature observed in other developed countries in general populations experiencing rapid increases in the biological standard of living. It is comparable for example to about half the increase in average height (among men) in the U.S. general population between 1900 and 1950 (Steckel, 1995), suggesting these estimates are plausibly related to the discrepancies in standard of living in residential schools and the alternate environment. Estimates on covariates other than residential schooling are similar to OLS estimates, lending plausibility to the nonlinear model estimates.

Estimates of body weight using econometric model (3) are displayed in Table 6. Unlike in the case of height, both the OLS and selection–corrected estimates indicate that residential schooling decreases mean body weight. The instrumental variables estimates are notably larger in magnitude than the OLS estimates. After correcting for selection, exogenous assignment to residential schooling decreased mean BMI by 0.80 units when conditioning only on pre-treatment covariates (specification 1, p=0.07). Conditioning on income and educational outcomes in specification (2) has basically no effect on the residential schooling coefficient. Conditioning on adult identity measures in specification (3), or all covariates in specification (4) also has limited effect. The effect in specification (5) is also statistically significant although slightly smaller in magnitude. These results suggest that the effects of residential schooling operate through a direct effect on childhood development rather than affecting adult body weight indirectly through changes in education, income, or Indigenous identity.

We test for the robustness of our results to using the primary source of variation in Feir (2012) and Feir (2016): the non-Indigenous proportion of Catholics in 1941 surrounding an Indigenous community multiplied by the national trends in enrollment in residential schools. We do not use this instrument as the primary one here because it is not as powerful of a predictor of residential school attendance in the 2001 APS as would be desirable. We report these results in Table A1. These estimates are similar in magnitude to those in Table 5 and 6 hence the findings are robust to using this instrument.

5.1 Falsification test

Whether or not school opening, closing, and distance is exogenous to the factors that influence adult height conditional on pre-treatment characteristics, age, cohort, and location controls is extremely important for whether we can make causal assertions regarding the effects of residential schooling. Table 7 provides a form of falsification test by estimating the "intent to treat" for two Indigenous populations in Canada that would have lived in similar geographic regions and would face many similar social conditions but one of which could be compelled to attend residential schools and one of whom could not be compelled to attend residential schools. As discussed above, individuals covered by the *Indian Act* could be compelled to attend residential schools. On the other hand, another Indigenous populations in Canada, such as the Métis, could not be compelled to attend.[23] Although Métis children could, in theory attend residential schools, it was only under very limited circumstances. The explicit policy outlining admission of Métis students stated that they were not to be admitted unless registered North American Indian children did not meet the schools authorized admission requirements and even if a Métis student was admitted, their schooling and living costs would not be covered by the federal government (RCAP 1996). As a consequence, in practice very few Métis would attend residential schools. This suggests that if we find any effect of the instruments on the Métis living around the same geographic regions as the Status Indian population in the sample, it would be evidence against the exclusion restriction.

The top of Table 7 reports the results of estimating a reduced form regressions with the dependent variable as height and the independent variables the controls used in previous models

[23]The Métis are the descendants of the Indian women and European fur traders who developed distinct communities with their own customs separate from the broader European community and are not eligible to be Registered as Status Indians under the *Indian Act*.

and the instruments. The instruments are statistically significant and have the expected signs. This is encouraging because these estimates of the intent to treat also account for the possibility of spill-over effects and we find little evidence for this.[24]

The lower section of Table 7 reports the estimated coefficients on the instrumental variables for the same specifications. Once controls for age, birth year, province and other pre-treatment variables are added, the instruments are not statistically significant, small, and of the opposite sign as for the Status Indian population. These results do not reject the possibility of our exclusion restrictions being valid.

5.2 Heterogeneity in the effect of residential school attendance

Imposing the condition that the effect of residential schooling does not vary across people is restrictive. In Table 8 and Figure 5 we report estimates of heterogeneous treatment effect models which relax that assumption. Estimates of the effect of covariates on the causal effect of residential schooling on height and on BMI, that is, estimates of elements $(\beta_1 - \beta_0)$ and of the overall ATE $(\gamma_1 - \gamma_0)$ from equation (4), are displayed in Panel A of Table 8. Figure 5 illustrates the estimated birth-year effects.

The unconditional ATE of residential schooling on height is slightly over three inches (t=9.767), substantially higher than the analogous estimate from the more restrictive models discussed above. This height increase is approximately the same as that observed in last quarter of the 19th century to the last quarter of the 20th century in the United Kingdom (Fogel, 1994). It is also equivalent to an increase in per capita income of about 10,000 dollars American in 1985 (Fogel, 1994). However, the average effect obscures substantial variation across respondents. Female respondents experienced lower causal effects of residential schooling than male respondents, by about a third of an inch (t=3.6). People who attended residential schools in Ontario experienced the largest causal effect on height, with respondents in Manitoba, Saskatchewan, Alberta, and B.C. experiencing height increases between 0.5 and 1.5 inches lower in magnitude than in Ontario. This result suggests that the relative difference between the biological standard of living across residential schools and the Indigenous communities was largest in Ontario and smallest in British Columbia, that is, either that (for the subpopulation at the margin of selection

[24]Dividing the school open indicator with the proportion treated yields an estimated effect of residential school of slightly less than 1 inch. This difference is within the bounds of statistical error.

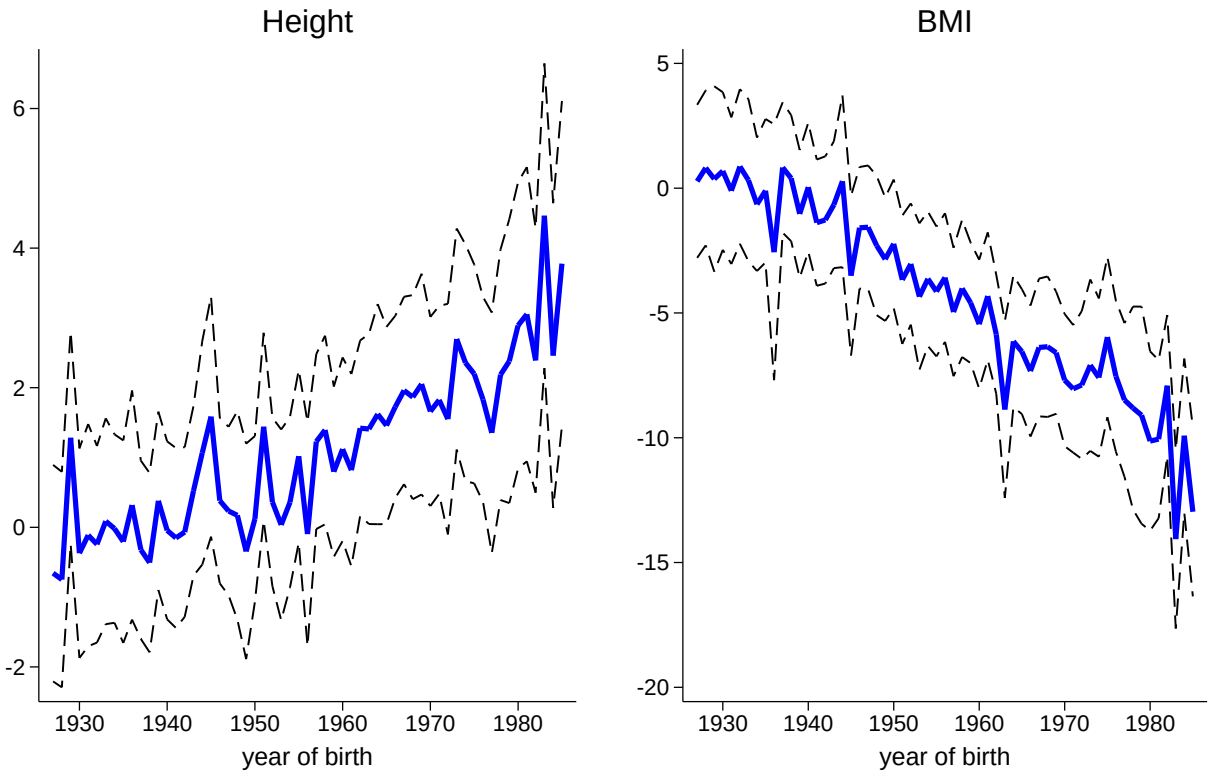

Figure 5: Variation by cohort in the causal effect of residential schooling . *Figure shows estimated birth year effects from heterogeneous causal effects models of height and BMI (the birth year component of $(\beta_1 - \beta_0)$ from specification (4)). Positive values indicate the causal effect of residential schooling is higher (or less negative) in the indicated year than the omitted base cohort (1928). Other covariates include a sex dummy, province effects, and pre-treatment covariates as summarized in Table . Dashed lines display a 95% confidence interval.*

into residential schools) residential schools in Ontario had the highest standard of living, or that the children selected in Ontario had the lowest standard of living, or some combination of these explanations. If one assumes that residential schools, in particular after 50s regulation changes, were somewhat similar across the country, this finding is consistent with longer occupation and colonization having worse outcomes on communities standards of living in Ontario relative to BC (e.g. earlier contact, under treaty, more active, and large settler populations earlier on) making the counterfactual between on reserve and in residential schools potentially greater.

Table 8 shows that the estimated ATE of residential schools on BMI is very large in magnitude, at -11.05 units (t=38.39), more than an order of magnitude larger than the estimates from constant effects models. For a person who is five feet, six inches tall, this would correspond to a decrease of about 50 pounds. We interpret this large estimate as possibly reflecting an unusual draw from the distribution of the instrumental variables estimator—although our

model is nonlinear, in the linear case the estimator would have a mean but no variance (with one overidentifying restriction), that is, it would have wide tails. We tentatively conclude that the causal effect of residential schooling is likely to be negative and may be substantially larger in magnitude than either OLS or conventional instrumental variables.

Figure 5 illustrates the estimated birth year effects for height and BMI. Cohorts born earlier than circa 1960 experienced roughly the same effect of residential schooling on height, and thereafter the effect of residential schooling rises rapidly, to about two more inches as of roughly 1980. This result suggests that relative living standards in residential schools improved markedly in the roughly the mid-1960s, consistent with policy reforms intended to improve nutrition and health in residential schools implemented during this period.

We anticipate that health–affecting conditions in residential schools which lead to greater height will also lead to lower BMI. To confirm the estimates are consistent with the hypothesis that our results primarily reflect the direct effects of residential schooling on development during childhood, as opposed to effects on acculturation or other later–life mediating influences, we further explore the heterogeneous treatment effects models by asking whether the covariates which cause larger positive causal effects on height also lead to more negative causal effects on BMI. Figure 6 displays a scatter of the estimated coefficients from the height and BMI models reported in Table 8. With the exception of the Indigenous mother tongue dummy, which is imprecisely estimated in the height equation, all effects are of opposite signs in the two equations, consistent with our hypothesis. Further, the magnitudes tend to mirror each other as well; the correlation between the estimated coefficients in the two models is -0.80. We re-estimate the models including census division fixed effects and display the correlation between the estimated fixed effects in the height and BMI models in Figure 7. Once again, the estimates are strongly negatively correlated. The fact that these estimates line up as expected if residential schooling affected childhood health conditions is reassuring that our estimates may be picking up the causal effect of residential school.

The results of this exercise are robust to controlling for a full set of census division fixed effects. However, with census division fixed effects, the heterogeneous effects identified for individual level covariates are no longer relevant. While this is anticipated for factors such as latitude and distance of the community with Census division (which is highly correlated with

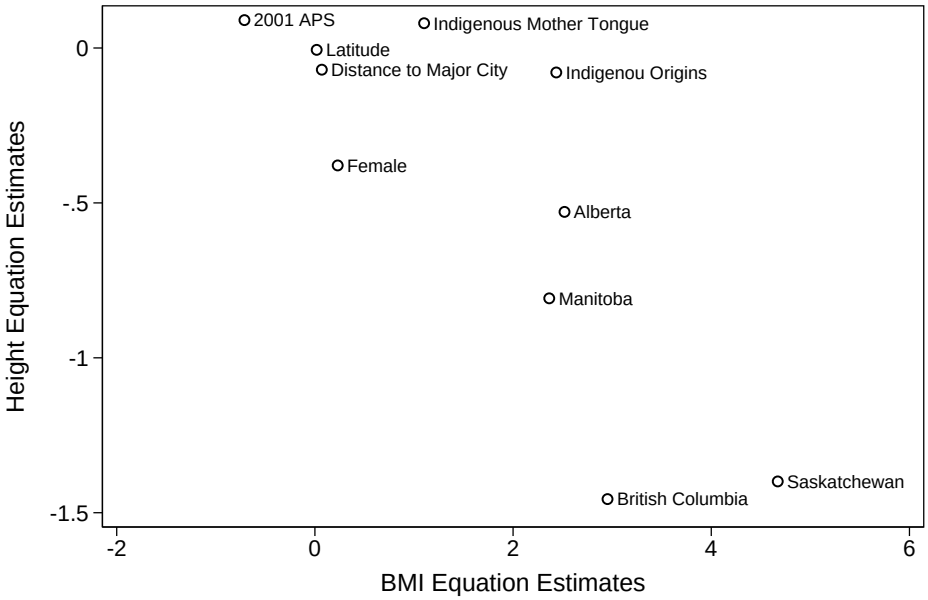

Figure 6: Scatter of treatment effect coefficient estimates in BMI and height models. *Figure shows estimated coefficients from the heterogeneous treatment effects models reported in Table 8.*

census division)[25], this suggests the heterogeneity at the community level in speaking an Indigenous language is approximating for some community-wide characteristic rather than important individual variation within communities. However, the pattern in the treatment effect for height and BMI by cohort are still systematically larger in later cohorts. Alternative specifications that use the non-Indigenous proportion of Catholics augmented by the national level enrollment trend are significantly smaller in magnitude with the ATE for height and not statistically significant, and the cohort trends far less pronounced; however similar patterns are found for body weight. One way to interpret the difference in the effects is the fact that all of the variation from the instrument that uses the proportion of non-Indigenous Catholic surrounding the community will only pick up variation from Catholic schools and will have no power to identify effects for other religious groups.

5.3 Selection versus scarring

The fact that we find residential schooling is robustly associated with increased adult height and body weight once we account for selection could be seen as surprising given the large body of

[25] They are not perfectly correlated because these distances and latitudes are computed at the census subdivision level which is slightly finer than census division.

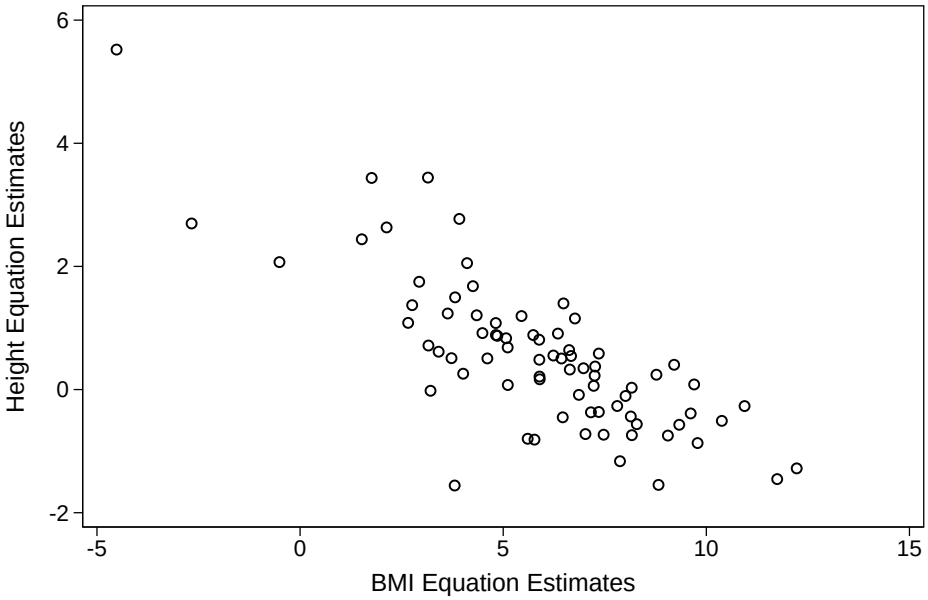

Figure 7: Scatter of census division treatment effect coefficient estimates in BMI and height models. *Figure shows estimated census division fixed effects from the heterogeneous treatment effects models.*

literature describing the poor quality of care children received at the schools.[26]. A serious concern is whether what we are picking up is not increased health, but rather increases in mortality rates. Bozzoli et al. (2007) demonstrated that in countries with very high mortality rates, child mortality is actually positively associated with adult height; in some regions of disease-mortality-height space, selection into the sample (i.e., being alive as an adult) will dominate the effects of poor health conditions reducing adult height. However, at relatively low mortality levels, the effect of disease on height will still dominate any selection. In their model, and in a sample of developing and developed countries, they find that the "selection" effect of disease dominates "scarring" effects at infant mortality rates above 150 per 1,000.

The possibility of residential schooling increasing mortality to the point that selection dominates scarring is arguably unlikely to significantly undercut our results given the era we study and the time period for which we find affects. While the available data on death in residential schools collected by the Truth and Reconciliation shows very high mortality rates for the Indigenous children who attended, mortality rates outside of residential schools were also very high (Lux, 2001). In 1906 a former medical employee attested that the Indigenous population had mortality rates twice that of the Canadian average and as much as three times as high in some provinces (TRC, 2015, 97). Between the 1920s and 1940s, the mortality rates in residential

[26]See for example Milloy (1999), Lux (2001), Stoops (2006), and Mosby (2013).

schools appeared to be approximately the same magnitude as the average Canadian school-aged child (TRC, 2015, 95).[27]

However, by the 1950s, mortality rates in residential school fell dramatically. This change coincides with changes in conditions within residential schools (like the availability of a vaccine for tuberculous), increased health regulations within the schools, and increased levels of student selection. By the start of the 1950s, the mortality rates in residential schools were approximately that of non-Indigenous Canadian children (TRC, 2015, 95). At this point, deaths per 1,000 students fluctuated around 1 per 1000. Increased mortality due to residential schools in this period seems unlikely to be large enough to explain our findings. This is especially unlikely since mortality rates outside of residential schools were very unlikely to be less than 1 per 1000; the mortality rates of Indigenous children today are still significantly higher than their non-Indigenous counterparts (Akee and Feir, 2016; Tjepkema et al., 2009).

Taken together, while we can not rule out that residential schooling may have had significant negative health consequences in some eras, we think that, in the era in which we find a positive effect on health, mortality related censoring of the health distribution is unlikely to be driving the observed increases in height.

6 Conclusions

Our results suggest that residential schooling caused increased adult height and decreased adult body weight of the average child who attended. We demonstrate that selection on unobservables into residential schooling undermines means comparisons and other single-equation methods implicit or explicit in much of the literature; such estimates confound the selection of the most vulnerable children into residential schools with the causal effect of residential schooling.

We shed light on whether the effect of residential schooling was direct or indirect by investigating whether the effects on height and body weight change once we condition on educational, labor market, and identity measures. This lends support to the idea that residential schooling directly affected the health conditions faced for the marginal attendant, possibly through improving access to medical care, nutrition, or generally reducing negative health shocks relative

[27] An exception here was between 1926 and 1930 where the death rate in residential school spikes to about 4 times the general rate of school aged children. However, data is not available on the mortality rates of Status Indian children more generally in this time period.

to alternative environments. While our results on adult height are specification dependent, we find that residential school attendance is associated with an increase in adult height of at least half an inch and robustly associated with a reduction of adult BMI of at least 0.8 units.

To acknowledge the diversity of potential environments in residential schools and changes over time, we relax the assumption of homogeneous treatment effects on height and body weight. Among other results, we find that residential schooling increased the height of those that were born in the 1960s more than those born earlier. Stricter health regulation, increases in funding, and the ban of manual labor in these schools in the 1950s by the federal government, and the more selective student population that attended residential schools, may partly explain this finding. The establishment of Indigenous advisory councils in the late 1960s for residential school may have also substantially contributed to improved conditions in residential schools relative to their alternative environments. Another possibility is that pre-1960, many of the negative health shocks children suffered were from communicable diseases, and the presence of residential schools may have had general equilibrium effects through contagion. This would bias any effect of residential schools towards zero. Post-1960 many of the negative shocks children suffered may have been from factors such as malnutrition, inadequate housing conditions, and access to medical care, and the general equilibrium effects of residential school would then be relatively small. We do rule out the possibility of large negative health effects of residential schools before the 1950s and leave this to future research.

Our findings also contribute to the literature on health more generally by demonstrating that interventions in later childhood can have important effects on adult health. Much of the literature has focused on the role of conditions or interventions during the intrauterine period or during infancy and has demonstrated that these conditions have important effects into adulthood. Our work suggests that children faced with adverse conditions during this early period may still be able to have significant health gains later in life, at least in terms of anthropomorphic measures of health. This implies that later childhood interventions are still relevant for policy despite the recent focus on early childhood health in the literature.

In interpreting all our results, it is important to emphasize that during the time period we investigate, Indigenous communities had faced dramatic disruption to their traditional economies, faced massive losses of resources, and lived in legislated communities determined by the government who provided substandard access to clean water and adequate housing. Any health

gains we attribute to residential schooling need to be interpreted in the broader context of the long term consequences of colonization and as a result, poor living conditions that generate the counterfactual. Although we only include Indigenous peoples in our analysis, together with what is commonly known about standards of care in residential schools relative to that received by the average non-Indigenous Canadian, we see our results as an indictment of the broader colonial conditions faced by Status Indian people during the 20th century.

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Table 1: Selection on observables into residential schooling.

	(1)	(2)	(3)	(4)	(5)
2001 APS	−0.0215 ∗ ∗	−0.0100	−0.0322∗∗∗	−0.0192∗	−0.0155∗
	(−2.8501)	(−1.2402)	(−3.9867)	(−2.2840)	(−1.9628)
latitude	−0.0013	0.0001	−0.0000	0.0012	0.0192∗∗∗
	(−0.5783)	(0.0432)	(−0.0028)	(0.5271)	(3.6530)
distance to major city	0.0067	0.0100∗	0.0109∗	0.0133 ∗ ∗	−0.0251∗∗∗
	(1.3231)	(2.0361)	(2.1103)	(2.6492)	(−4.4674)
only ab. origins	0.1154∗∗∗	0.1198∗∗∗	0.0990∗∗∗	0.1029∗∗∗	0.0779∗∗∗
	(8.5576)	(9.0509)	(6.9311)	(7.3929)	(6.6824)
female	0.0063	−0.0015	0.0092	0.0021	0.0035
	(1.0124)	(−0.2621)	(1.5444)	(0.3835)	(0.7302)
ab. mother tongue	0.0570∗∗∗	0.0597∗∗∗	0.0703∗∗∗	0.0700∗∗∗	0.0697∗∗∗
	(8.3819)	(8.8827)	(9.7052)	(9.9529)	(10.5500)
income		−0.0111∗∗∗		−0.0103∗∗∗	−0.0081∗∗∗
		(−3.7234)		(−3.5144)	(−3.3945)
grade 1 to 4		−0.0567 ∗ ∗		−0.0548∗∗∗	−0.0536∗∗∗
		(−3.2741)		(−3.3259)	(−3.4439)
grade 5 to 8		−0.0235		−0.0252	−0.0217
		(−1.3852)		(−1.5740)	(−1.4835)
grade 9 to 10		−0.0593∗∗∗		−0.0583∗∗∗	−0.0600∗∗∗
		(−3.3078)		(−3.3840)	(−3.7637)
grade 11 to 13		−0.1448∗∗∗		−0.1398∗∗∗	−0.1466∗∗∗
		(−6.6810)		(−6.6462)	(−7.3185)
high school		0.0083		0.0025	−0.0020
		(0.4501)		(0.1407)	(−0.1280)
trades		0.0038		0.0046	0.0045
		(0.3209)		(0.4022)	(0.5041)
university		0.0026		0.0033	−0.0095
		(0.1358)		(0.1788)	(−0.5582)
traditional healer			0.0336∗∗∗	0.0318∗∗∗	0.0296∗∗∗
			(4.2360)	(4.1350)	(4.2515)
on reserve			0.0559∗∗∗	0.0534∗∗∗	0.0467∗∗∗
			(4.9714)	(4.8993)	(5.3838)
speak ab. at home			−0.0775∗∗∗	−0.0670∗∗∗	−0.0681∗∗∗
			(−9.0460)	(−7.9579)	(−8.5054)
Province FE.	X	X	X	X	
Census Division FE					X

Notes. *Marginal effects from probit estimates of residential schooling attendance, 1991 and 2001 Aboriginal Peoples Surveys. Clustered t–ratios at the census subdivision, birth cohort, and survey level are in parentheses. N=37,787.*

Table 2: School openings, closings, and distance as determinants of attendance.

	(1)	(2)
Estimates from OLS models: School Presence.		
School open within 30km	0.0559*** (5.8199)	0.0498*** (5.1014)
F–stat from OLS models	33.8708	26.0247
Birth-year effects	X	X
Province effects	X	
Census Division effects		X
Pre-treatment covariates	X	X

Notes. *Table reports estimates on the excluded instruments, a dummy indicating the closest residential school with in 30 km to the respondent is open when the respondent is age seven and less than 16 from OLS regressions. N=34,696. All models also include constants and dummy indicating survey wave when the samples are pooled. All estimates are statistically significant at* $p< 0.001$. *t-ratios clustered at the community, birth year and survey wave level are in parentheses.*

Table 3: OLS estimates of adult height.

	(1)	(2)	(3)	(4)	(5)
residential school	−0.1125∗	−0.1365 ∗ ∗	−0.1254∗	−0.1454 ∗ ∗	−0.1685∗∗∗
	(−2.2635)	(−2.7909)	(−2.5224)	(−2.9820)	(−3.7285)
2001 APS	0.1594 ∗ ∗	0.0022	0.0976	−0.0330	−0.0538∗
	(2.8227)	(0.0335)	(1.5835)	(−0.4801)	(−0.9363)
latitude	0.0305	0.0354	0.0308	0.0358	0.1406∗∗∗
	(1.5378)	(1.9036)	(1.5429)	(1.9090)	(2.7049)
distance to major city	−0.0095	0.0044	0.0093	0.0156	0.0160
	(−0.2511)	(0.1207)	(0.2427)	(0.4313)	(0.3768)
only ab. ancestry	−0.6175 ∗ ∗	−0.4877 ∗ ∗	−0.5760 ∗ ∗	−0.4781 ∗ ∗	−0.4990∗∗∗
	(−3.2767)	(−2.7882)	(−3.0318)	(−2.7209)	(−3.4528)
female	−5.0556∗∗∗	−5.0496∗∗∗	−5.0675∗∗∗	−5.0555∗∗∗	−5.0550∗∗∗
	(−81.9375)	(−84.5631)	(−83.1474)	(−86.3955)	(−99.2893)
ab. mother tongue	−0.3523∗∗∗	−0.2754∗∗∗	−0.2567∗∗∗	−0.2205∗∗∗	−0.1219∗
	(−5.2170)	(−4.3205)	(−3.7435)	(−3.2958)	(−2.2680)
income		0.0919∗∗∗		0.0883∗∗∗	0.1089∗∗∗
		(3.6013)		(3.5590)	(5.3404)
grade 1 to 4		−0.6383 ∗ ∗		−0.6198 ∗ ∗	−0.5605 ∗ ∗
		(−3.2582)		(−3.1828)	(−3.1539)
grade 5 to 8		−0.6018 ∗ ∗		−0.5982 ∗ ∗	−0.5547 ∗ ∗
		(−3.2480)		(−3.2506)	(−3.2634)
grade 9 to 10		−0.3568		−0.3456	−0.29428
		(−1.8369)		(−1.7905)	(−1.6717)
grade 11 to 13		−0.1628		−0.1451	−0.1248
		(−0.8157)		(−0.7310)	(−0.6816))
high school grad		−0.2462		−0.2509	−0.1088
		(−1.1585)		(−1.1825)	(−0.6286)
trades		0.2608∗		0.2617∗	0.1417∗
		(2.1705)		(2.1667)	(2.0401)
university		0.6354∗		0.6307∗	0.5026∗
		(2.3810)		(2.3644)	(2.0607)
traditional healer			0.1619∗	0.1100	0.0974
			(2.1827)	(1.5283)	(1.5898)
on reserve			−0.0654	−0.0109	−0.0888
			(−0.7444)	(−0.1321)	(−1.4186)
speak ab. at home			−0.2825∗∗∗	−0.1856 ∗ ∗	−0.2128∗∗∗
			(−4.5482)	(−3.1326)	(−3.7288)
birth year f.e.	X	X	X	X	X
province f.e.	X	X	X	X	
census division f.e.					X

Notes. *OLS estimates of adult height in inches, 1991 and 2001 Aboriginal Peoples Surveys. N=37,787. All models also include fixed effects for birth year and for province. t-ratios clustered at the community, birth year and survey wave level are in parentheses.*

Table 4: OLS estimates of adult BMI.

	(1)	(2)	(3)	(4)	(5)
residential school	−0.2277∗	−0.2001∗	−0.2619 ∗ ∗	−0.2345∗	−0.1684
	(−2.3618)	(−2.1099)	(−2.7782)	(−2.5358)	(−1.8261)
2001 APS	1.9028∗∗∗	1.9112∗∗∗	1.8817∗∗∗	1.8840∗∗∗	1.9715∗∗∗
	(17.4418)	(16.8834)	(16.2163)	(16.1187)	(19.8294)
latitude	−0.0914∗	−0.1000 ∗ ∗	−0.0854∗	−0.0941 ∗ ∗	−0.2824∗∗∗
	(−2.5115)	(−2.9302)	(−2.2967)	(−2.7072)	(−4.0051)
distance to major city	0.1183∗	0.1161∗	0.1250∗	0.1231∗	0.1793 ∗ ∗
	(2.0795)	(2.1325)	(2.1452)	(2.2233)	(2.7860)
only ab. ancestry	0.3742	0.3717	0.2154	0.1979	0.1173
	(0.8120)	(0.9081)	(0.4817)	(0.4976)	(0.3685)
female	−0.2191∗	−0.1670	−0.1958	−0.1345	−0.1227
	(−1.9823)	(−1.7198)	(−1.8276)	(−1.4552)	(−1.4963)
ab. mother tongue	0.2130∗	0.1985∗	0.1938	0.1797	0.2039∗
	(2.0832)	(2.0595)	(1.8996)	(1.8221)	(2.3044)
income		0.1215 ∗ ∗		0.1349 ∗ ∗	0.1158 ∗ ∗
		(2.7008)		(2.9842)	(2.9664)
grade 1 to 4		0.7898∗		0.7806∗	0.7999∗
		(2.4779)		(2.4643)	(2.4634)
grade 5 to 8		0.2349		0.2146	0.2278
		(0.7531)		(0.6937)	(0.6767)
grade 9 to 10		0.2464		0.2289	0.2748
		(0.7837)		(0.7367)	(0.8473)
grade 11 to 13		0.1972		0.1939	0.2473
		(0.6083)		(0.6038)	(0.7399)
high school grad		0.2825		0.2474	0.1585
		(0.8470)		(0.7435)	(0.4897)
trades		−0.1188		−0.1139	0.0441
		(−0.7423)		(−0.7000)	(0.3747)
university		0.0357		0.0477	0.0527
		(0.0366)		(0.0488)	(0.0850)
traditional healer			0.2215	0.2208	0.3025∗
			(1.4759)	(1.4018)	(2.2459)
on reserve			0.3953 ∗ ∗	0.4350 ∗ ∗	0.3942∗∗∗
			(2.7733)	(3.2812)	(3.3386)
speak ab. at home			−0.1809	−0.1983∗	−0.0725
			(−1.8230)	(−2.0481)	(−0.7409)
birth year f.e.	X	X	X	X	X
province f.e.	X	X	X	X	
census division f.e.					X

Notes. *OLS estimates of adult body mass index (BMI), 1991 and 2001 Aboriginal Peoples Surveys. N=37,787. All models also include fixed effects for birth year and for province. t-ratios clustered at the community, birth year and survey wave level are in parentheses.*

Table 5: Instrumental variables estimates of adult height.

	(1)	(2)	(3)	(4)	(5)
residential school	1.1194 ∗ ∗	0.8222	1.1887 ∗ ∗	0.9068	0.4314
	(2.824)	(1.6193)	(3.0476)	(1.8763)	(1.151)
2001 APS	0.1874 ∗ ∗	0.0131	0.1435∗	−0.0098	−0.0419
	(3.2535)	(0.2019)	(2.281)	(−0.1460)	(−0.7356)
latitude	0.0332	0.0361	0.0325	0.0359	0.1293∗
	(1.6327)	(1.9114)	(1.5907)	(1.8832)	(2.4553)
distance to major city	−0.0148	−0.0021	−0.0023	0.0047	0.0321
	(−0.3804)	(−0.0551)	(−0.0562)	(0.1198)	(0.7696)
only ab. ancestry	−0.7260∗∗∗	−0.5778 ∗ ∗	−0.6684∗∗∗	−0.5583 ∗ ∗	−0.5321∗∗∗
	(−3.5997)	(−2.9694)	(−3.3626)	(−2.9662)	(−3.5182)
female	−5.0639∗∗∗	−5.0480∗∗∗	−5.0792∗∗∗	−5.0566∗∗∗	−5.0564∗∗∗
	(−80.4010)	(−83.8441)	(−81.4524)	(−85.6903)	(−98.8743)
ab. mother tongue	−0.4229∗∗∗	−0.3328∗∗∗	−0.3534∗∗∗	−0.2976∗∗∗	−0.1675 ∗ ∗
	(−5.7646)	(−4.6298)	(−4.7757)	(−3.9755)	(−2.9034)
income		0.1030∗∗∗		0.0997∗∗∗	0.1147∗∗∗
		(3.8065)		(3.8237)	(5.6015)
grade 1 to 4		−0.5875 ∗ ∗		−0.5684 ∗ ∗	−0.5313 ∗ ∗
		(−2.7880)		(−2.7277)	(−2.8950)
grade 5 to 8		−0.5788 ∗ ∗		−0.5717 ∗ ∗	−0.5415 ∗ ∗
		(−2.9684)		(−2.9527)	(−3.1076)
grade 9 to 10		−0.304		−0.2891	−0.2623
		(−1.4474)		(−1.3911)	(−1.4300)
grade 11 to 13		−0.0518		−0.0292	−0.0573
		(−0.2304)		(−0.1320)	(−0.2927)
high school grad		−0.2537		−0.2543	−0.1112
		(−1.1740)		(−1.1772)	(−0.6289)
trades		0.2504∗		0.2495∗	0.1358
		(2.1732)		(2.1572)	(1.959)
university		0.6201∗		0.6135∗	0.5029∗
		(2.2601)		(2.2296)	(2.0196)
traditional healer			0.1173	0.0767	0.081
			(1.624)	(1.0794)	(1.3068)
on reserve			−0.1321	−0.061	−0.1151
			(−1.3587)	(−0.6453)	(−1.7025)
speak ab. at home			−0.1726∗	−0.1084	−0.1676 ∗ ∗
			(−2.4165)	(−1.5339)	(−2.6410)
birth year f.e.	X	X	X	X	X
province f.e.	X	X	X	X	
census division f.e.					X

Notes. *Instrumental variables (FIML) estimates estimates of height (measured in inches), 1991 and 2001 Aboriginal Peoples Surveys. N=37,787. All models also include fixed effects for birth year and for province. t-ratios clustered at the community, birth year and survey wave level are in parentheses. Residential schooling dummy treated as endogenous and instrumented with a dummy variable indicating the closest residential school with in 30 km to the respondent is open when the respondent is age seven and less than 16.*

Table 6: Instrumental variables estimates of BMI.

	(1)	(2)	(3)	(4)	(5)
residential school	−0.8657∗	−0.8403∗	−0.9262∗	−0.8875∗	−0.6867∗
	(−2.3041)	(−2.2616)	(−2.4245)	(−2.3472)	(−2.0694)
2001 APS	1.8883∗∗∗	1.9039∗∗∗	1.8585∗∗∗	1.8696∗∗∗	1.9612∗∗∗
	(17.1191)	(16.8077)	(15.695)	(15.8975)	(19.6563)
latitude	−0.0928∗	−0.1005 ∗ ∗	−0.0863∗	−0.0942 ∗ ∗	−0.2726∗∗∗
	(−2.5553)	(−2.9582)	(−2.3338)	(−2.7311)	(−3.9027)
distance to major city	0.121∗	0.1204∗	0.1309∗	0.1299∗	0.1654 ∗ ∗
	(2.1334)	(2.1956)	(2.2253)	(2.3027)	(2.6327)
only ab. ancestry	0.4304	0.4319	0.2621	0.2477	0.146
	(0.9544)	(1.0876)	(0.5977)	(0.6396)	(0.4665)
female	−0.2148	−0.1681	−0.19	−0.1338	−0.1215
	(−1.9456)	(−1.7359)	(−1.7711)	(−1.4489)	(−1.4822)
ab. mother tongue	0.2496∗	0.2368∗	0.2427∗	0.2276∗	0.2433 ∗ ∗
	(2.4982)	(2.4965)	(2.3984)	(2.3051)	(2.6476)
income		0.114∗		0.1278 ∗ ∗	0.1108 ∗ ∗
		(2.5273)		(2.8187)	(2.8837)
grade 1 to 4		0.7559∗		0.7487∗	0.7746∗
		(2.3567)		(2.3541)	(2.3792)
grade 5 to 8		0.2195		0.1982	0.2164
		(0.7003)		(0.6384)	(0.6418)
grade 9 to 10		0.2111		0.1939	0.2472
		(0.6666)		(0.6209)	(0.7573)
grade 11 to 13		0.123		0.122	0.189
		(0.3749)		(0.3765)	(0.5559)
high school grad		0.2875		0.2495	0.1605
		(0.8642)		(0.7523)	(0.4966)
trades		−0.1119		−0.1063	0.0492
		(−0.7152)		(−0.6694)	(0.4206)
university		0.0459		0.0584	0.0524
		(0.0473)		(0.06)	(0.0852)
traditional healer			0.244	0.2415	0.3167∗
			(1.5981)	(1.5227)	(2.3254)
on reserve			0.429 ∗ ∗	0.4661∗∗∗	0.4169∗∗∗
			(3.0696)	(3.5739)	(3.6185)
speak ab. at home			−0.2365∗	−0.2462∗	−0.1116
			(−2.2012)	(−2.3948)	(−1.1229)
birth year f.e.	X	X	X	X	X
province f.e.	X	X	X	X	
census division f.e.					X

Notes. *Instrumental variables (FIML) estimates estimates of height (inches), 1991 and 2001 Aboriginal Peoples Surveys. N=37,787. All models also include fixed effects for birth year and for province. t-ratios clustered at the community, birth year and survey wave level are in parentheses. Residential schooling dummy treated as endogenous and instrumented with a dummy variable indicating the closest residential school with in 30 km to the respondent is open when the respondent is age seven and less than 16.*

Table 7: Estimates of the "Intent to Treat" for Registered Indians and the Métis.

	Sample: Registered North American Indian			
	Height		BMI	
	(1)	(2)	(1)	(2)
School open within 30km	0.1467*	0.1426**	-0.1787*	-0.1611
	(2.5230)	(2.8469)	(-2.2946)	(-1.9097)
	Sample: Métis			
	Height		BMI	
	(1)	(2)	(1)	(2)
School open within 30km	-0.0367	-0.0186	0.0256	-0.0462
	(-0.4454)	(-0.2178)	(0.1757)	(-0.3071)
Birthyear effects	X	X	X	X
Province effects	X		X	
Census division effects		X		X
Pre-Treatment covariates	X	X	X	X

Notes. *Table displays estimates of effect of covariates on height. 1991 and 2001 Aboriginal Peoples Surveys, N=37,787 for Registered Indians and N=5,000 for the Métis (approximately - exact accounts not released because of restrictions regarding confidential data use in these surveys). Only Métis in the same Census Divisions as Registered Indians in the sample are included. The sigma are estimated variances in the error structure (5) and the ρ are error correlations. t–ratios and standard errors are robust.*

Table 8: Heterogeneous treatment effect estimates of height and BMI.

Panel A: Coefficient estimates.

	Height		BMI	
	coefficient	t–ratio	coefficient	t–ratio
Unconditional Average Treatment Effect ($\gamma_1 - \gamma_0$)				
ATE	3.42	9.77	−11.0512	−38.40
Effect of covariates on the causal effect of residential schooling ($\beta_1 - \beta_0$).				
2001 APS	0.090	0.837	−0.711	−2.958
Latitude	−0.006	−0.177	0.018	0.255
Distance to major city	−0.070	−1.217	0.070	0.507
Indigenous origins	−0.079	−0.280	2.436	3.857
Female	−0.379	−3.604	0.230	1.133
Indigenous mother tongue	0.080	0.653	1.102	5.131
Manitoba	−0.808	−2.650	2.365	4.230
Saskatchewan	−1.399	−3.633	4.671	6.115
Alberta	−0.529	−1.547	2.519	3.956
British Columbia	−1.456	−4.280	2.953	5.117

Panel B: Covariance parameter estimates.

	coefficient	std. err.	coefficient	std. err.
σ_0	1.0914	0.0111	1.6294	0.0191
σ_1	1.243	0.04156	2.1021	0.0180
ρ_0	−0.1181	0.0780	0.0031	0.0517
ρ_1	−0.8933	0.0845	1.8298	0.0427

Notes. *Table displays estimates of effect of covariates on the causal effect of residential schooling on height and BMI, that is, estimates of elements of* ($\beta_1 - \beta_0$) *and the average treatment effect* ($\gamma_1 - \gamma_0$) *from model (4). Positive coefficients indicate the causal effect of residential schooling the larger (or less negative). 1991 and 2001 Aboriginal Peoples Surveys, N=37,787. The sigma are estimated variances in the error structure (5) and the* ρ *are error correlations. t–ratios and standard errors are clustered at the cohort and census division level.*

Table A1: Instrumental variables estimates of Height and BMI with Proportion Catholic Times Trend as Instrument.

	Height		BMI	
	(1)	(2)	(1)	(2)
residential school	0.4986	0.4314	−0.7289∗	−0.6867∗
	(1.3676)	(1.151)	(−2.0875)	(−2.0694)
2001 APS	0.1721∗∗∗	−0.0419	1.9634∗∗∗	1.9612∗∗∗
	(3.3669)	(−0.7356)	(20.3037)	(19.6563)
latitude	0.1283∗	0.1293∗	−0.2709∗∗∗	−0.2726∗∗∗
	(2.2231)	(2.4553)	(−3.8547)	(−3.9027)
distance to major city	−0.003	0.0321	0.1832 ∗ ∗	0.1654 ∗ ∗
	(−0.0670)	(0.7696)	(2.851)	(2.6327)
only ab. ancestry	−0.6993∗∗∗	−0.5321∗∗∗	0.2852	0.146
	(−3.9873)	(−3.5182)	(0.8008)	(0.4665)
female	−5.0671∗∗∗	−5.0564∗∗∗	−0.1893∗	−0.1215
	(−93.1172)	(−98.8743)	(−2.0395)	(−1.4822)
ab. mother tongue	−0.3037∗∗∗	−0.1675 ∗ ∗	0.2991∗∗∗	0.2433 ∗ ∗
	(−4.8047)	(−2.9034)	(3.3764)	(2.6476)
income		0.1147∗∗∗		0.1108 ∗ ∗
		(5.6015)		(2.8837)
grade 1 to 4		−0.5313 ∗ ∗		0.7746∗
		(−2.8950)		(2.3792)
grade 5 to 8		−0.5415 ∗ ∗		0.2164
		(−3.1076)		(0.6418)
grade 9 to 10		−0.2623		0.2472
		(−1.4300)		(0.7573)
grade 11 to 13		−0.0573		0.189
		(−0.2927)		(0.5559)
high school grad		−0.1112		0.1605
		(−0.6289)		(0.4966)
trades		0.1358		0.0492
		(1.959)		(0.4206)
university		0.5029∗		0.0524
		(2.0196)		(0.0852)
traditional healer		0.081		0.3167∗
		(1.3068)		(2.3254)
on reserve		−0.1151		0.4169∗∗∗
		(−1.7025)		(3.6185)
speak ab. at home		−0.1676 ∗ ∗		−0.1116
		(−2.6410)		(−1.1229)
birth year f.e.	*X*	*X*	*X*	*X*
census division f.e.	*X*	*X*	*X*	*X*

Notes. *Instrumental variables (FIML) estimates estimates of height (inches) and BMI, 1991 and 2001 Aboriginal Peoples Surveys. N=37,787. All models also include fixed effects for birth year and for province. t-ratios clustered at the community, birth year and survey wave level are in parentheses. Residential schooling dummy treated as endogenous and instrumented with the proportion Catholic times the proportion enrolled at the time the respondent was seven years of age.*