THE CENTER FOR INDIAN COUNTRY DEVELOPMENT

WORKING PAPER SERIES

NO. 1905

Indian Residential Schools, Height, and Body Mass Post-1930

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August 16, 2019

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Abstract

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 Survey and leverage detailed historical data on school closures and location to make causal inferences. We find evidence that, on average, residential schooling increases adult height and the likelihood of a healthy adult body weight for those who attended. 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 First Nations peoples' health, so our results must be understood in the broader social context. Taken in context, our results suggest that health interventions in later childhood can have significant impacts on adult health. We also document significant increases in height and body weight for status people born after the 1960s, suggesting substantial changes in diet and living conditions during this time period.

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

1 Introduction

Until the end of the 20th 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 are known as Indian residential schools. The policies associated with residential schools resulted in the largest class action settlement in Canadian history and the establishment of the Truth and Reconciliation Commission of Canada.[1] Previous research has demonstrated that attendance at Indian residential schools increased children's eventual educational and labor market outcomes on average, but 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.[3] It has been suggested that residential schools explain a significant part in the relatively poor average health outcomes of Indigenous people in Canada today.[4] In this paper, we present quantitative estimates of the causal effect of residential schooling on eventual health outcomes.

To understand the impact of residential school, both the social context outside residential schools and the health environment within residential schools need to be taken into account. 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). The resulting poor conditions outside of residential schools imply that the causal effect of residential schooling is unclear even acknowledging well-documented adverse health conditions within the schools. Further, health conditions within the schools, and the alternative environments facing those who would attend residential school, varied over communities and over time. Notably, health environments within residential schools changed markedly beginning in the late 1950s. 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,

[1] See Reimer et al. (2010) and the Truth and Reconciliation Commission of Canada (2015).

[2] See Feir (2012, 2016), Jones (2013), and Gregg (2018).

[3] See Bryce (1922); Lux (2001); Milloy (1999); Mosby (2013), and Truth and Reconciliation Commission of Canada (2015).

[4] See Gracey and King (2009); Kaspar (2014); King et al. (2009); Reading and Wien (2009); Smith et al. (2005); Truth and Reconciliation Commission of Canada (2015); Waldram et al. (2006), and Akee and Feir (2018).

1996; Milloy, 1999). Deaths from communicable diseases also fell sharply after the introduction of vaccines, changing the disease environment both within residential schools and in Indigenous communities. Which children tended to be selected into residential schools also changed over time. More explicit regulations over which students were to attend residential schools were implemented in the 1960s, changing the characteristics of the students who attended. Those who attended residential schools were increasingly likely to be among the most vulnerable, explicitly including children with chronic conditions who otherwise would not have had access to medical care (Armstrong, 1969, 1-2) and children who may otherwise have been placed into provincial foster care (Milloy, 1999). Thus, the effects of residential schooling on the physical health of the children who attended residential schools may have changed over time. It is important to highlight that, given the data available, we are only able to study the effects of residential schooling on those born after 1930.

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 mass index (BMI) for status First Nations people.[5] Both adult height and BMI may be interpreted as proxies for the biological standard of living experienced during childhood such as nutrition, sanitation, exposure to disease, and workload (Steckel, 2008). Since body weight, but not height, is also affected by adult behaviors and adult material standard of living, we offer evidence on the extent to which residential schooling affects adult BMI due to changes in childhood conditions versus later life conditions. In addition, pooling the confidential data files from the 1991 and 2001 APS allows us to examine trends in status peoples' height and body weight during the mid–20th century. To our knowledge we are the first to map trends in status First Nation height and BMI over this time. We find large increases in adult height for those born in the 1960s and 1970s relative to earlier generations, along with increases in mean BMI exceeding similar trends in the general population. These results suggest large changes in status First Nation living conditions over this period (Bozzoli et al., 2009; Katzmarzyk, 2002a,b).

Estimating the causal effect of residential schools on health is challenging because children were not randomly assigned to attend, implying comparing health distributions for

[5]Status First Nations people or "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.

attendees to those who did not attend confounds treatment and selection effects, even controlling for observed characteristics in early childhood. To overcome this problem we exploit detailed historical data on school openings, closings, and locations as a source of plausibly exogenous variation in residential school attendance to identify a local average treatment effect of residential schooling on adult height and body weight.[6] The use of residential school opening and closing as exogenous variation 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, and we go beyond previous literature on residential schooling and adult BMI in two significant ways.[7] First, by pooling the 1991 and 2001 APS, we are able to disentangle age and cohort effects, which are important in studying outcomes such as height and BMI. Second, we characterize how the causal effect of residential school attendance varied with 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 First Nations 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 inch to an inch and stronger evidence that it decreased mean BMI by about 0.8 units. Further examination suggests that the decrease in adult BMI is entirely due to declines in the probability of being severely obese (a BMI over 35) and that residential school attendance also decreased the probability of being underweight (BMI less than 18). We also show that residential school attendance reduced the probability of diabetes, and that reduced the probability that respondents reported being in good or excellent health. Determining mechanisms

[6] Our identification strategy is most similar to Feir (2012, 2016), but the models that use school opening and closure also akin to those of Jones (2013) and Gregg (2018).

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

through which attendance affected these health outcomes is challenging, but we present suggestive results that these effects are not substantially mediated by education, income, and cultural attachment. Taken together, our results indicate that the impact of residential schools on adult height and BMI results directly from residential schools impacting childhood environmental conditions, rather than indirectly though impacting later-life economic and social conditions.

The positive local average treatment effects on our measures of adult physical health may be somewhat surprising. We explore these results further by relaxing the assumption of a constant treatment effect, and we find notable heterogeneity in the effect of attending residential school. The effect of residential school varies substantially by the era in which children attended. The causal effect of residential schooling on height increased over time, and the effect on BMI became more negative over time. Only respondents born after circa 1960 experienced substantial positive effects of residential schooling on health. This result is consistent with increased funding, tightened health 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 those who attended residential school, as well as their counterfactual environments, explains the increasing effects of residential school over time. For example, during the 1960s and 1970s 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] These results suggest that status First Nations physical 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 importance of childhood health environments later in life. Most of the literature in this area has focused on the role of early childhood health conditions, revealing large effects on adult health, educational, and labor market outcomes.[9] The literature on the effects of later childhood health interventions on

[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).

[9]Notable contributions to this "early origins" hypothesis in economics include Cunha 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. (2011), who find that early health endowments

adult health is smaller; a notable contribution closely related to our context is Akee et al. (2013), who study the effect of substantial cash transfers given to American Indian households with older children on the children's later life BMI. Our findings provide evidence that, at least among the most marginalized children, increasing the resources devoted to childhood health, even in later childhood, can have positive and substantial long run effects.

In the next section we provide a brief history of residential schooling in Canada and present evidence on changes over time in childhood environments and in 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 final section discusses the possible mechanisms though which residential school may have affected adult physical health, and 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, and the last residential school closed its doors in 1996 (TRC, 2015). Residential schools existed in nearly every province in Canada. 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 and 1945 about half of status First Nations students attended a residential school.

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, and in 1911 it set out

are important causes of socioeconomic outcomes in early adulthood. See Currie and Almond (2011) and Almond et al. (2018) for a review of research in economics on the effects of early childhood health outcomes on later life health and socioeconomic outcomes.

a basic set of regulations for the operation of residential schools (Milloy, 1999).

While some residential schools were located within First Nations communities, others were hundreds 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.[10] Note 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.

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 shortfalls. 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 A1 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. The change in government enforcement of attendance at residential schools was largely attributable to the findings of the Reestablishment and Reconstruction Commission, which was designed to survey the state of Canadian affairs after the war (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 secular and Protestant education unfavorably (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 Catholic public schools (Milloy, 1999, 220).

The government forced the churches out of the official operation of the schools in 1969

[10] See McFarlane (1999); Miller (1996) for examples.

and school closure rapidly increased (TRC, 2015, 72). Government enforcement of residential school attendance was never random, but the specialized selection of children to attend residential schools increased during the period of closure. Although the *Indian Act* made school attendance mandatory for all status First Nations children between the ages of seven and 15, 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 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" (Hawthorn, 1967, 50).

In 1969, the director of the Indian Affairs Operations Branch set clear guidelines to schools operators and Indian agents that only First Nations 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).[11] If children were not taken to a residential school, they either attended an integrated provincial public school from home, a white boarding home, or a foster care institution, or they attended a school in their community. In the 1960s and 1970s, 50 percent to 80 percent of the children who attended residential schools were deemed to be neglected in their homes by non-Indigenous child welfare agents (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]

[11] 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 years of age and over.

[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 result of the earlier residential school system: Early residential schools may have resulted in worse outcomes of future parents that may have lead their own children to be more likely to have attended residential school.

2.2 Health and nutrition in residential schools and in counterfactual environments

Residential schools had poor 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 who attended 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). At least up until reforms that began 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, health conditions improved. Indian Health Services dietitians inspected the schools more regularly than had ever been the case and gave detailed menu planning and advice and on-site training to staff based on Canada's food guide (Milloy, 1999, 277). There were also increases in funding in 1957, 1962, and again in 1969 (Milloy, 1999, 273).[13] While there is less evidence on the health conditions in residential schools in the 1970s and 1980s, the funding and regulatory improvements would suggest that health conditions in residential schools in these decades were substantial improvements over the pre-1960s eras.

Since the causal effect of residential schools on the health and development of children who attended depends on the alternate environment these children would have faced, we briefly survey the evidence that exists on living conditions within First Nations communities during the time periods we study. Comprehensive quantitative evidence is not available, but case studies of many communities, and times suggest that conditions in First Nations communities were often dire (Kelm, 1999; Waldram et al., 2006) and worse than in the pre-

[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).

contact and early colonial periods (Carlos and Lewis, 2010; Feir et al., 2017; Steckel and Prince, 2001).

During the late 18th to mid-19th century, First Nations communities were often subject to over-crowding, deficient medical care, poor sanitation, nutritional deprivation, and inadequate clothing (Lux, 2001, 107). Hunger may have been even worse in many 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, 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 (Morley et al., 2010). Further, the vast majority of the calories in these communities "were supplied by white flour, lard, sugar and jam," foods which on their own do not provide adequate nutrition. Medical services to First Nations people were also limited. While the 1920s to the 1970s saw the rise of Indian hospitals, they were often located far from the communities they were meant to serve and it has been argued the hospitals had little positive impact on health and may have even widened health disparities over this time period (Lux, 2016; Young, 1981).

Health data are sparse in the 1950s and 1960s, but data on economic conditions are suggestive of health environments. Lutz (2009) estimate some coastal peoples faced lower real per capita income in 1969 than in 1881 (Lutz, 2009, 49). In 1965, the total income from employment for those living on-reserve was a little over $300 per capita at a time when the Canadian average was $1,400, and a study of 35 nationally representative communities found that the vast majority of households did not have access to indoor toilets and most households did not have access to electricity (Hawthorn, 1967). Discrimination also likely limited economic opportunities: "Even where Indians have the necessary educational or skill qualifications for employment, they face widespread discrimination from potential fellow workers as well as from employers" (Hawthorn, 1967, 55). These economic conditions persisted into the 1970s and 1980s. Akee and Feir (2018) show that during the 1970s and mid-1980s, status First Nations people had mortality rates that were two to almost four times that of non-Indigenous people.

These average patterns mask a great deal of diversity among First Nations communities.

For example, Hawthorn (1967, 81) reported per capita incomes of communities ranging from \$55 to \$2,961 (1961 dollars) and demonstrated substantial diversity in other outcomes. Lutz (2009) argues that communities have fared differently over time after colonization, and Feir et al. (2017) presents evidence that some communities impacted by negative economic shocks during the early 1900s recovered much more quickly than others.

The alternative environment to a residential school may not have been a First Nations community. The 1960s saw the rise of the provincial child welfare system which led to a large number of First Nations children placed into foster care as the residential schools closed (Johnston, 1983). The residential school system was often used to house students removed from outsider-perceived problems in the home, and the provincial child welfare system played a similar role (Blackstock, 2007). If a nearby residential school closed, or didn't have the capacity to house students, children may have been be taken from their communities and placed in foster care (Milloy, 1999). Consistent quantitative work on the conditions for First Nations children in foster care at this time is sparse; the existing non-quantitative work suggests that the child welfare system had serious systemic deficiencies and may have had a long run impact on First Nations health (Davies, 1992; Monture, 1989; Tait et al., 2013).

Summarizing, the environments children would live in if not sent to residential school were deeply impacted by colonization, and commonly had poor health conditions. These environments also varied substantially across space and across time, and the conditions within residential schools themselves changed over time.

2.3 Childhood nutrition and adult body weight and height

Adult height is widely used as a marker of important aspects of standard of living.[14] In addition, Case and Paxson (2008) argue 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 causes diminished adult stature (Li et al., 2003). This literature largely

[14]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).

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 Indigenous 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, 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 overnourished children experiencing a higher probability of obesity as adults (Eckhardt, 2006). Communicable diseases during childhood have similar effects as inadequate nutrition because disease both increase caloric expenditures and can reduce absorption of nutrients (Bozzoli et al., 2009; Silventoinen, 2003).

Evidence presented in both the social science and medical literature, then, suggests that under-nutrition early in life may lead to both diminished adult height 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 decrease the probability of overweight or underweight.

3 The Aboriginal Peoples Survey (1991 and 2001)

We study the effect of residential schooling on stature and BMI using the 1991 and 2001 confidential waves of the Aboriginal Peoples Surveys (APS) Adult Retrieval file. These surveys are unique in that they cover both the on-reserve and off-reserve population and include detailed questions on demographics such as height, 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-reserve and 100 per-

cent of people on-reserve. All those who 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. 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. We also exclude anyone older than the age of 65 because they were not asked the residential school question in the 1991 APS and restrict our focus on communities with sample sizes over 100 people since only the largest Indigenous communities are included in the 2001 APS. See the online appendix Section A for a further discussion of the Aboriginal Peoples Survey and for more detail on how the sample was constructed. A total of 502 communities and 85 residential schools are included in the final sample.

Table 1 contains summary statistics for those who attended residential schools and those who did not. Approximately 20 percent of the sample attended residential school. 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 (the variable "Indigenous mother tongue"). Those with "single ancestry" (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 likely to have fewer years of education than those who did not. We also see that the average height of those who attended is lower and BMI higher. Whether having a higher BMI is associated with poor physical health is not obvious if it is associated with a reduced probability of being underweight or only a slight increase in probability of having a slightly above average body weight. Thus we also include other measures of physical health including diabetes, self reported health, and the probability of being severely obese or underweight. On average, those that attended residential school are more likely to have diabetes, not report excellent or very good health, and are more likely to be severely obese. 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.

3.1 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 opening, closing, and location of residential schools[15] and combine it with data on the coordinates of Census Subdivisions 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 geographic information software (specifically ARC GIS) is used to locate the closest residential school to a given community.[16] Using these files, the latitude and distance from the closest city are also calculated. Then, the closest residential school to each Census Subdivision is chosen using "as the crow flies" distance from the center of the subdivision.[17] This distance is used as the main distance measure. By construction, all communities are tied to some residential school. The ages for which children could be compelled to attended residential school are taken from either the *Indian Act* or provincial schooling laws as found in Riddell and Song (2017).[18] This gives us the appropriate age range over which children could be compelled to attend residential school; however, using a constant age range over time does not impact the results.

Since legislative requirements for attendance at a residential school only applied to status First Nations people, we restrict our sample to this population.[19] Our identification strat-

[15]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.

[16]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.

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

[18]See the online appendix Section A for more details

[19]Status women (and their children) would lose their status under the *Indian Act* if they married a non-status man until 1985. However, after this period they could regain their status (Furi and Wherrett, 2003; Hurley and Simeone, 2014). The benefits to status are non-trivial (possible access to additional educational and medical funds are an example), so there is a significant incentive to claim status is one is eligible. We are not concerned about differential selection into our sample based on status since all women who lost status in this period have likely reclaimed it: the formerly named Indian and Northern Affairs Canada (the home of the Indian Register at the time) estimated that 56,800 people were to granted status based on the changes in 1985, and this estimated number was nearly fully accounted for by 1990 (See Figure A6 in Akee and Feir (2018)).

egy 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 the formerly named 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 than the matching process for the 1991 APS due to the lack of information on band membership in 2001. 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 five 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 A3 shows the distribution of residential schools across the country during the peak of the system in 1930. The triangles represent the locations of residential schools and the dots indicate the centroid of First Nations communities included in the 2001 Census. The shades of the triangles and dots indicate which communities and residential schools are included in our final data. See the online appendix Section A for a greater discussion of this.

4 Econometric methods

4.1 Single–equation descriptive estimates

We first regress adult height and BMI on residential schooling to gain a sense of the basic patterns in the data. Let y_{it} denote a measure of adult 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 that reflect the respondent's condition at the time of selection into residential schooling—including gender, birth year, a dummy for sur-

vey 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 that may be impacted by residential schooling, 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

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

by ordinary least squares (OLS), where β, π and θ are parameters to be estimated, to document partial associations between residential schooling and adult height. Since the later-life outcomes included in W_{it} may be on the causal path from residential schooling to adult outcomes, we estimate equation (1) both including and excluding W. The changes in the estimates of the θ across these two specifications provide suggestive evidence of the degree to which the correlation between residential schooling and the dependent variable can be "explained" by early or late-life experiences.

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 30 km of the respondent's community was open when

the respondent was of schooling age.[20] Let Z_{it} denote this variable. Residential schooling outcomes are modeled,

$$\begin{aligned} R^*_{it} &= X_{it}\pi_0 + \alpha Z_{it} + \epsilon_{it} \\ R_{it} &= 1[R^*_{it} > 0], \end{aligned} \tag{2}$$

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, and $1[\cdot]$ denotes the indicator function. Note that X_{it} includes a full set of census division fixed effects as well as birth year fixed effects, sweeping out 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 (2) 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.

4.3 Modeling heterogeneous treatment effects

We relax the implicit assumption in the models presented above that the causal effect of residential schooling on adult 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.[21] This model may be represented,

$$\begin{aligned} y^1_{it} &= \gamma_1 + [X_{it} - \mu_X]\beta_1 + u^1_{it} \\ y^0_{it} &= \gamma_0 + [X_{it} - \mu_X]\beta_0 + u^0_{it} \\ R_{it} &= 1[X\pi_0 + Z_{it}\pi_1 + u^R_{it} > 0], \end{aligned} \tag{3}$$

where μ_X is the vector of sample means of X_{it}, the γ are constants, y^1_{it} denotes a continuous health outcome respondent i would experience if she was sent to residential school, and y^0_{it}

[20]"Schooling age" is defined by the compulsory schooling age legislated by the *Indian Act* that changes 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 15 are used.

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

the outcome this respondent would experience if she was not sent to residential school, the u are outcome-specific unobserved causes of health, R_{it} again indicates 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,

$$\begin{pmatrix} u^1 \\ u^0 \\ u^R \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_1^2 & \sigma_{12} & \sigma_{13} \\ \sigma_{12} & \sigma_0^2 & \sigma_{23} \\ \sigma_{23} & \sigma_{23} & 1 \end{pmatrix} \right). \tag{4}$$

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

$$\widehat{ATE(X_{it})} = (\hat{\gamma}_1 - \hat{\gamma}_0) + \sum_i (X_{it} - \mu_X)[\hat{\beta}_1 - \hat{\beta}_0], \tag{5}$$

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

$$\begin{aligned} ATE(X_{it}) &= (\gamma_1 - \gamma_0) + (\text{age}_{it} - \mu_{\text{age}})[\beta_1 - \beta_0] \\ &= 2.3 - 0.03[\text{age}_{it} - \mu_{\text{age}}], \end{aligned} \tag{6}$$

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. When models with binary outcomes are used, we explicitly account for the binary nature of the data using a switching probit model. All models were estimated using Stata 15.

4.4 Identification from school openings, closings, and location

We use an identification strategy similar to Feir (2012) when estimating the specifications (2) and (3), 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.[22] 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; 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, Indigenous communities gained more authority and control over the schooling of their children. Thus while school closing and child attendance at residential schools were 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.

Figure A2 shows predicted values from a probit model of attendance, controlling for cohort, sex, and region. The figure shows that the farther away the closest residential school is,

[22] 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.

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 km 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 and across specifications (1) and (2) we vary the province versus census division fixed effects. The estimated effects of the closest school being open are remarkably stable across the OLS 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 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–statistic is at least 26 across specifications, indicating the instruments are not weak (Stock et al., 2002).

5 Results

Figure 1 depicts the adult height and BMI of each cohort by birth year adjusted by OLS for a quadratic in age, sex, and region. Height is reported in inches and BMI in BMI units. The figure demonstrates that significant changes in height occurred in the First Nations 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 First Nations 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 1950s and 1960s. 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 re-

sults in this figure also make a separate contribution to the literature. We believe that we are the first to document that status First Nations height and BMI increased substantially over this sampling window, implying large changes in the biological standard of living. The observed increased in height is either similar or larger than increases in height among the general population in North America over this time period (Batty et al., 2009; Katzmarzyk, 2002b) whereas changes in BMI appear to be larger. For example, between 1953 and 1998, the height of an average non-Indigenous adult increased by between one and two inches while adult height for weight has increased by roughly five percent (Katzmarzyk, 2002b). We see a similar increase in the First Nations population for adult height, but a much larger change in BMI.[23]

Having demonstrated the plasticity of both height and BMI over time within the First Nations community, we now turn to broader determinants of height. First consider OLS models of the form in equation (1). OLS estimates of height and BMI are reported in Table 3. The first three columns present the results for height and the second three for BMI. 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, Indigenous identity measures, and (3) specifications with all these covariates and census division fixed effects instead of province fixed effects.[24]

Across specifications, attendance at residential school is associated with lower adult 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 between about -0.12 and -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.

The results in Table 3 also show that residential school attendance is associated with lower adult BMI. In the most parsimonious specification, (1), BMI is about 0.22 units lower for attendants which is relatively stable across specifications. Again, these estimates are consistent

[23]Height for weight measurements are not equivalent, but the increase in BMI is about 15 percent.
[24]For tables that show the results of adding each set of covariates, see Table A1 and Table A2.

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 4 displays estimates of height and BMI models of the form in equation (2), instrumenting for residential schooling using the school open dummy within 30 kilometers. After correction for selection bias, we find that residential schooling caused increases in adult height in the first columns. 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 specification (2).[25] However, in specification (3), where we control for census division fixed effects, the estimated effect falls to 0.43 inches and becomes statistically insignificant. Despite the estimate in (3) 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 mass using model (2) are displayed in the next three columns of Table 4. Unlike in the case of height, both the OLS and selection–corrected estimates indicate that residential schooling decreases mean BMI. The instrumental variables estimates are notably larger in magnitude than the OLS estimates. After correcting for selection, exogenous as-

[25]For estimates control for subsets of the covariates, see Table A3.

signment to residential schooling decreased mean BMI by 0.87 units when conditioning only on pre-treatment covariates (specification 1, p=0.07). Conditioning on the adult outcomes in specification (2) does not substantially alter this estimate. The estimated effect in specification (3) is also statistically significant, although slightly smaller in magnitude.[26] These results suggest that the effects of residential schooling operate through a direct effect on childhood development rather than affecting adult BMI indirectly through changes in education, income, or Indigenous identity.

5.1 Falsification test

The validity of the results above hinges on the conditional exogeneity of school openings, closings, and distances. This assumption is not testable, but we shed light on its validity by re-estimating our models using data on people who could not be compelled to attend residential schools, the Métis. The Métis are the descendants of the First Nations 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 First Nations under the *Indian Act*. Very few Métis attended residential schools, on a voluntary basis. The explicit policy outlining admission of Métis students stated that they were not to be admitted unless status First Nations children did not meet the schools authorized admission requirements and even if a Métis student were admitted, their schooling and living costs would not be covered by the federal government (The Royal Commission on Aboriginal Peoples, 1996). If we find that school openings, closings, and distance appear to cause change in health outcomes for the Metis, then school openings, closings, and distances are correlated with some unobserved factor affecting population health and are not valid instruments. A finding of no apparent effect among the Métis increases the credibility of our identification assumptions.

The top panel of Table 5 reports the results of estimating reduced form regressions of our main health outcomes, adult height and BMI, on the controls and the dummy for a school open within 30km for our estimation sample. Consistent with our main results, a close, open residential school statistically significantly predicts greater height and reduced BMI. These results bolster our main specifications in that the intent to treat method accounts for the pos-

[26] For estimates control for subsets of the covariates, see Table A4.

sibility of spill-over effects, and we find little evidence of such spillovers. Dividing the school open indicator with the proportion treated yields an estimated effect of residential school of slightly less than one inch, an estimate similar to our main results.

The lower panel of Table 5 reports the estimated coefficients on the instrumental variables for the same specifications, but for the Métis sample. In these models a substantial effect of the school open dummy would cast doubt on our identification strategy. The table shows that the school open dummy has neither a statistically significant nor substantial effect on health outcomes among the Métis. Our identification strategy is not falsified by this test.

5.2 Heterogeneity in the effect of residential school attendance

Which respondents were most likely to experience beneficial or harmful effects of residential schools? In Table 6 and Figure 2 we report estimates of heterogeneous treatment effect models of the form of equation (3). Estimates of the effect of covariates on the causal effect of residential schooling on height and on BMI, that is, estimates of elements of $(\beta_1 - \beta_0)$ and of the overall ATE $(\gamma_1 - \gamma_0)$ from equation (3), are displayed in Panel A of Table 6. Figure 2 illustrates the estimated birth-year effects.

The unconditional ATE of residential schooling on adult height is slightly over 3 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 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 than in Ontario. These results suggest that the difference between the biological standard of living between residential schools and the Indigenous communities was largest in Ontario and smallest in British Columbia, that is, either that — for the sub-population at the margin of selection into residential schools — residential schools in Ontario had the high-

est standard of living, or that communities in Ontario had the lowest standard of living, or some combination of these explanations.

Table 6 shows that the estimated ATE of residential schools on adult 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 effect corresponds 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. It is also plausible that our preferred specification is too demanding of the data given that the exogenous variation relies on a simple indicator variable. If we also use distance to the nearest open residential school as an additional instrumental variable by including the interaction of distance to nearest school and the school open dummy, and we exclude the survey wave indicator, the estimated effect is a more plausible -3.05 units (t=5.36).[27] However, we do not focus on this specification as the simpler model has a readily interpretable LATE but the more complex model does not. The results are qualitatively similar regardless of which set of instruments is used.

Figure 2 shows how the causal effect of residential schooling varied over time. The effect of schooling on height substantially increased over time, and the effect of schooling on BMI became substantially more negative over time. Put another way, later generations experienced larger positive effects of schooling on health than earlier generations. One explanation for this pattern is the regulatory reforms in the 1950s and 1960s succeeded in improving health conditions in residential schools. Another, complementary, explanation is that negative selection into residential schools increased over time. Again, however, we observe that the estimated effects on BMI are very large in magnitude. Results from the less demanding specification that excludes the survey wave indicator and uses additional variation in the distance to the closest open residential school are presented in Figure A4. The estimated effects on BMI presented in Figure A4 are smaller in magnitude than those in Figure 2, but are again qualitatively similar.

[27] The estimated effects remain similar to those in Table 6 if the survey wave indicator is included, even using the additional instrumental variable. This possibly indicates there is too little variation in these two survey waves to separately identify age and cohort effects in this model.

If greater height and lower BMI both reflect better health conditions in residential schools, then health–affecting conditions in residential schools that lead to greater height will also lead to lower BMI. To test if this is so, we check whether covariates that cause larger positive causal effects on height also lead to more negative causal effects on BMI. Figure 3 displays a scatter of the estimated coefficients from the height and BMI models reported in Table 6. 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 4. Once again, the estimates are strongly negatively correlated.

5.3 Mortality selection versus scarring

If residential schooling caused increased mortality among attendees, then our results above may reflect greater health among respondents who *survived* residential school, and therefore appear in our data, rather than a positive causal effect of residential schooling on the marginal attendant's health. This selection effect is well–known in the literature on height and material standard of living. For example, Bozzoli et al. (2007) demonstrated that in countries with very high mortality rates, child mortality is positively associated with adult height. However, at relatively low mortality levels, the effect of disease on height (the "scarring" effect) will still dominate mortality selectivity. Bozzoli et al. (2007) estimate that in their context the selection effect dominates scarring effects at infant mortality rates above 150 per 1,000.

In the context of residential schooling, our results become difficult to interpret if the mortality rate in residential schools was substantially higher than the mortality rate outside of residential schools. Unfortunately, no systematic data exist on either of these mortality rates for the complete time period of interest. However, we can draw some conclusions from existing studies on mortality in residential schools and First Nations mortality more generally.

The evidence on death in residential schools between 1921 and 1965 collected by the Truth and Reconciliation Commission shows very high mortality rates for the Indigenous children

who attended, but mortality rates outside 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 schools appeared to be approximately the same magnitude as the average Canadian school-aged child (TRC, 2015, 95).[28]

However, by the 1950s, mortality rates in residential school fell dramatically. This change coincides with changes in conditions within residential schools (such as the availability of a vaccine for tuberculosis), increased health regulations within the schools, and increased levels of student selection. By the start of the 1950s, the annual mortality rates in residential schools were similar to those of non-Indigenous Canadian children (TRC, 2015, 95). At this point, deaths per 1,000 students were roughly 1 per 1000. Mortality rates outside of residential schools were very unlikely to be substantially less than 1 per 1000; the mortality rates of First Nations children today are still significantly higher than their non-Indigenous counterparts (Akee and Feir, 2018; Tjepkema et al., 2009).

This evidence then suggests that our results mostly reflect scarring and not selection, particularly for the latter cohorts in our data. However, we cannot rule the possibility of some bias due to mortality selection, particularly before 1950.

5.4 Are reductions in BMI indicative of improvements in physical health?

Reductions in BMI do not reflect increased health for individuals who are already underweight. To discern whether the negative effect of residential schooling should be interpreted as increased or decreased health, we estimate the impact of residential school across the distribution of body weight and consider two other measures of health: whether a respondent has been diagnosed with diabetes, and whether an individual reports "very good" or "excellent" self-assessed health.[29]

We report the results of estimating models of the form of equation (2) in Table 7 for several

[28] An exception here was between 1926 and 1930 where the death rate in residential school spikes to about four times the general rate of school aged children.

[29] The survey question does not distinguish between type I and type II diabetes. The self-report health variable has five categories: in poor health, average health, good health, very good health, and excellent health. We choose the cut-off of "very good health" to construct a binary since most of the respondents report at least good health.

body weight categories, diabetes, and self-assessed health.[30] Across specifications which, as before, vary the set of controls, the results robustly indicate that residential schooling decreases the probability of both severe obesity and underweight and, mechanically, increases the probability of normal weight or overweight. These effects are substantial in magnitude: residential schooling decreases probability of underweight by seven to nine percentage points, and decreases probability of severe obesity by about two percentage points. The overall negative effect of schooling on BMI, then, largely reflects decreases in body weight among overweight people and not further reductions in body weight for underweight people.

The results for diabetes and self-assessed health provide further evidence that residential schooling increased the physical health of those that attended relative to the alternative environments they would have faced. The estimates suggest that residential schooling caused about two percentage points lower prevalence of diabetes for those induced to attend by school openings or closings. Across specifications, the effect on self-assessed health is positive, and substantially so, at about seven to nine percentage points in all specifications except (4). However, the effect is substantively and statistically insignificant in specification (4), which includes all mediators and census division effects, and therefore suggestive that there was no average impact on self-assessed health.

Since the evidence presented in Section 5.2 suggests heterogeneity in the impact of residential schools, we estimate switching probit models for the outcomes discussed above and present the results in Figure 5. The dashed line depicts the distribution of the estimated treatment effect for the full sample, whereas the solid line depicts the distribution of the estimated treatment effect for only those who attended residential schools. In panel A, the estimated treatment effect for the full sample on the probability of being severely obese is symmetrically centered around zero. However, the effect of residential school attendance on those who attended is shifted to the left, suggesting that the effect of residential schools on those who actually attended is very different from for those who did not. A similar pattern obtains in panels B and C. The results in panel C suggest that a substantial number of respondents would have experienced a negative impact of residential school on their self-reported health in adulthood had they actually attended residential school, while for the population that attended, the impact of residential schools has a positive effect on self-reported health. The

[30]The results are not sensitive to using a bivariate probit model.

final panel D, depicts the distribution of the treatment effect for diabetes. In this case, the distributions overlap between the full sample and those who actually attended residential school. Generally, the distribution is skewed to the left suggesting that a larger number of respondents would have had a reduction in the probability of adulthood diabetes by attendance at a residential school.

All these results taken together suggest that residential school attendance on average had a positive impact on the physical health of those who attended relative to the counterfactual environments they would have faced. The estimated model also reveals substantial heterogeneity around that average effect, and predicts that the causal effect would have been smaller, or negative, for those who did not actually attend.

6 Discussion and Conclusion

Our results provide evidence that residential schooling increased adult height and increased the probability of a healthy adult BMI for the average child who attended given the broader social context of Canada at the time. 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 find that residential school attendance caused an increase in adult height of about one-half to one inch and decreased BMI by almost one BMI unit. Further, residential schooling led to lower prevalence of underweight, overweight, and diabetes, and possibly to higher self-assessed health for those who attended. We stress that these findings in no way legitimize the residential schooling system, rather, they imply that the conditions within the colonized environment outside of residential schools were even more adverse for some children than the conditions they faced in residential schools.

Understanding the mechanisms through which residential schooling impacted adult health would require far more data than is currently available on the medical services, diets, and routines in each residential school, and on the alternative environments over time. Such a data collection effort is beyond the scope of the work here. However, our analysis is suggestive at least of some plausible mechanisms through which residential schooling may have

affected adult health. Specifically, we demonstrate that the impact of residential schooling cannot be explained through factors such as income, education, or changes in measurable cultural practices or whether a respondent lives on-reserve. This result implies that the impact of residential school on adult height and BMI for the marginal attendant operated directly though the nutritional and disease environments faced in childhood rather than through affecting behaviours that manifest later in adulthood.

The conclusion that the effect of residential schools operated through relatively improved nutrition and disease environments is also supported by the timing of the impact of residential schools. 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 First Nations 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 that had less connection with First Nations communities at the time (i.e., the provincial child welfare system).[31] However, the evidence we present on the mechanisms through which residential schooling impacted the health of the marginal attendant is largely speculative, and we leave the marshaling of more direct evidence to future work. Future work that examines the impact of the child welfare system on Indigenous children post-1960, and its possible interaction with the residential schooling system would be a potentially valuable contribution.

Our findings contribute to the literature on health more generally by demonstrating that interventions in later childhood can have important consequences for adult physical 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

[31] Another explanation for timing of the estimated effects of residential school 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. Such effects 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 not rule out the possibility of large negative health effects of residential schools before the 1950s and leave this to future research.

during this early period may still be able to have significant health gains later in life[32] with increased access to medical care and nutrition. This result implies that later childhood interventions are still relevant for policy despite the recent focus on *early* childhood health in the literature.

We close by repeating that during the time period we investigate, First Nations communities had experienced dramatic disruption to their traditional economies, faced massive losses of resources, lived in legislated communities determined by the government, were provided with substandard access to clean water and housing, and were disproportionately impacted by the child welfare system. Any physical health gains we attribute to residential schooling need to be interpreted in the broader context of the long–term consequences of colonization and the resulting poor living conditions that generate the counterfactual. Together with what is commonly known about standards of care in residential schools relative to that received by the average non-Indigenous Canadian, our results should be interpreted as an indictment of the broader colonial conditions faced by status First Nations people during the 20th century.

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Tables and Figures

Table (1) SUMMARY STATISTICS OF OUTCOME AND DEPENDENT VARIABLES: SELECTION ON OBSERVABLE FACTORS INTO RESIDENTIAL SCHOOLING.

	Did not Attend	Attended	Difference
height	66.83	66.15	0.68
	(3.78)	(3.94)	
body mass index	26.49	27.54	-1.05
	(5.51)	(5.17)	
diabetes	0.07	0.12	-0.05
	(0.26)	(0.32)	
health is excellent or very good	0.61	0.47	0.14
	(0.49)	(0.5)	
severely obese (BMI>35)	0.06	0.08	-0.02
	(0.25)	(0.26)	
underweight (BMI<18)	0.07	0.03	0.04
	(0.26)	(0.18)	
school open within 30 km	0.36	0.62	-0.26
	(0.48)	(0.49)	
female	0.53	0.55	-0.02
	(0.5)	(0.5)	
age	32.56	41.66	-9.1
	(11.19)	(11.93)	
2001 APS	0.41	0.37	0.04
	(0.49)	(0.48)	
latitude	50.69	51.89	-1.2
	(3.85)	(2.72)	
nearest city ln(dist km)	0.89	1.02	-0.13
	(0.97)	(1.01)	
single ancestry	0.77	0.93	-0.16
	(0.42)	(0.25)	
Indigenous mother tongue	0.21	0.48	-0.27
	(0.4)	(0.5)	
used traditional healer	0.08	0.14	-0.06
	(0.28)	(0.35)	
lives on reserve	0.27	0.47	-0.2
	(0.45)	(0.5)	
speaks language at home	0.07	0.15	-0.08
	(0.25)	(0.36)	
highest grade 1 to 4	0.08	0.14	-0.06
	(0.27)	(0.35)	
highest grade 5 to 8	0.18	0.15	0.03
	(0.38)	(0.36)	
highest grade 9 to 10	0.1	0.09	0.01
	(0.3)	(0.29)	
highest grade 11 to 13	0.07	0.04	0.03
	(0.25)	(0.19)	
high school graduate	0.5	0.52	-0.02
	(0.5)	(0.5)	
trades degree	0.36	0.38	-0.02
	(0.48)	(0.49)	
university degree	0.07	0.04	0.03
	(0.26)	(0.2)	

Notes. *Means and standard deviations (in parentheses) for dependent and independent variables used in the analysis stratified by residential schooling attendance. Data source are the 1991 and 2001 Aboriginal Peoples Survey. N=37,787.*

Table (2) FIRST STAGE ORDINARY LEAST SQUARES REGRESSION: SCHOOL OPENINGS, CLOSINGS, AND DISTANCE AS DETERMINANTS OF ATTENDANCE.

	(1)	(2)
Estimates from OLS models: School Presence.		
school open within 30km	0.0559	0.0498
	(5.8199)	(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=37,787. 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 THE CORRELATES OF ADULT HEIGHT AND BODY MASS INDEX, RESIDENTIAL SCHOOL ATTENDANCE AND OTHER FACTORS.

	Height in inches			**Body Mass Index**		
	(1)	(2)	(3)	(1)	(2)	(3)
residential school	-0.1125	-0.1454	-0.1685	-0.2277	-0.2345	-0.1684
	(-2.2635)	(-2.9820)	(-3.7285)	(-2.3618)	(-2.5358)	(-1.8261)
2001 APS	0.1594	-0.033	-0.0538	1.9028	1.884	1.9715
	(2.8227)	(-0.4801)	(-0.9363)	(17.4418)	(16.1187)	(19.8294)
latitude	0.0305	0.0358	0.1406	-0.0914	-0.0941	-0.2824
	(1.5378)	(1.909)	(2.7049)	(-2.5115)	(-2.7072)	(-4.0051)
nearest city ln(dist km)	-0.0095	0.0156	0.016	0.1183	0.1231	0.1793
	(-0.2511)	(0.4313)	(0.3768)	(2.0795)	(2.2233)	(2.786)
single ancestry	-0.6175	-0.4781	-0.499	0.3742	0.1979	0.1173
	(-3.2767)	(-2.7209)	(-3.4528)	(0.812)	(0.4976)	(0.3685)
female	-5.0556	-5.0555	-5.055	-0.2191	-0.1345	-0.1227
	(-81.9375)	(-86.3955)	(-99.2893)	(-1.9823)	(-1.4552)	(-1.4963)
native mother tongue	-0.3523	-0.2205	-0.1219	0.213	0.1797	0.2039
	(-5.2170)	(-3.2958)	(-2.2680)	(2.0832)	(1.8221)	(2.3044)
income		0.0883	0.1089		0.1349	0.1158
		(3.559)	(5.3404)		(2.9842)	(2.9664)
grade 1 to 4		-0.6198	-0.5605		0.7806	0.7999
		(-3.1828)	(-3.1539)		(2.4643)	(2.4634)
grade 5 to 8		-0.5982	-0.5547		0.2146	0.2278
		(-3.2506)	(-3.2634)		(0.6937)	(0.6767)
grade 9 to 10		-0.3456	-0.2942 8		0.2289	0.2748
		(-1.7905)	(-1.6717)		(0.7367)	(0.8473)
grade 11 to 13		-0.1451	-0.1248		0.1939	0.2473
		(-0.7310)	(-0.6816))		(0.6038)	(0.7399)
high school grad		-0.2509	-0.1088		0.2474	0.1585
		(-1.1825)	(-0.6286)		(0.7435)	(0.4897)
trades		0.2617	0.1417		-0.1139	0.0441
		(2.1667)	(2.0401)		(-0.7000)	(0.3747)
university		0.6307	0.5026		0.0477	0.0527
		(2.3644)	(2.0607)		(0.0488)	(0.085)
used traditional healer		0.11	0.0974		0.2208	0.3025
		(1.5283)	(1.5898)		(1.4018)	(2.2459)
lives on reserve		-0.0109	-0.0888		0.435	0.3942
		(-0.1321)	(-1.4186)		(3.2812)	(3.3386)
speaks language at home		-0.1856	-0.2128		-0.1983	-0.0725
		(-3.1326)	(-3.7288)		(-2.0481)	(-0.7409)
birth year f.e.	*X*	*X*	*X*	*X*	*X*	*X*
province f.e.	*X*	*X*		*X*	*X*	
census division f.e.			*X*			*X*

Notes. *OLS estimates of adult height in inches, 1991 and 2001 Aboriginal Peoples Survey. 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) INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF RESIDENTIAL SCHOOLS ON ADULT HEIGHT AND BODY MASS INDEX.

	Height in inches			Body Mass Index		
	(1)	(2)	(3)	(1)	(2)	(3)
residential school	1.1194	0.9068	0.4314	-0.8657	-0.8875	-0.6867
	(2.824)	(1.8763)	(1.151)	(-2.3041)	(-2.3472)	(-2.0694)
2001 APS	0.1874	-0.0098	-0.0419	1.8883	1.8696	1.9612
	(3.2535)	(-0.1460)	(-0.7356)	(17.1191)	(15.8975)	(19.6563)
latitude	0.0332	0.0359	0.1293	-0.0928	-0.0942	-0.2726
	(1.6327)	(1.8832)	(2.4553)	(-2.5553)	(-2.7311)	(-3.9027)
nearest city ln(dist km)	-0.0148	0.0047	0.0321	0.121	0.1299	0.1654
	(-0.3804)	(0.1198)	(0.7696)	(2.1334)	(2.3027)	(2.6327)
single ancestry	-0.726	-0.5583	-0.5321	0.4304	0.2477	0.146
	(-3.5997)	(-2.9662)	(-3.5182)	(0.9544)	(0.6396)	(0.4665)
female	-5.0639	-5.0566	-5.0564	-0.2148	-0.1338	-0.1215
	(-80.4010)	(-85.6903)	(-98.8743)	(-1.9456)	(-1.4489)	(-1.4822)
native mother tongue	-0.4229	-0.2976	-0.1675	0.2496	0.2276	0.2433
	(-5.7646)	(-3.9755)	(-2.9034)	(2.4982)	(2.3051)	(2.6476)
income		0.0997	0.1147		0.1278	0.1108
		(3.8237)	(5.6015)		(2.8187)	(2.8837)
grade 1 to 4		-0.5684	-0.5313		0.7487	0.7746
		(-2.7277)	(-2.8950)		(2.3541)	(2.3792)
grade 5 to 8		-0.5717	-0.5415		0.1982	0.2164
		(-2.9527)	(-3.1076)		(0.6384)	(0.6418)
grade 9 to 10		-0.2891	-0.2623		0.1939	0.2472
		(-1.3911)	(-1.4300)		(0.6209)	(0.7573)
grade 11 to 13		-0.0292	-0.0573		0.122	0.189
		(-0.1320)	(-0.2927)		(0.3765)	(0.5559)
high school grad		-0.2543	-0.1112		0.2495	0.1605
		(-1.1772)	(-0.6289)		(0.7523)	(0.4966)
trades		0.2495	0.1358		-0.1063	0.0492
		(2.1572)	(1.959)		(-0.6694)	(0.4206)
university		0.6135	0.5029		0.0584	0.0524
		(2.2296)	(2.0196)		(0.06)	(0.0852)
used traditional healer		0.0767	0.081		0.2415	0.3167
		(1.0794)	(1.3068)		(1.5227)	(2.3254)
lives on reserve		-0.061	-0.1151		0.4661	0.4169
		(-0.6453)	(-1.7025)		(3.5739)	(3.6185)
speaks language at home		-0.1084	-0.1676		-0.2462	-0.1116
		(-1.5339)	(-2.6410)		(-2.3948)	(-1.1229)
birth year f.e.	X	X	X	X	X	X
province f.e.	X	X		X	X	
census division f.e.			X			X

Notes. *Instrumental variables (FIML) estimates of height (measured in inches), 1991 and 2001 Aboriginal Peoples Survey. 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 within 30 km to the respondent is open when the respondent is of schooling age.*

Table (5) ESTIMATES OF THE "INTENT TO TREAT" FOR STATUS FIRST NATIONS AND THE MÉTIS.

	Sample: status First Nations			
	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)
birth year 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 Survey, N=37,787 for status First Nations 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 (4) and the ρ are error correlations. t–ratios and standard errors are robust.*

Table (6) 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
nearest city ln(dist km)	−0.070	−1.217	0.070	0.507
single ancestry	−0.079	−0.280	2.436	3.857
female	−0.379	−3.604	0.230	1.133
native 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 (3). Positive coefficients indicate the causal effect of residential schooling the larger (or less negative). 1991 and 2001 Aboriginal Peoples Survey, N=37,787. The sigma are estimated variances in the error structure (4) and the* ρ *are error correlations. t–ratios and standard errors are clustered at the cohort and census division level.*

Table (7) INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF RESIDENTIAL SCHOOLS ACROSS THE BMI DISTRIBUTION, ON DIABETES, AND SELF-PERCEIVED HEALTH.

	(1)	(2)	(3)	(4)
severely obese				
residential school	-0.0200	-0.0215	-0.0210	-0.0244
	(-2.1580)	(-2.2968)	(-2.3025))	(-2.2683)
underweight				
residential school	-0.0942	-0.0924	-0.0918	-0.0731
	(-6.3957)	(-6.2707)	(-6.2945)	(-5.9929)
normal and overweight				
residential school	0.0722	0.0713	0.0632	0.0643
	(3.5859)	(3.5490)	(2.6084)	(3.1712)
diabetes				
residential school	-0.0239	-0.0224	-0.0236	-0.0210
	(-3.2920)	(-3.2814)	(-3.2979)	(-2.6608)
health very good or excellent				
residential school	0.0804	0.0925	0.0756	0.0174
	(1.8647)	(2.3811)	(1.5925)	(0.3802)
pre-treatment controls	X	X	X	X
economic mediators		X		X
cultural mediators			X	X
birth year f.e.	X	X	X	X
province f.e.	X	X	X	
census division f.e.				X

Notes. *Instrumental variables (FIML) estimates of effect of residential school attendance on self-reported diabetes, self-perceived health being very good or excellent, being severely obese (BMI greater than 35), normal or over-weight (between 35 and 18), and being underweight (BMI less than 18), 1991 and 2001 Aboriginal Peoples Survey. N=37,787. A linear probability model is used in the second stage, but similar treatment effects are estimated using a bivariate probit regression. 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 within 30 km to the respondent is open when the respondent is of schooling age. "Pre-treatment controls": 2001 survey wave indicator, latitude of community, distance to nearest major city, single ancestry reported, gender, and whether the respondents mother tongue is an Aboriginal language; "Economic mediators": income, indicators for highest level of education is grades 1 to 4, 5 to 8, 9 to 10, 11 to 13, high school graduate, a trades degree or university degree. "Cultural mediators": whether they have seen a traditional healer in the previous year, whether they live on reserve, and whether they speak an Aboriginal language at home.*

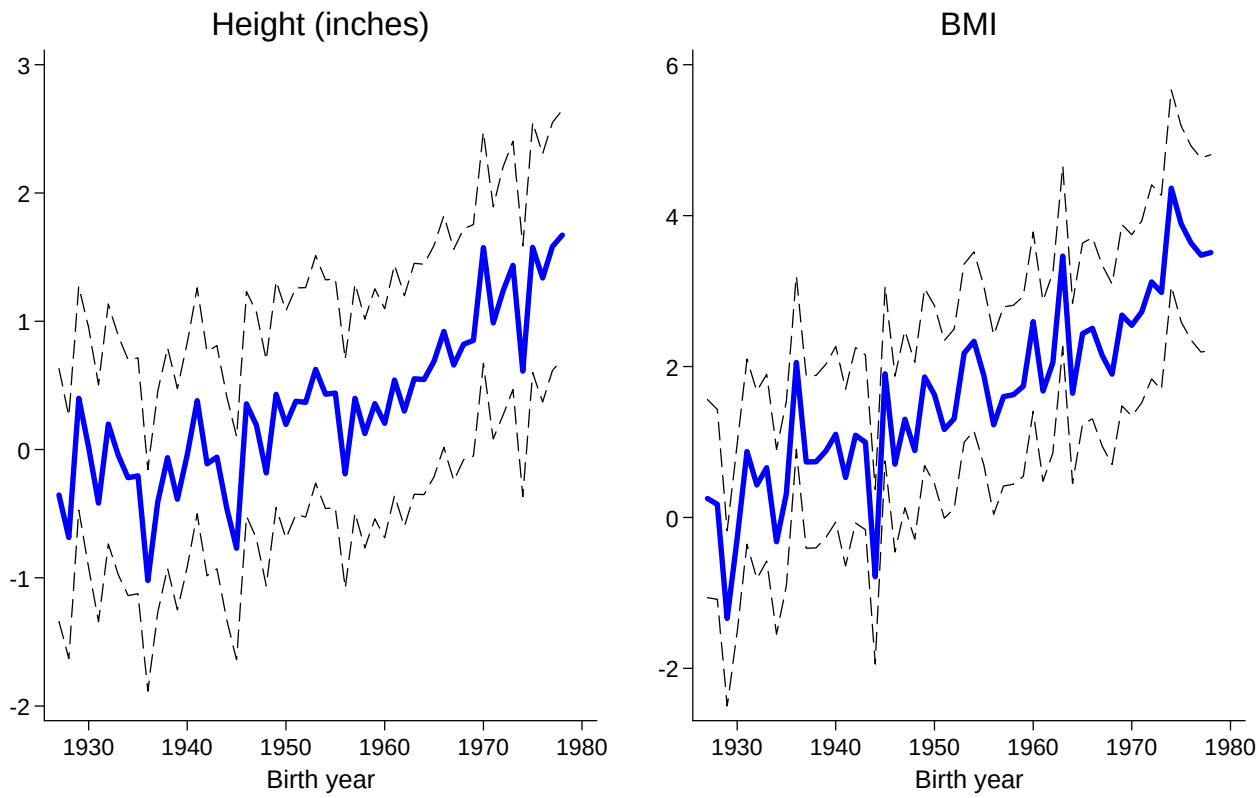


Figure (1) Variation in age, province, and sex adjusted height and BMI by birth cohort. *Figure shows estimated birth year effects from OLS regressions also included a quadratic in age, a sex dummy, and province effects. 1991 and 2001 Aboriginal Peoples Survey. Dashed lines display a 95% confidence interval.*

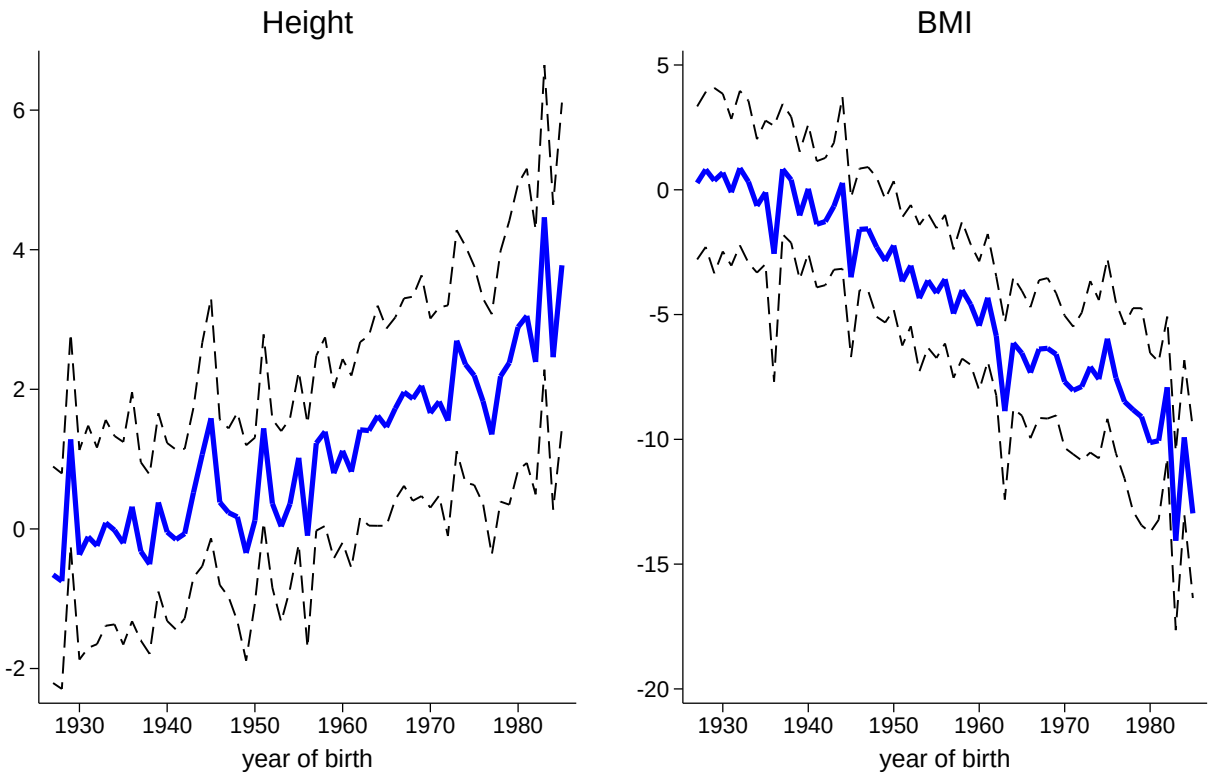


Figure (2) **Variation by cohort in the causal effect of residential schooling.** *Figure shows estimated birth year effects from heterogeneous causal effects models of height and BMI (the birth year component of $(\beta_1 - \beta_0)$ from specification (3)). Positive values indicate the causal effect 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 A4. Dashed lines display a 95% confidence interval.*

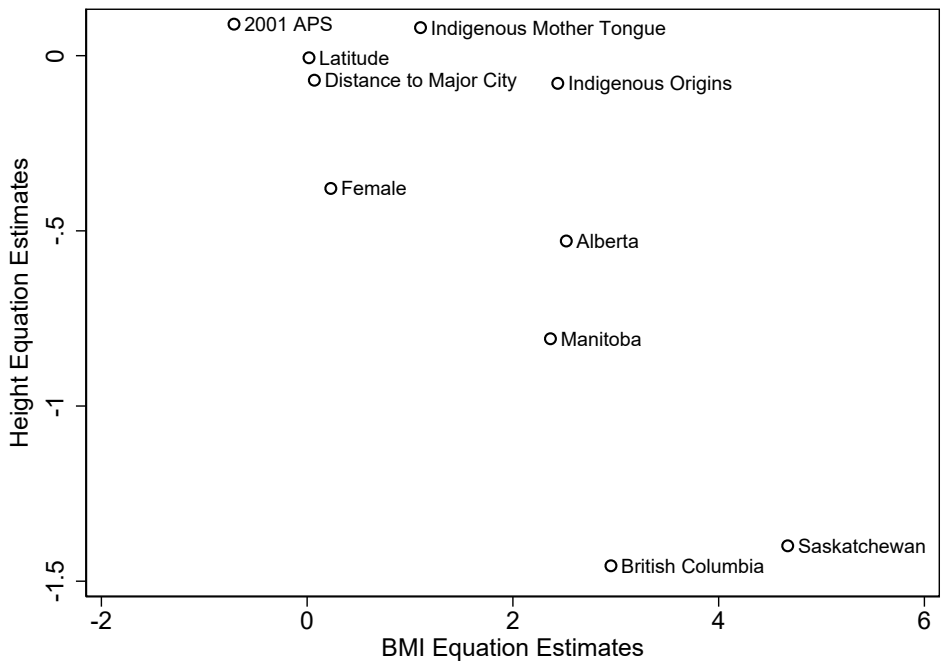


Figure (3) **Scatter of treatment effect coefficient estimates in BMI and height models.** *Figure shows estimated coefficients from the heterogeneous treatment effects models reported in Table 6.*

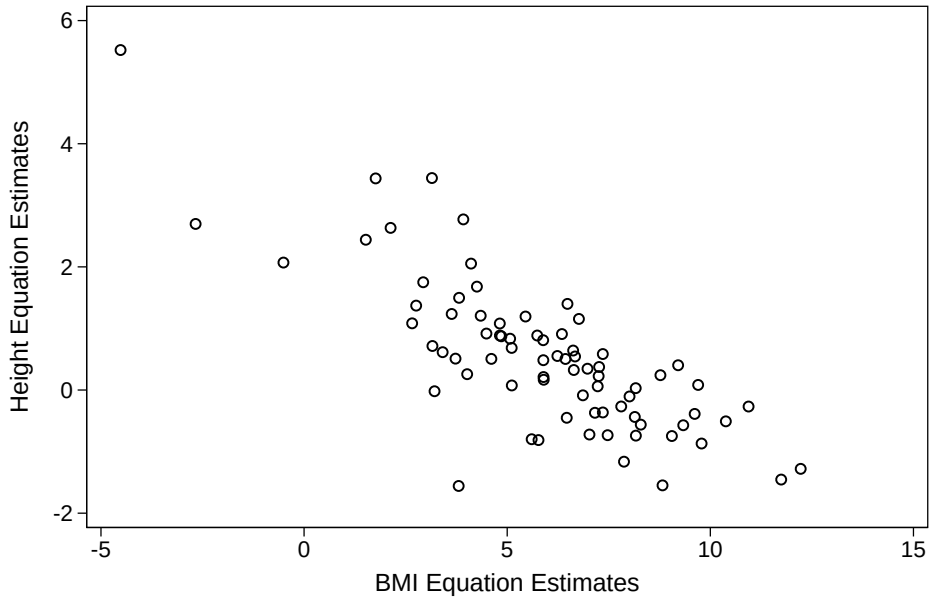
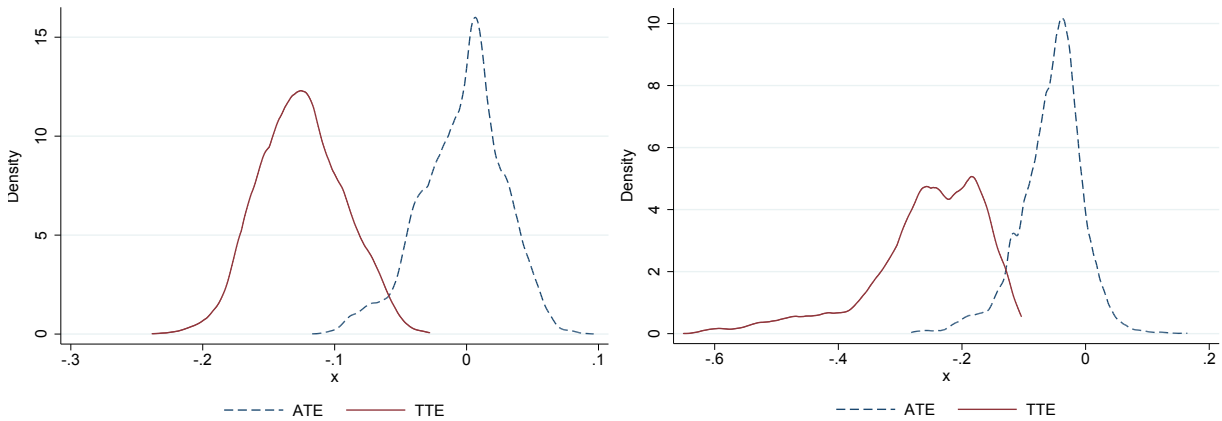
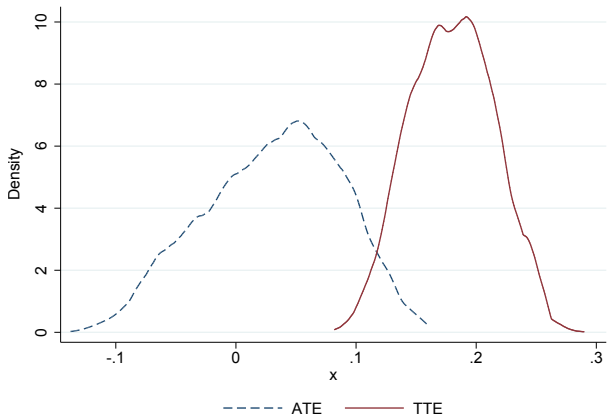


Figure (4) **Scatter of census division treatment effect coefficient estimates in BMI and height models.** *Figure shows estimated census division fixed effects from the heterogeneous treatment effects models.*

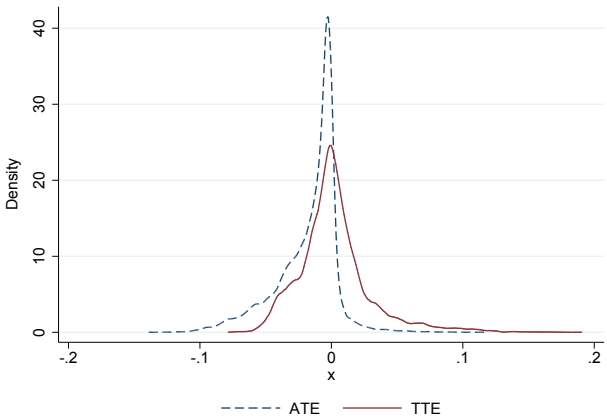


(a) Severely Obese -BMI over 35

(b) Underweight - BMI under 18



(c) Health Very Good or Excellent



(d) Has Diabetes

Figure (5) **Density of estimated treatment effects of residential school attendance on binary health outcome variables.** *ATE indicates the average treatment effect and the TTE indicates the treatment on the treated.*

Online Appendix to “Indian Residential Schools, Height, and Body Mass Post-1930"

A Sample construction and discussion of the 1991 and 2001 Aboriginal Peoples Surveys.

The 2001 APS differs from the 1991 APS 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 Métis), 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 percent 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. The total number of communities included are 502 (about 50 percent of all communities in Canada) and the total number of schools is 85 (about 56 percent of all residential schools).

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.

The age–based difference in the question in the 1991 survey should not be of concern for our findings for two reasons. First, the individuals who are asked separate questions for residential schooling are born between 1929 and 1941 and the results presented in section Y suggest there is no change in effects by year of birth until 1950 or 1960. Second, even if the age-based questions in 1991 do impact the estimated effect of residential schools around 1960 in some unanticipated way, it seems intuitive that the change in the residential schooling question would result in some discontinuous break in the treatment effect by birth cohort rather than resulting in a continuous trend such as the one we observe.

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 last restriction results in a little over 10 percent of the sample being dropped.

Figure A3 shows the communities that remain after our final sample is selected out of the total communities included in the 2001 Census. We can see that there is no obvious selection geographically other than the intentional selection to exclude Quebec, the Atlantic Provinces, and the Inuit. There are 502 communities in the final sample (about 50 percent of all communities in Canada) and 85 schools (about 56 percent of all residential schools).

In constructing the instrument, we need to determine the appropriate ages for which students could be compelled to attend residential school. We take these schooling ages from the *Indian Act* and provincial schooling laws. If the provincial compulsory schooling ages were more stringent than the ages in the *Indian Act*, the provincial compulsory schooling ages are used after 1945. Provincial laws are used only after 1945 because of the implementation of the Family Allowance, which depended on provincial schooling ages. If the *Indian Act* school age requirements were more stringent than the provincial requirements, then the *Indian Act*

ages are the ones we use.

B Tables and Figures

Table (A1) 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)
nearest city ln(dist km)	−0.0095	0.0044	0.0093	0.0156	0.0160
	(−0.2511)	(0.1207)	(0.2427)	(0.4313)	(0.3768)
single 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)
native 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)
used traditional healer			0.1619	0.1100	0.0974
			(2.1827)	(1.5283)	(1.5898)
lives on reserve			−0.0654	−0.0109	−0.0888
			(−0.7444)	(−0.1321)	(−1.4186)
speaks language 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 Survey. 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 (A2) 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)
nearest city ln(dist km)	0.1183	0.1161	0.1250	0.1231	0.1793
	(2.0795)	(2.1325)	(2.1452)	(2.2233)	(2.7860)
single 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)
native 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)
used traditional healer			0.2215	0.2208	0.3025
			(1.4759)	(1.4018)	(2.2459)
lives on reserve			0.3953	0.4350	0.3942
			(2.7733)	(3.2812)	(3.3386)
speaks language 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 Survey. 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 (A3) 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)
nearest city ln(dist km)	−0.0148	−0.0021	−0.0023	0.0047	0.0321
	(−0.3804)	(−0.0551)	(−0.0562)	(0.1198)	(0.7696)
single 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)
native 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)
used traditional healer			0.1173	0.0767	0.081
			(1.624)	(1.0794)	(1.3068)
lives on reserve			−0.1321	−0.061	−0.1151
			(−1.3587)	(−0.6453)	(−1.7025)
speaks language 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 of height (measured in inches), 1991 and 2001 Aboriginal Peoples Survey. 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 within 30 km to the respondent is open when the respondent is of schooling age.*

Table (A4) 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)
nearest city ln(dist km)	0.121	0.1204	0.1309	0.1299	0.1654
	(2.1334)	(2.1956)	(2.2253)	(2.3027)	(2.6327)
single 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)
native 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)
used traditional healer			0.244	0.2415	0.3167
			(1.5981)	(1.5227)	(2.3254)
lives on reserve			0.429	0.4661	0.4169
			(3.0696)	(3.5739)	(3.6185)
speaks language 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 of height (inches), 1991 and 2001 Aboriginal Peoples Survey. 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 within 30 km to the respondent is open when the respondent is of schooling age.*

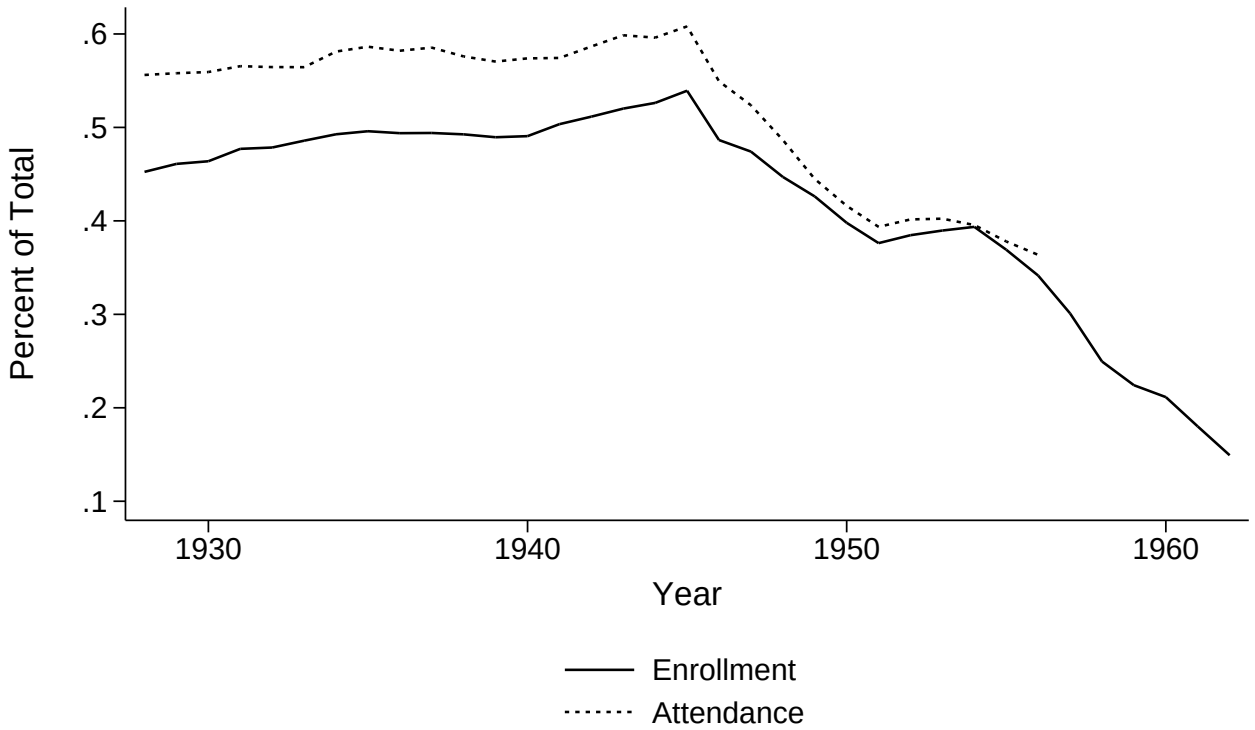


Figure (A1) *These calculations were made using the 1941 to 1980 Indian Affairs Reports. This figure also appears in Feir (2012) and Feir (2016). The solid line indicates the proportion of the total schooling age Status Indian population enrolled in a residential school. The dashed line indicates the proportion of the total number of students enrolled in residential schools that are actually attending.*

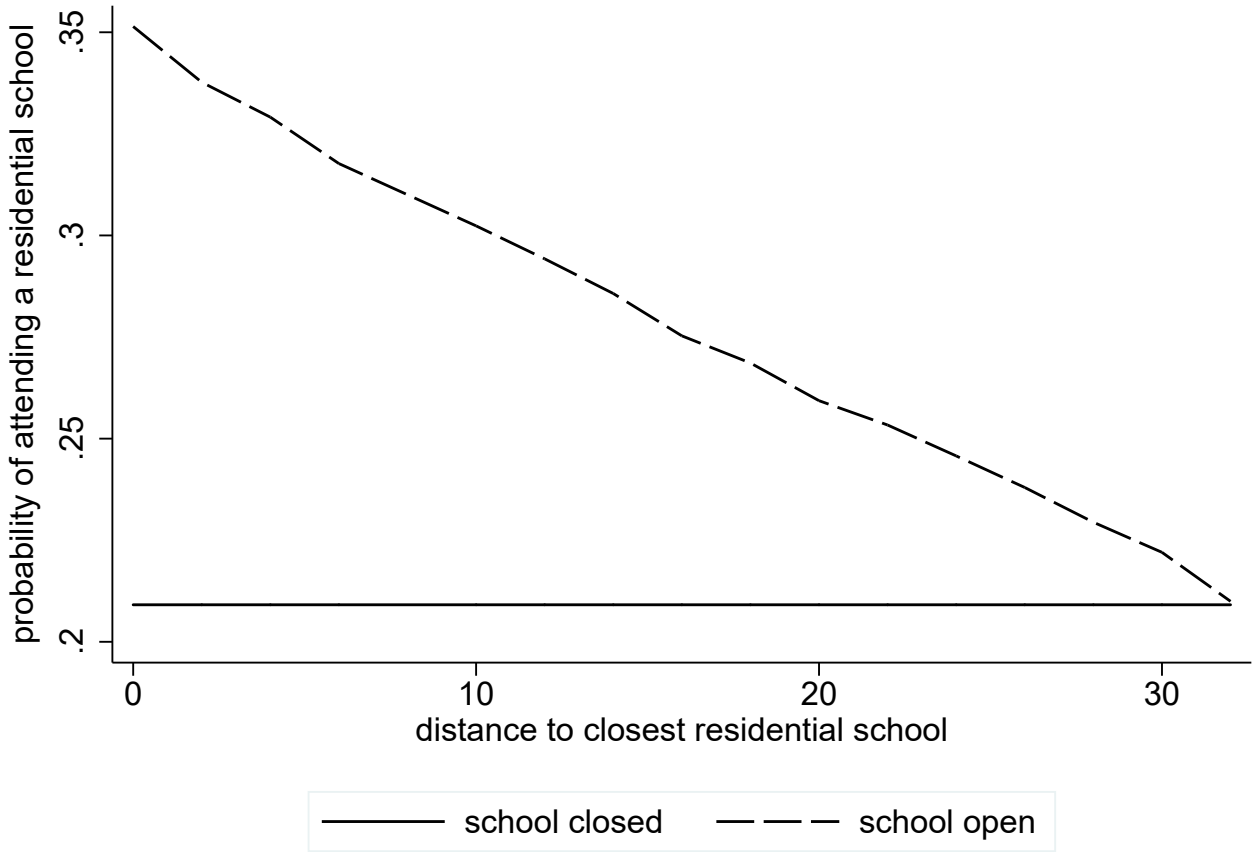


Figure (A2) *The figure illustrates predicted probabilities of attending residential school when the nearest school to the respondent's community is open or closed against the geographic distance to that school, measured in kilometers. The probit regression controls for gender, province effects, and a full set of birth year effects; these covariates were evaluated at their sample means when constructing the figure.*

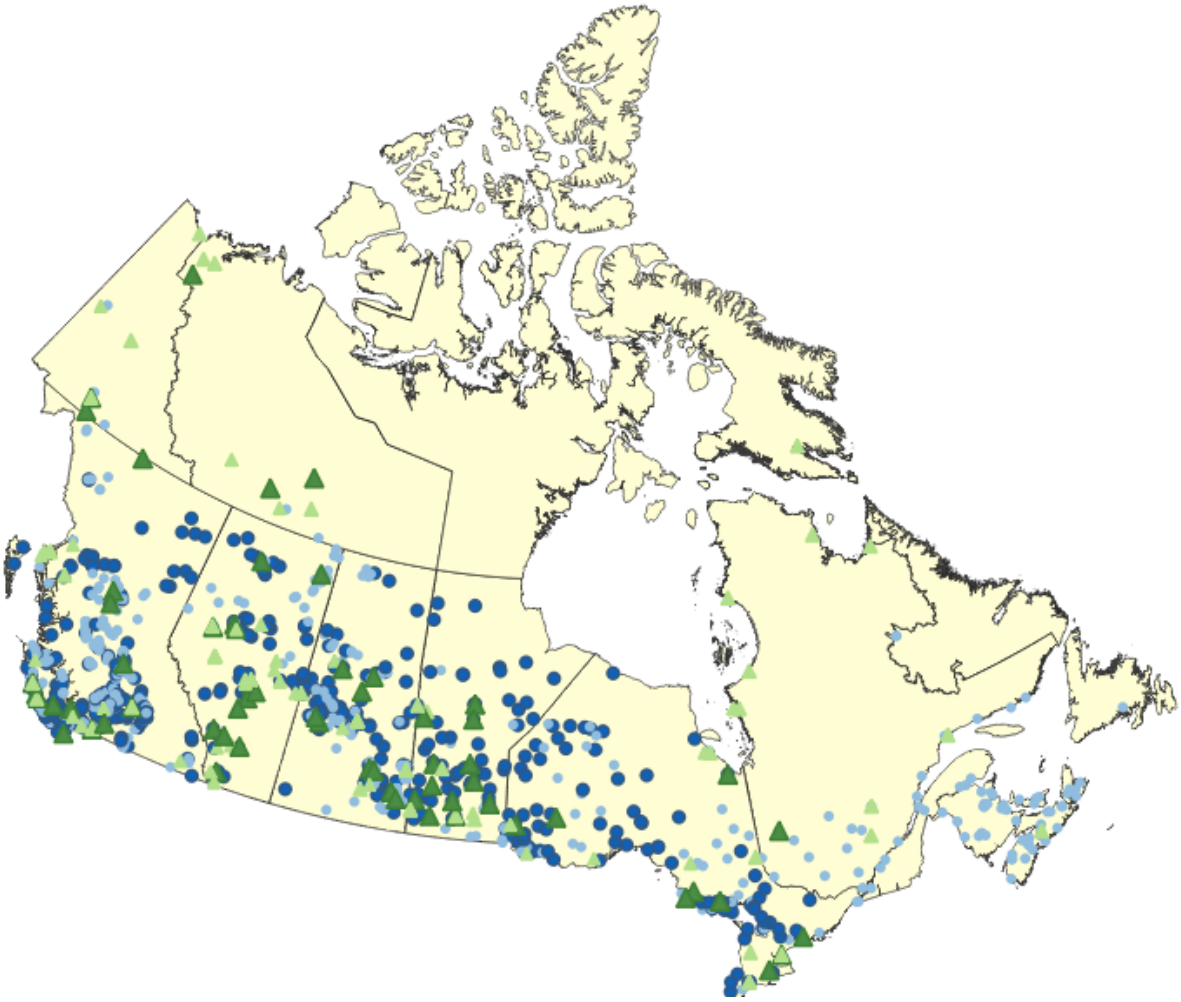


Figure (A3) *Data on Indigenous settlements and positions of residential school locations compiled from geographic sources cited in the geographic references Section. Circles are the reservation (2001 census subdivision) centroids and triangles are residential schools. Dark blue circles indicate that a reservation is included in the sample and light blue indicates it is not. Dark green indicates the residential school is included and light green indicates it is not. Schools are included if they are the closest school to at least one community and they opened or were still open after 1928. 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.*

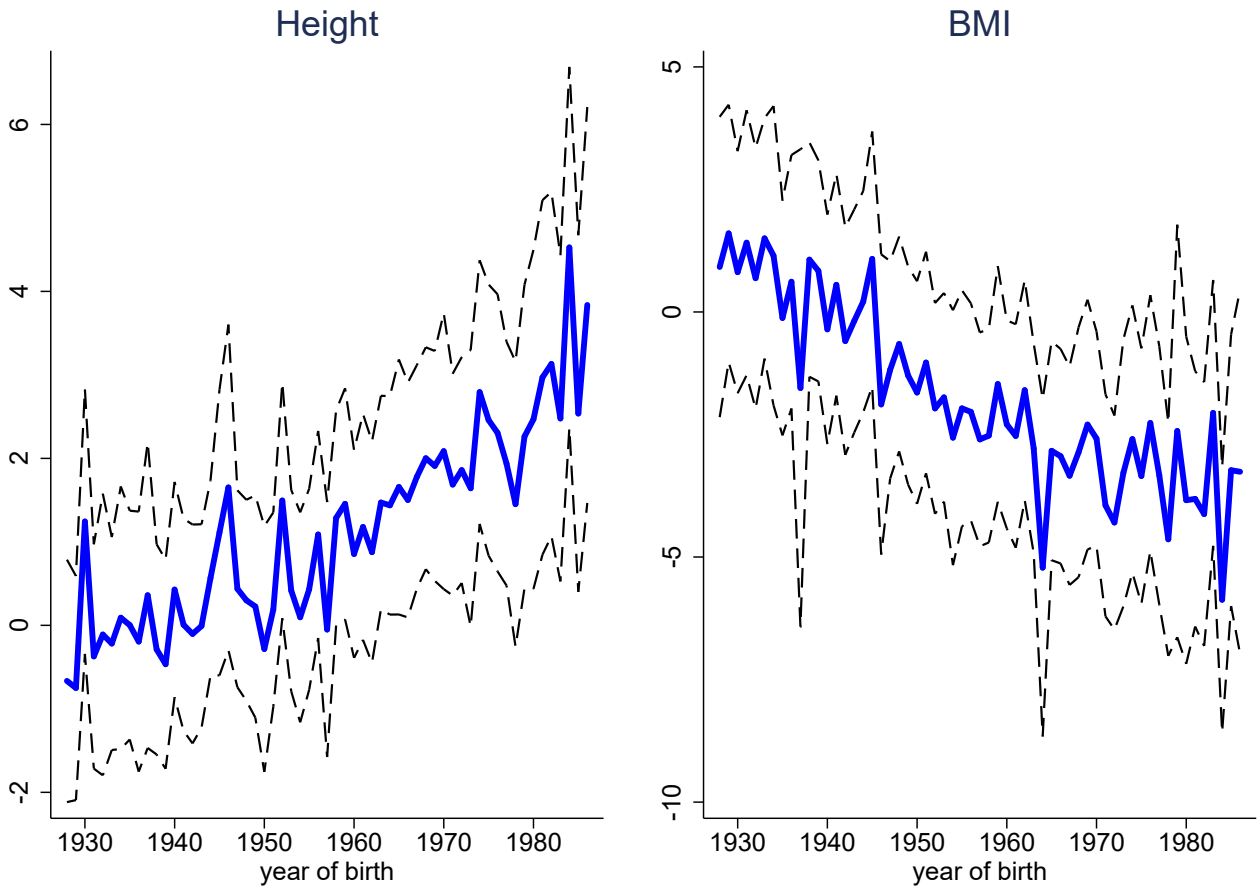


Figure (A4) **Variation by cohort in the causal effect of residential schooling: alternative specification.** *Figure shows estimated birth year effects from heterogeneous causal effects models of height and BMI (the birth year component of $(\beta_1 - \beta_0)$ from specification (3)). Positive values 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 gender dummy, province effects, and pre-treatment covariates as summarized in Table but also include variation in the distance of the closest residential school as an excluded instrument and exclude the survey wave indicator. Dashed lines display a 95% confidence interval.*