Coercive Assimilation Policy Across Generations

Evidence from American Indian Boarding Schools

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Culture and identity have fundamental economic, social, and political implications. Throughout history, governments, colonial powers, and other state actors have sought to reshape these characteristics through assimilation policies and indoctrination efforts, often targeting ethnic minorities. In this paper, I show that coercive assimilation policy can cause substantial cultural change among ethnic minorities, but that these effects do not necessarily persist into later generations, and may even reverse. I focus on a historical policy in the United States under which authorities removed Native American children to distant boarding schools. I exploit the staggered recruitment patterns of schools and variation in cohort exposure to facilitate causal identification. I show that exposure to boarding schools offered few economic benefits, but did lead to substantial cultural and social assimilation. Treated cohorts were more likely to speak English, more likely to give their children western names, and more likely to be perceived as 'White' in their communities. However, I find that the next generation tended to be less socially and economically integrated. I show that tribe- and family-level characteristics affected the trajectory of intergenerational outcomes, which, in line with the historical literature, points to resistance to the schools on the part of Native American individuals and communities.

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A great general has said that the only good Indian is a dead one... I agree with the sentiment, but only in this: that all the Indian there is in the race should be dead. Kill the Indian in him, and save the man.
– Richard Henry Pratt, Nineteenth Annual Conference of Charities and Correction, 1892

1. Introduction

A growing body of empirical work in Economics has shown that culture and identity have important economic, social, and political implications (Fernández 2011; Shayo 2020). More recently, the literature has found that 'fundamental' aspects of culture and identity – such as kinship traditions or ethnic identity – are remarkably malleable (Bau 2021; Atkin, Colson-Sihra, and Shayo 2021; Dahis, Nix, and Qian 2020). These findings are in line with the actions by governments, colonial powers, and other state actors, who, throughout history, have sought to reshape these characteristics through assimilation, nation-building, and indoctrination efforts. Prior research has shown that these policies do not always have the desired effects. While the tendency for culturally diverse groups (typically immigrants) to assimilate into their 'host' society has been well-documented (e.g., Abramitzky, Boustan, and Erikkson 2020), coercive state efforts to promote assimilation have sometimes had the opposite effect (Fouka 2022). We still have a limited understanding of the conditions under which efforts to reshape culture and identities succeed, the conditions under which they backfire, and the extent of persistence across generations. That said, prior work suggests that the degree of coercion is likely to play an important and possibly counterintuitive role.

One of the most coercive ways for governments to reshape identities is through the removal of children from their families for (re)education. Over the last century, removal policies have targeted indigenous populations across the world (e.g., in Australia, Canada, Denmark, and the United States, among others).[1] These policies generally sought to replace indigenous languages, religions and customs with those of the dominant society, with the justification that assimilation would lead to economic self-sufficiency (Smith 2009). In the Economics literature, modern assessments of indigenous boarding school programmes have shown that they caused a loss of cultural connection, as measured through indigenous language use or participation in traditional activities, but also generated long-term economic and educational benefits (e.g., Feir 2016; Gregg 2018).

[1] Indigenous children were removed to missions or adoptive families in Australia, to Church-run 'Indian residential schools' in Canada, and to state-run 'Off-reservation schools' in the United States. The policy in Denmark was similar in spirit, but smaller in scale, with 22 children removed from their families in Greenland and place in foster homes on the mainland in the early-1950s. On Australia and Canada see ABC News (2008) and CBC News (2008) respectively. On Denmark, see BBC News (2020). Indigenous boarding schools are currently the subject of a federal investigation in the United States (U.S. Department of the Interior 2024).

However, due to a lack of historical data, there has been no quantitative analysis focusing on the responses of indigenous individuals and communities to boarding schools at the time these schools were operational. Based on the historical literature, these effects are likely to have been nuanced in a way that is hard to capture with long-run data.[2] For example, indigenous boarding schools targeting Native Americans in the United States are generally seen to have been successful in promoting assimilation, to the extent that they created cultural barriers between students and their communities (Adams 2020, p. 303). But some scholars have also posited that these schools served the unintended purpose of strengthening Native American identity and facilitating indigenous activism later in the 20th century (Hertzberg 1971; Nagel 1996). An empirical investigation of these effects is important for understanding both the historical legacy of indigenous boarding schools, and the effects of coercive assimilation policies more generally.

In this paper, I provide one such empirical investigation. I focus on the above-mentioned indigenous boarding school programme in the United States, which was arguably the most coercive assimilation effort in the country's history. The policy, known as the off-reservation school system, involved the removal of Native American children from their communities (reservations) to distant boarding schools for periods of up to 5 years. The first off-reservation boarding schools opened in the late 1870s, and were rolled out across the country over the next 30 years. Once children entered the schools, educators sought to completely reshape their identities by banning the use of tribal languages and the practice of tribal religions, and promoting western cultural practices over their tribal alternatives. Off-reservation schools were considered to be more effective at achieving cultural assimilation than existing schools located on or near reservations, since children at the latter schools could still be visited and influenced by their families and communities.

Data limitations have inhibited the study of historical 'Indian policy', including the off-reservation school system. While Native Americans were enumerated from the 1890 population census onwards, key information needed to study historical policies – such as an individual's home reservation – has not been systematically digitised. There is also no comprehensive data on the timing of each reservation's exposure to off-reservation schools. This is the first paper to address both limitations. I match around 75 per cent

[2]The nuanced effects of the off-reservation system are well-illustrated by the case of Chauncey Yellow Robe. A 'full blood' Lakota Sioux from South Dakota, Yellow Robe entered the Carlisle Indian School in Pennsylvania in 1883, in 'full Indian costume… not knowing a word of English, not having seen a book or a schoolhouse before' (cited in Weinberg 2004, p. 17). He eventually came to be a model student, remaining at Carlisle for two terms, and graduating in 1894. Yellow Robe subsequently settled in Rapid City, South Dakota, and married a White American. However, while seamlessly assimilating into western society, Yellow Robe also maintained a strong connection with his indigenous identity. He actively sought to familiarise his daughters with the Lakota language, and with the customs and traditions of the tribe (Weinberg 2004, p. 29), and later joined the Society of American Indians (the first Native American-run civil rights organisation in the United States). Yellow Robe's eldest daughter, Rosebud Yellow Robe, went on to become a prominent Native American folklorist and educator. Clearly, none of this is observable in modern-day census or reservation-level data.

of Native Americans in the 1910 census to reservations, and in doing so, construct the first individual-to-reservation crosswalk for the 1910 census. I then obtain the dates that off-reservation schools first began recruiting from each reservation using a variety of primary and secondary historical sources, producing the first reservation-level dataset on off-reservation school recruitment patterns during their roll-out. Finally, to examine outcomes across generations, I link male individuals and their children across census years, from 1910 to 1940, using a newly-published database of links from the Census Tree (Price et al. 2023).[3]

A simple comparison between treated and non-treated reservations, or treated and non-treated cohorts, is unlikely to be informative about the causal effects of the off-reservation school system on educational, socioeconomic and cultural outcomes. For example, off-reservation schools may have targeted reservations that were more open to assimilation (confounding a simple cross-reservation comparison), and younger cohorts were likely to have greater exposure to White Americans (confounding a simple cross-cohort comparison). To address these concerns, I use an event study design that exploits two sources of variation: firstly, that reservations were exposed to off-reservation schools at different points in time, and secondly, that individuals already past schooling age were less likely to be recruited. Specifically, as I observe an individual's reservation and age in my data, as well as the year that their reservation was treated, I can infer exposure on the basis of age when an off-reservation school *first* started recruiting from their reservation.[4] This strategy allows me to account for unobservable reservation-level characteristics, as well as time-varying trends common to all cohorts, through the inclusion of reservation and cohort fixed effects. I am therefore able to estimate the causal effects of exposure to off-reservation schools at the reservation-by-cohort level.

I examine the effects of exposure to off-reservation schools on standard measures of cultural and social assimilation (e.g., intermarriage with White Americans, naming practices) and economic assimilation (i.e., measures of integration with the labour market, such as labour force participation) (e.g., Fouka 2019; Abramitzky, Boustan, and Eriksson 2014). I also examine a new, context-specific measure of assimilation: whether Native Americans linked across census years were counted as 'White' in the later census. Since race was inferred by census enumerators during this period (rather than being self-reported), I interpret this outcome as a measure of *community perceptions* of an individual's race (Dahis, Nix, and Qian 2020).

I find that the first generation to be exposed to off-reservation schools was more culturally

[3] My focus on men is common in the Economic History literature. During the early-20th century women typically changed their names at marriage, and are therefore more difficult to track across census years.

[4] This approach is informed by information on recommended 'Indian schooling ages' in historical publications by the Indian Office, as well as attendance records for selected off-reservation schools.

assimilated in 1910. In particular, treated cohorts were more likely to speak English, more likely to intermarry with White Americans, more likely to give their children western names, and when linked across census years, more likely to be counted as 'White' in the 1920 census. I then link children from first generation households to adults in 1940 (i.e., the second generation). Here, I find that adult children from treated first generation cohorts tended to be less socially and economically integrated. In particular, these individuals were more likely to live in rural areas, less likely to have a White spouse, and less likely to be counted as 'White' in 1940. I show that these results are robust to a battery of tests, including methods that account for potential biases of the standard two-way fixed-effect estimator in the presence of treatment effect heterogeneity.

In order to disentangle the mechanisms behind these effects, I show that the intergenerational trajectory of outcomes varied on the basis of community-level and family-level characteristics. With respect to community-level characteristics, I find that second-generation social and economic assimilation tended to be lower on reservations where tribes were (historically) organised into extended families or clans (where coordinated resistance to the schools may have been easier to achieve).[5] With respect to family-level characteristics, I show that there was a divergence between second generation individuals that grew up in more and less assimilated first generation households, with the main effects driven by the less-assimilated group.

Finally, I provide evidence against an alternative mechanism: that assimilation in the first generation – for example, migration to white-majority urban areas – led to increased discrimination against and / or exclusion of the second generation (Fouka, Mazumder, and Tabellini 2021). I show that inflows of Native Americans to metropolitan areas were negligible in the first decades of the 20th century, and that public attitudes towards Native Americans – as proxied by the language used in historical newspapers – remained stable over the same period.

My paper studies a question that is central to understanding economic, social and political interactions in multiethnic societies, and one that in many countries remains a divisive political issue: the extent to which ethnic minorities (immigrant, indigenous or otherwise) successfully integrate into their 'host' society, and whether state policies help or hinder this process. In this respect, my work is related to the substantial literature in Economic History that studies the cultural assimilation and economic integration of immigrants to the United States during the 'Age of Mass Migration', from the mid-19th to early-20th centuries (e.g., Abramitzky, Boustan, and Eriksson 2014; Abramitzky, Boustan, and Erikkson 2020). Other

[5] Recent work by Carvalho, Koyama, and Williams (2023) provides a theoretical analysis of resistance to educational institutions, and the implications of such resistance for the transmission of (minority) cultural traits.

studies have highlighted the role of policy, including education policy, as a means of promoting cultural assimilation during this period (Bandiera et al. 2018). These policies have not always shifted attitudes and behaviours in the intended direction. For example, Fouka (2019) shows that coercive assimilation policies in the early-20th century targeting German Americans led to stronger ethnic German identification, contrary to the intent of the policies.

I make a number of contributions to these literatures. Firstly, with respect to the literature on immigration, a key barrier to studying assimilation patterns is that information on many relevant dimensions of cultural assimilation, such as a person's accent or form of dress, has not been systematically collected. This is a particular limitation when studying the assimilation of first generation immigrants (Abramitzky, Boustan, and Erikkson 2020). A related challenge in the literature has been to distinguish between observed assimilation (an equilibrium outcome) and effort on the part of immigrants to assimilate (Fouka, Mazumder, and Tabellini 2021). In this paper, I propose a measure of cultural assimilation that arguably captures both the unobserved dimensions of assimilation, and effort to assimilate: whether a Native American individual was perceived (and therefore recorded in the census) as 'White' by census enumerators.

Secondly, with respect to work that has studied the state's role in assimilation efforts, I examine a policy that is arguably the most coercive attempt to reshape individual attitudes, behaviours and identities in US history. While policies during the Age of Mass Migration sought to instil American values in culturally-similar European immigrants, off-reservation schools represented an effort to completely replace the identities of diverse and culturally-dissimilar indigenous populations, many of which had little familiarity with western cultural practices or traditions (Adams 2020, pp. 109-121).

Thirdly, since off-reservation schools were opened from the late-1870s, I am able to study the intergenerational effects of coercive assimilation policies across 40 years of historical census data: in particular, the effects of parental exposure to coercive assimilation policies on the outcomes of their *adult* children. As de-anonymised census data is only available up to 1950, this kind of analysis is not possible for policies implemented in later years (e.g., the German language ban studied in Fouka 2019).[6]

My paper is also closely related to recent work on racial identification in the United States. Among these, research by Dahis, Nix, and Qian (2020) studies the phenomenon of 'passing' for white by African Americans in the early 20th century. Linking across census years from 1880 to 1940, they find that a higher share of African American men 'passed' (i.e., switched race from 'Black' to 'White') than previously thought, and that the decision to pass was

[6]The de-anonymised 1950 census was released in 2022, but has not yet been fully digitised.

influenced by discrimination, as well as educational and employment opportunities. With respect to this literature, my newly-constructed datasets and research design allow me to estimate, for the first time, the causal effects of coercive assimilation policies on shifts in racial classification in the United States.

Finally, my study adds to research on the socioeconomic and cultural consequences of indigenous boarding school policies.[7] This topic has received more attention in the historical and sociological literature (e.g., Adams 2020; Lomawaima 1994; Hertzberg 1971; Nagel 1996), and has been the subject of numerous government reports (e.g., Australian Human Rights Commission 1997; U.S. Department of the Interior 2024). There are fewer studies that estimate the causal effects of exposure to indigenous boarding schools. The most closely related work is Gregg (2018), who finds that modern-day reservations with greater historic exposure to off-reservation schools are more culturally assimilated (e.g., smaller family sizes and a greater proportion of individuals that exclusively speak English) and economically prosperous. Another two closely related studies are Feir (2016) and Jones (2022), who both examine the long-term effects of Canada's Indian Residential School system. With respect to this literature, my historical datasets allow me to study a richer set of assimilation-relevant outcomes, including intermarriage and racial classification, and to obtain causal estimates of the integenerational effects of indigenous boarding school policies. I am also able to starkly document the effects of off-reservation schools on cohorts (and their children) with varying levels of exposure using my event study design, which is not possible with long-run data.

The remainder of this paper is structured as follows. Section 2 provides context on the off-reservation school system and details on the enumeration of Native Americans in historical censuses. Section 3 outlines the main data sources used in my analysis. Section 4 presents my empirical strategy, and section 5 sets out my main results. Section 6 discusses mechanisms, and section 7 concludes.

2. Context

When Europeans first arrived in North America, the indigenous population is estimated to have been between 2 and 5 million, with more than 1000 distinct communities (Nagel 1996, p. 4). Over the next two centuries, diseases, wars, the loss of traditional food sources and forced relocation decimated the Native American population. By the mid-19th century, Native American military resistance had been overcome and populations were largely

[7] There is also a growing literature in economics that studies the effects of other historical indigenous policies (e.g., reservation formation, the elimination of traditional food sources, and self-governance) on contemporary outcomes (respectively, Dippel 2014; Feir, Gillezeau, and Jones 2023; Frye and Parker 2021).

confined to reservations under the administration of federal 'Indian agents'.[8] Native Americans were now considered 'wards of the government; the duty of the latter being to protect them, to educate them in industry, the arts of civilization... to sustain and clothe them until they can support themselves' (Board of Indian Commissioners 1869, p. 10).

2.1. Indian education policy prior to off-reservation schools

From the second half of the 19th century, the federal government became increasingly involved in the provision of education to Native Americans (Vuckovic 2008, p. 12). Education policy had four main aims: to provide a basic academic education, to give Native Americans training in practical skills and trades, to encourage cultural assimilation, and to promote Christianity over tribal religions (Adams 2020, pp. 24-29).

Two forms of schools existed prior to the development of the off-reservation system. The first form was the reservation day school. These schools were located near villages, with children returning to their families at the end of each school day. Policymakers soon came to the view that day schools did not sufficiently separate children from the influence and practices of their families and communities. In 1878, for example, the Indian agent of the Shoshone and Bannock Agency (Wyoming) opined that placing Native American children 'under a teacher's care but four or five hours a day... to spend the other nineteen in the filth and degradation of the village, makes the attempt to educate and civilize them a mere farce' (Office of Indian Affairs 1878, cited in Adams 2020, p. 34). The second form of schools, developed in the 1870s in response to the above concerns, were reservation boarding schools. These were also located on or near reservations. Children lived at reservation boarding schools during the school term, but returned home for vacations. By 1879, when the first off-reservation schools began taking students, there were 107 day schools and 52 reservation boarding schools in 14 states. The total attendance across all schools was 4,448 students, or between 7 - 10 per cent of school-age Native American children (Office of Indian Affairs 1909, p. 87).[9] A breakdown of attendance between boarding and day schools is only available from 1882 onwards. In 1882, reservation boarding schools accounted for 50 per cent of total school attendance, reservation day schools accounted for 30 per cent, and off-reservation schools (at the time the Carlisle Indian School, and the Chemawa Indian School) accounted for 5 per cent. The remainder attended non-government operated contract schools.

[8] Indian agents were federal employees responsible for the day-to-day administration of Native American reservations. Among other things, Indian agents were responsible for disbursing food rations, building and maintaining infrastructure, and enrolling children at schools. On average, agencies had jurisdiction over 1.5 reservations (Gregg 2018).

[9] School-age population is calculated from agency-level statistical tables in Office of Indian Affairs (1879). Since some agencies did not report school-age population, I estimate school-age population as 20 per cent of total agency population.

While children remained in reservation boarding schools for around nine months of the year, policymakers eventually concluded that these schools also failed to sufficiently remove children from tribal influences. Children were found to 'relapse' into tribal ways during vacation periods, and parents were still able to easily visit their children (Adams 2020, p. 37).

2.2. Rise of off-reservation schools

The first off-reservation school, the Carlisle Indian School (Pennsylvania), was opened in 1879.[10] Carlisle was the culmination of efforts by an Army officer, Captain Richard Henry Pratt, to develop a new model of education that could rapidly assimilate Native American children into mainstream society. Under Pratt's vision, children would be removed from the influence of the reservation, where they could be completely immersed in western society for an extended period. Policymakers saw promise in this model, and new schools opened rapidly over the next 20 years, stabilising at 25 schools in the early 1900s (Figure 1). By 1912, when the last off-reservation school was opened in Tacoma (Washington), there were 27 schools in operation. Figure 2 shows the distribution of off-reservation schools with respect to Native American reservations as at 1889. Table A.1 in the Appendix shows the locations and opening years of off-reservation schools from 1879 until 1912.

2.3. Off-reservation school recruitment practices

The recruitment practices of off-reservation schools varied from school to school and across time, but did follow some general principles.

With respect to coercion, some (but not all) children were forcibly taken in the early years of the off-reservation system. This practice was banned in 1893, when the Commissioner of Indian Affairs ruled that parental consent was required to send children to schools outside the reservation (Adams 2020, p. 71).[11] In theory, coercion was further restricted in 1894 when Congress banned Indian agents and other government employees from obtaining parental consent 'by withholding rations or by other improper means' (cited in Prucha 1984, p. 905). However, the extent to which these laws were respected is unclear. Indeed, the fact that rules prohibiting forced removals needed to be re-issued in 1917 suggests that such practices continued well after 1893 (Lomawaima 1994, p. 36).

The school age designated for Native American children was set at 6 to 16 years (Office of Indian Affairs 1890, p. 452).[12] Off-reservation school administrators were encouraged to

[10] Another boarding school, the Hampton Institute (Virginia), took its first Native American students in late-1878 but was not formally part of the off-reservation system.

[11] At the time, the Commissioner justified the ruling on the basis that 'even ignorant and superstitious parents have rights' (cited in Adams 2020, p. 71).

[12] This band was widened to 5 to 18 years in 1891 (Office of Indian Affairs 1891, p. 511).

FIGURE 1. Number and pupil share of off-reservation schools, 1879 - 1909

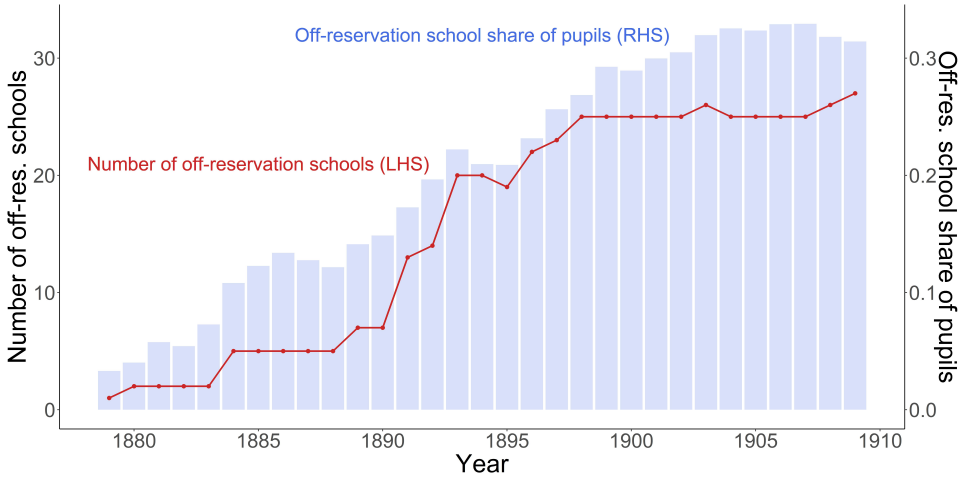


Figure shows the cumulative number of off-reservation schools between 1879 and 1909 (left axis) and the share of pupils attending off-reservation schools (right axis). Share of pupils attending off-reservation schools is calculated as the average attendance at off-reservation schools over the average attendance at all schools (federal government boarding and day, non-government boarding and day, and state public schools) in a given year.
Source: Own calculations using data from Annual Reports of the Commissioner of Indian Affairs, 1880 - 1900.

FIGURE 2. Distribution of off-reservation schools, 1912

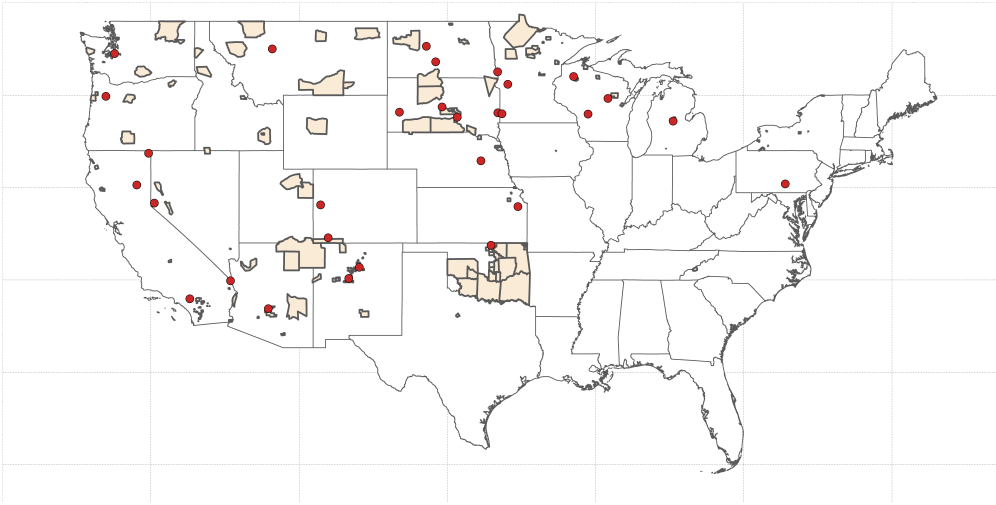


Figure shows off-reservation schools in operation in 1912. Red points represent off-reservation schools, and polygons represent reservation boundaries as at 1889.
Source: Own calculations using digitised map of reservations originally from Office of Indian Affairs (1889) and school locations from Adams (2020).

target individuals at the upper end of this band, who had already received some education. That said, individuals above and below these bands were recruited by off-reservation schools. The Commissioner of Indian Affairs recommended an upper limit of 21 years in 1902 (Office of Indian Affairs 1902).[13]

Off-reservation schools were also encouraged to target 'full blood' children (i.e. those without, or with very little, white ancestry), as they were considered most in need of assimilation. However, administrative reports from the time suggest that a substantial number of students had at least some white ancestry (e.g., Office of Indian Affairs 1897, p. 319). A preference for students with a low percentage of 'white blood' and / or those 'living in Indian fashion' was formalised in 1902. Finally, with the exception of two large schools (Carlisle and the Haskell Institute, Kansas) that recruited across the entire country, off-reservation schools tended to recruit from within their state or from adjacent states. In part, this practice was motivated by concerns about the costs of transporting children between their communities and off-reservation schools (Gregg 2018). By the mid-1890s, the Office of Indian Affairs began to formally restrict off-reservation school recruiting zones, which typically covered the school's state, and for larger schools, surrounding states.

2.4. Decline of off-reservation schools

While enrollment in off-reservation schools peaked in 1915 and remained relatively stable for the next 15 years, the model began to fall out of favour in the early 20th century. Policymakers came to see the goal of rapid assimilation as unfeasible, and the practice of separating children from their families as cruel (Adams 2020, pp. 338-339). In 1907, then-Commissioner of Indian Affairs Francis Leupp argued for closing off-reservation schools and enlarging the day schools system (Adams 2020, p. 348), and from 1908 off-reservation schools were banned from directly recruiting students at reservations (though parents that wished to send their children to such schools could still do so) (Prucha 1984, p. 820). Between 1900 and 1925 the number of off-reservation schools fell from 25 to 18 (including the closure of Carlisle). A comprehensive review of federal Indian policy published in 1928 was highly critical of off-reservation schools, noting a lack of nutrition, poor sanitary conditions, and the fact that the industrial training offered at the schools was irrelevant to students that returned to their reservations (Institute for Government Research 1928). In response to the review, another round of school closures occurred over the next decade. While this did not signal the end of the off-reservation school system, it did represent the end of their role in the aggressive assimilation of Native American children.

[13]The Circular states: 'The enrollment of young Indian men and women… will only be permitted when valid reasons are assigned. When such persons have reached the age of 21 years, it is ordinarily time for them to cease leaning on the arm of government and endeavor to make a living for themselves'.

2.5. Off-reservation schools and reservation schooling alternatives

In order to understand how off-reservation schools may have affected Native American children differently from on-reservation alternatives, it is helpful to highlight the similarities and differences between off-reservation boarding schools and alternatives on or near reservations. As discussed above, there were two types of schools on reservations: day schools and reservation boarding schools.

Due to their isolation, small size, few teachers (generally one teacher and their assistant) and lack of furnishings, day schools typically offered a poorer academic education than reservation and off-reservation boarding schools, with instruction mostly at the primary grades (Office of Indian Affairs 1890, p. XIII and p. CLIV). Initially, the academic education provided at reservation and off-reservation boarding schools was generally at a similar level. As government-operated institutions, off-reservation schools followed a similar curriculum to reservation boarding schools.[14] While the largest off-reservation schools, such as Carlisle, eventually offered a post-primary education, this was not significantly more advanced than the education available at large reservation schools.[15] Pratt himself stated that Carlisle 'would not attempt even a high school education… graduation was fixed between the grammar and high school grades of our public schools' (Pratt 1912, p. 13). Off-reservation schools only began to offer a high school curriculum in the 1920s (Gregg 2018).

Historical administrative data support the view that the quality of education at reservation boarding schools and off-reservation boarding schools was similar. School statistics in the 1900 Annual Report to the Commissioner of Indian Affairs indicate that the cost per pupil at off-reservation boarding schools was around $148, while the cost per pupil at reservation boarding schools was slightly higher, at $151 (Office of Indian Affairs 1890, p. 635). According to the same statistics, off-reservation schools reported 10 pupils per employee, while the corresponding figure at reservation boarding schools was 6 pupils per employee (the statistics do not distinguish between teachers and other employees). While these statistics are imperfect proxies of school quality, they at least suggest that the quality of education at off-reservation schools was not significantly higher than that at reservation boarding schools.[16]

[14]The curriculum for Native Americans, known as the 'course of study' was formalised in 1890 (Office of Indian Affairs 1890, pp. CLVI-CLX).

[15]The 1890 Annual Report stated that off-reservation schools 'are not universities, nor colleges, nor academies nor high schools. In the best of them the work done is not above that of an ordinary grammar school, while in most it is of the primary or intermediate grade. The pupils come to them for the most part ignorant of the English language, unaccustomed to study, impatient of restraint, and bringing, with them many of the vices and degraded habits of camp life' (Office of Indian Affairs 1890, p. IX).

[16]In line with the discussion in the preceding paragraph, both measures of school quality were lower at reservation day schools. Days schools operated at a cost per pupil of $48, and had 13 pupils per employee.

The main differences between off-reservation and reservation boarding schools were geographical and operational. Firstly, while some off-reservation schools were closer to reservations than others, they were invariably located outside Native American communities. This limited the ability of families and / or communities to influence the education of their children. Secondly, while children at reservation boarding schools returned home at least once a year, children at off-reservation schools typically remained there, continuously, for three to five years. Thirdly, while reservation boarding schools recruited directly from reservations, and were therefore ethnically homogeneous, children from different tribes were deliberately mixed at off-reservation schools. This policy was, in part, intended to promote the use of English: while indigenous languages were banned or discouraged in both reservation and off-reservation boarding schools, it is likely that such a rule was harder to enforce in schools where the same language was spoken by the entire student body (Adams 2020, p. 154).[17]

Another practice specific to off-reservation schools, and likely to promote cultural assimilation, was the 'outing programme', which placed students with white families for up to a year (Adams 2020, pp. 174-175). This programme was first implemented at Carlisle, and subsequently rolled out to other schools.[18]

Given the differences described above, one might expect that off-reservation schools were more effective than reservation boarding schools in breaking the cultural connection between Native American children and their families and communities.[19] This hypothesis is supported by historical reports from the Indian Office, as well as the personal accounts of returned students. For those that did return to reservations, there were sometimes clear cultural barriers between students and their families (Adams 2020, p. 303). Students that had spent three to five years speaking English sometimes found it difficult to communicate with their families in their own languages (Child 1993, p. 76). Cultural differences between returned students and their communities were also evident in episodes where tribes enforced compliance with rituals by way of economic sanctions and / or corporal

[17] Policymakers and school administrators were aware of the effects of combining children from linguistically diverse tribes. In 1896, the Superintendent of Wittenberg Indian School (Wisconsin) reported that 'it has been and is one of the main principles of the school to have children from different tribes about equally divided… forcing the use of the English language' (Office of Indian Affairs 1896, p. 47). Similarly, in 1898 the Superintendent of Phoenix Indian School (Arizona) opined that 'the intermingling of different tongues is the surest and best way to teach English and broaden the tribal view' (Office of Indian Affairs 1898, p. 365).

[18] Pratt considered the outing programme as 'the best possible means of inducting Indian boys and girls into our civilized family and national life' (Office of Indian Affairs 1897, p. 30).

[19] Off-reservation schools were generally seen as ineffective in promoting economic integration. After attending off-reservation schools, students typically returned to their home reservations, where they often had difficulties finding work (Office of Indian Affairs 1898, p. 339). While students had received training in farming and various trades as part of their vocational education, the land on reservations was often unsuitable for the former, and there was limited demand for the latter (Adams 2020, pp. 308-309). Authorities eventually sought to address these problems by providing returned students preferential access to clerical jobs, as well as establishing the Indian Employment Bureau in 1905 (Adams 2020, p. 325).

punishment (Office of Indian Affairs 1887, p. 168). At the same time, it is also possible that coercive efforts to suppress Native American identities at off-reservation schools strengthened the resolve of individuals and their communities to maintain these identities. There is evidence of such responses in the personal accounts of returned students. For example, upon returning to their reservations and realising they could not communicate comfortably with their parents, some returned students vowed to relearn their languages (e.g., Rogers 1974, cited in Child 1993). Others actively sought to educate their own children in the language, customs and history of their tribes (e.g., Weinberg 2004, p. 29).

2.6. Native Americans and racial classification in census records

The availability of information on Native Americans in historical censuses (i.e., 1790 to 1940) varies from census to census. No attempt was made to count Native Americans until the 1860 census, and this enumeration only included individuals that had left their tribes and lived in white communities. Native Americans, both on reservations and in white communities, were in principle fully counted from the 1890 census onwards.[20]

The information collected on Native Americans also varied from census to census. In 1900 and 1910, a special schedule was used to enumerate Native Americans. This schedule included information on an individual's tribe, degree of 'Indian blood' (e.g., 'full', 'half', 'quarter'), their 'Indian name' (if any), whether they were living in a polygamous relationship, and their dwelling type ('Aboriginal' or 'Civilized'). These 'Indian schedules' were not used in the 1920, 1930 or 1940 census. However, census enumerators were given special instructions to collect information on tribal affiliation and 'Indian blood' in the normal schedules for the 1930 census.

A practical implication of the change in schedules and instructions to enumerators is that many individuals that had been counted as 'Indian' in 1910 or 1930 were counted as 'White' in 1920 and in 1940 (Bureau of the Census 1937, 1960).[21] Many individuals that switched from 'Indian' to 'White' were likely to have had mixed ancestry, but 'full blood' individuals without identifiably 'Indian' traits were also counted as 'White'.[22]

[20] Individual records from the 1890 census are no longer available today. For more information on the fate of these records, see: `https://www.archives.gov/publications/prologue/1996/spring/1890-census`.

[21] According to Bureau of the Census (1960, p. 3): "In 1910 a special effort was made to secure a complete enumeration of persons with any perceptible amount of Indian ancestry. This probably resulted in the enumeration as Indian of a considerable number of persons who would have been reported as white in earlier censuses. There were no special efforts in 1920, and the returns showed a much smaller number of Indians than in 1910". Similarly, a supplementary report to the 1930 census estimated that at 20,000 individuals counted as 'Indian' in 1910 were counted as 'White' in 1920 in Oklahoma.

[22] Sherman Coolidge, a 'full-blood' Arapaho and founder of the Society of American Indians, was himself counted as 'White' in 1920.

It is important to note that race was not self-reported until the 1960 census. This means that an individual's racial classification in historical censuses reflects the perceptions of census enumerators, and may not correspond to the individual's own racial identity. However, as highlighted by Dahis, Nix, and Qian (2020), a change in racial classification required 'a change in lifestyle and situation so that a person would be accepted as white by those he encountered'. Such individuals, presumably, would have abandoned all identifiably 'Indian' traits (e.g., dress and language), lived away from reservations, and not engaged in tribal customs or practices. Therefore, even if individuals that were counted as 'White' did not actively intend to 'pass', their classification is still highly informative about the degree to which they had assimilated into western society.

3. Data

3.1. Data sources

This section outlines the main data sources used in my empirical analysis. More detailed information on data sources can be found in section B.1 of the Appendix.

Annual Reports of the Commissioner of Indian Affairs. My main sources of historical administrative data are annual reports by the Office of Indian Affairs from 1876 to 1900, known as the Annual Reports of the Commissioner of Indian Affairs ('Annual Reports'). I extract information from and / or digitise off-reservation school reports, schedules listing the tribes occupying reservations, and statistical tables with information on (pre-1879) agency-level characteristics, all contained in the Annual Reports.[23] In addition, I digitise a map of historical reservation boundaries from the 1889 Annual Report.[24]

Off-reservation school attendance records. Attendance records for off-reservation schools, provided they were kept, are stored in various National Archives facilities across the United States. These records have not been systematically digitised. However, I have obtained attendance records from five off-reservation schools: Carlisle (from the Carlisle Indian School Digital Resource Center), Chilocco (from the Oklahoma Historical Society), Hampton (from Brudvig 1994), Chemawa, and Haskell (both digitised from images on FamilySearch.org). These data cover the universe of attendees at these schools from their opening dates until their closures (Carlisle, Chilocco, Hampton) or 1900 (Chemawa,

[23] While the content of the statistical tables differs from year to year, common variables include the number of Native Americans of mixed ancestry, the number of Native Americans wearing western attire, and school-age population.

[24] I use the 1889 map because it is the earliest map that captures the last major changes to reservation boundaries (e.g, the splitting of the Great Sioux reservation in South Dakota into five smaller reservations in 1888). The 1889 reservation boundaries, therefore, are highly representative of actual reservation boundaries over the period of my analysis.

Haskell). These five schools accounted for two thirds of attendance at off-reservation schools in 1900 (Office of Indian Affairs 1900, p. 16). Records typically contain the names, ages (at entry), tribes and home agencies / addresses of attendees.

Historical censuses. My individual-level data are taken from two types of historical censuses in the United States. Firstly, I draw on the publicly-available and restricted full count censuses for the years 1910, 1920, 1930 and 1940 from IPUMS (Ruggles et al. 2021). In principle, the censuses cover the entire population of the United States. The number of individuals classified as 'Indian' in the censuses range from 280,000 (in 1910) to 350,000 (in 1940).[25] The publicly available censuses include information on an individual's location at the time of enumeration, as well as demographic and socioeconomic information (e.g., age, state of birth, marital status, employment status). In addition to the variables in the publicly-available censuses, the restricted censuses include first names and surnames, which are essential for linking individuals across census years.

Secondly, as the full count censuses do not include information on tribes (which is needed to match individuals to reservations), I draw on more detailed state-level 'Indian censuses' conducted by Indian agents.[26] Indian censuses contain the names, ages, tribes and reservations of Native Americans that fell under a given agency's jurisdiction. Indian censuses were not conducted by every agency every year, so I construct a complete cross-section using censuses on or around 1910. There are around 250,000 individuals in the cross-section.[27]

Thirdly, I construct an analogous cross-section of Indian censuses on or around 1930. This cross-section consists of roughly 320,000 individuals (in line with the 330,000 in the 1930 population census).[28] The sole use of the 1930 Indian census is to measure attachment to one's reservation or agency (described in section B.3).

The Census Tree. In order to link individuals across historical censuses, I use links published by the Census Tree (Price et al. 2023). The Census Tree combines and harmonises

[25] I identify Native Americans using the RACE variable from IPUMS.

[26] The collection of Indian censuses is known as the Indian Census Rolls. The Rolls can be accessed through FamilySearch.org and Ancestry.com.

[27] The total in my cross-section is lower than the 280,000 individuals enumerated as 'Indian' in 1910 because Indian censuses over this period did not cover Alaska Natives (around 20,000 individuals), Native Americans living in states without Indian agents (e.g., Louisiana, Maine, Mississippi) or some communities that were never subject to federal supervision (e.g., the Lumbee in North Carolina). Since Indian censuses were not taken for the Cherokee, Chickasaw, Choctaw, Creek and Seminole in Oklahoma, I obtain equivalent information from enumerations of these tribes in the Dawes Rolls, 1907. For more details, please see section B.1 of the Appendix.

[28] In part, the total population in the 1930 Indian census is lower than that in the 1930 population census because I continue to rely on the 1907 records for the Cherokee, Chickasaw, Choctaw, Creek and Seminole in Oklahoma.

links generated by five record-linking procedures, including the ABE NYSIIS Standard algorithm (Abramitzky, Boustan, and Eriksson 2014), which has been commonly used in the Economic History literature. The Census Tree achieves significantly higher match rates than existing methods, and represents the frontier for linking across historical censuses. The higher match rates achieved by the Census Tree are particularly desirable when studying underrepresented groups, and the data are starting to be used for this purpose by economists (e.g., Abramitzky et al. 2023).

'Western' names. In order to identify 'western' first names, I use a list of saint and biblical names compiled by Abramitzky, Boustan, and Eriksson (2016).[29] These data contain around 6,300 names of Judeo-Christian origin, including non-English versions (e.g., both 'John' and 'Jan', 'Jean', 'Johann' and 'Juan'.)

3.2. Data construction

3.2.1. Matching Native Americans to reservations in 1910

Around 260,000 Native Americans were counted in continental US states in the 1910 census.[30] I use information on locations in the 1910 census, as well as my cross-section of Indian censuses, to match around 75 per cent of these individuals to a unique reservation. I focus on the 1910 census because it is the first census in which the majority of the first generation to be exposed to off-reservation schools (i.e., born between roughly 1860 and 1890) had reached adulthood.[31]

In order to match individuals on the basis of location, I follow a similar strategy to Dippel and Frye (2020), geocoding location names using Google Maps, and overlaying these on historical reservation boundaries. All individuals residing in a place that can be matched to a unique reservation within a distance of 100 kilometres are assigned to that reservation.[32] Because this procedure only matches individuals that were living near reservations in 1910, I supplement location-based matches using the cross-section of Indian censuses. The procedure involves matching individuals in the Indian census to the 1910 census (to obtain their tribe), and then matching tribes to reservations using reservation schedules from the Annual Reports.[33]

[29] I thank Ran Abramitzky for sharing these data.

[30] This figure excludes roughly 20,000 Alaska Natives.

[31] Information on locations comes from enumeration district descriptions (obtained from `https://stevemorse.org/census/unified.html`) and standardised minor civil divisions (a string variable available in the restricted IPUMS data). While reservations are sometimes directly named, this is not always the case (e.g., a description may refer to a town inside a reservation).

[32] Results are robust to using different distance thresholds.

[33] In order to match between the Indian census and the 1910 census, I use the ABE-JW algorithm (Abramitzky, Boustan, and Eriksson 2019). This algorithm requires exact matches on state of birth, allows for minor spelling

In total, I am able to match around 195,000 individuals to a unique reservation or settlement. This represents 75 per cent of the Native American population in the 1910 census. Further information on the matching procedure is available in section B.2.2 of the Appendix.

3.2.2. Off-reservation school treatment years

I construct the first dataset on reservation-level exposure to off-reservation schools during the 19th and early-20th centuries. I begin by reading all off-reservation school reports contained in Annual Reports from 1879 to 1900. I use information on tribes, reservations and / or agencies contained in school reports, along with tribe-to-reservation correspondences derived from reservation schedules, to identify the years each off-reservation school recruited from a particular reservation. I use information from my attendance data to identify treatment years of reservations that were visited by Carlisle, Chemawa, Hampton and Haskell.[34] Finally, I supplement these data with information from secondary historical sources (typically research articles or theses on specific off-reservation schools).

I take the first year that any off-reservation school visited a reservation as that reservation's treatment year. I am able to identify 137 reservations or settlements that were treated on or before 1900 (out of the 148 reservations or settlements that existed in 1900). Figure 3 plots the number of reservations treated for the first time in a given year, and the cumulative number of reservations that were treated up to and including that year. Treatment years were fairly evenly spread over the period 1879 to 1900, which is consistent with the gradual roll-out of off-reservation schools across the country. Further information on the matching procedure, including a list of secondary historical sources, is available in section B.2.3 of the Appendix.

3.2.3. Linking across datasets and census years

I combine the two datasets above to match Native Americans in the 1910 census to reservations, and then to assign a treatment year to each individual on the basis of their reservation. I also link individuals between attendance records, and the list of 'western' names compiled by Abramitzky, Boustan, and Eriksson (2016) to the 1910 census using a standard algorithm in the Economic History literature.[35] Finally, I link individuals across census years (namely, 1910 to 1920, and 1910 to 1940). Here, I draw on the Census Tree. Of the 260,000 Native Americans enumerated in continental US states in 1910, the Census

differences in names, and for birth years to differ by +/- 5 years between records. I use publicly available codes made available by the Census Linking Project (`https://censuslinkingproject.org`).

[34] I do not use Chilocco attendance records for this purpose, as the reported entry years in the attendance data are inconsistent with annual attendance figures from school reports.

[35] When linking across datasets other than historical censuses, I use the ABE-JW algorithm (Abramitzky, Boustan, and Eriksson 2019).

FIGURE 3. Off-reservation school treatment years, 1879 - 1900

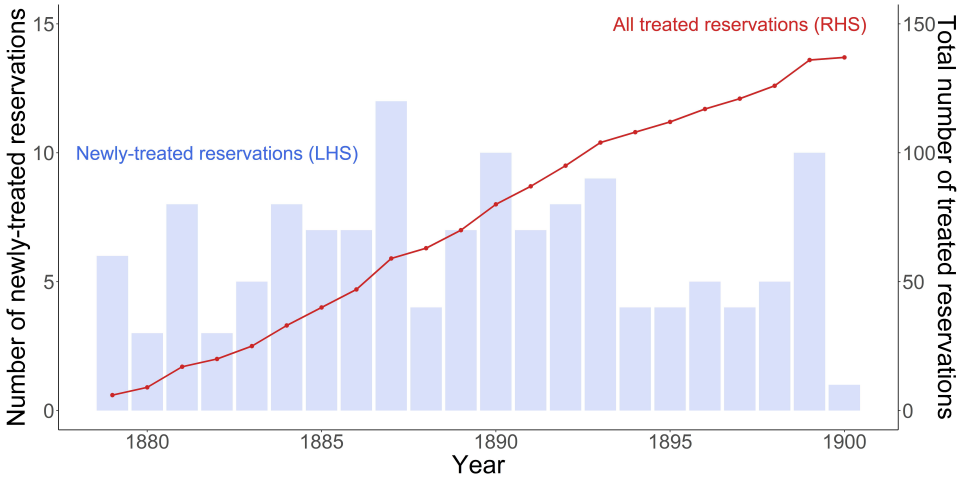


Figure shows the number of reservations that were treated by an off-reservation school for the first time in a given year (left axis) and the total number of reservations (out of 148) treated by an off-reservation school up to and including that year (right axis).
Source: Own calculations using data from Annual Reports of the Commissioner of Indian Affairs, 1880 - 1900, attendance records for Carlisle, Chemawa, Hampton and Haskell, and secondary sources.

Tree links 93,000 (36 per cent) to the 1920 census, and 56,000 (22 per cent) to the 1940 census.

Individuals are not randomly selected into linked samples. For this reason, I follow the literature by reweighting linked observations by the probability of being linked (Bailey, Cole, and Massey 2020). Specifically, I predict the probability that an individual is linked using a set of baseline (1910) characteristics in a probit regression, and then weight observations used in regressions by the inverse of these probabilities. Further details on differences between the unlinked and linked samples, as well as the reweighting procedure, are available in section B.2.5 of the Appendix.

3.3. Outcomes and samples

3.3.1. Outcomes

I primarily obtain outcomes from the 1910, 1920, and 1940 censuses; two outcomes (attendance, and western names) are obtained by matching the relevant datasets to the 1910 census. Educational outcomes include indicators for: attending one of Carlisle, Chemawa, Chilocco, Hampton or Haskell, being literate, and speaking English.[36] Labour market and economic outcomes include indicators for: being in the labour force, being employed, and home-ownership (a proxy for wealth). As data on wage income were not collected until the 1940 census, I use a proxy for income derived from occupation in 1910.[37] Cultural outcomes include indicators for: having a White spouse, giving one's children a western first name, and being counted as 'White' in a later (1920 or 1940) census.[38] Intermarriage and naming practices are standard measures of assimilation in the Economic History literature (e.g., Fouka 2019; Abramitzky, Boustan, and Erikkson 2020). Changes in racial classification (i.e., from nonwhite races to 'White') have not, to the best of my knowledge, been used as a measure of cultural assimilation in this literature. However, in my context, this outcome is arguably a good proxy for unobservable dimensions of cultural assimilation (e.g., accent and dress). More details on the outcomes used in my analysis are available in section B.2 of the Appendix.

[36] The IPUMS variable LIT treats individuals that were literate in a language other than English as being literate. Since I am interested in English literacy, I code individuals that could read or write, but could not speak English, as not literate.

[37] Specifically, I use the log of the IPUMS variable OCCSCORE, which is a standard pre-1940 proxy of wage income.

[38] In the case of naming practices, I restrict attention to the eldest male child in the household born *after* the reservation was treated by an off-reservation school (consistent with Fouka 2019).

3.3.2. Samples

I consider two main samples in my analysis. The first sample is used to measure 'first generation' outcomes (i.e., outcomes of individuals that grew up during the roll-out of off-reservation schools). These outcomes are measured in 1910 or 1920. The second sample is used to measure the adult outcomes of children from first generation households (i.e., 'second generation' outcomes). I describe the restrictions applied to each sample below.

First generation. The first generation sample consists of Native American, male, household heads in non-group quarters that were born between 1845 and 1892 (i.e., aged between 19 and 65 in 1910). Outcomes are observed primarily in 1910 (the single exception is racial classification in the 1920 census). I restrict attention to household heads in non-group quarters because some outcomes are only relevant for households (e.g., intermarriage, naming of children, and home ownership). I focus on males between the age of 19 and 65 (i.e., roughly working age) because I am interested in measuring labour market outcomes. I also make several restrictions at the reservation-level. I restrict attention to ever-treated reservations, since I cannot rule out the possibility that not-treated reservations *were* treated, but not mentioned in my data sources. I also exclude individuals that were matched to reservations under the jurisdiction of the Union agency, since Native Americans on these reservations were not targeted by off-reservation schools, and were significantly more assimilated than the Native American population as a whole.[39] Finally, to ensure that my results are not driven by compositional change, I restrict attention to reservations that have full cohort representation between the years of 1850 and 1890. This amounts to dropping small reservations, which is standard in the literature (e.g., Gregg 2018). Table 1 shows descriptive statistics for the baseline (1910) first generation sample, as well as the subsample that can be linked to the 1920 census.

Second generation. The second generation sample consists of male children that were living in first generation households in 1910, that could be linked to the 1940 census using the Census Tree. I restrict attention to linked individuals that were themselves household heads in 1940. Table 2 shows descriptive statistics.

4. Empirics

4.1. Identification

My goal is to estimate the effects of exposure to off-reservation schools during the late-19th century on educational, socioeconomic and assimilation outcomes in 1910 and 1920 (first

[39] The five reservations were occupied by the Cherokee, Chickasaw, Choctaw, Creek, and Seminole in what is now Oklahoma.

TABLE 1. Descriptive statistics, first generation samples

	Year	Mean	SD	N	Reservations
First gen., 1910					
In attendance data	1910	0.017	0.130	9,562	62
Literate	1910	0.326	0.469	10,533	69
Speaks English	1910	0.629	0.483	10,622	69
In lab. force	1910	0.854	0.354	10,622	69
Occ. inc. score	1910	2.745	0.344	8,119	69
White spouse	1910	0.015	0.121	9,367	69
Child has western name	1910	0.528	0.499	5,180	47
Year of birth	1910	1868.162	7.820	10,622	69
First gen., 1920					
Counted as 'White'	1920	0.144	0.351	2,805	38

The first generation sample in 1910 consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The first generation sample in 1920 consists of individuals from the 1910 sample that were linked to the 1920 census using the Census Tree. The number of reservations for the outcome 'In attendance data, 1878 - 1900' is lower because the sample only includes reservations treated by one of Carlisle, Chemawa, Hampton or Haskell. The sample is restricted to reservations with full cohort representation from 10 years before to 10 years after treatment.

TABLE 2. Descriptive statistics, second generation sample

	Year	Mean	SD	N	Reservations
Finished primary school	1940	0.421	0.494	1,530	20
Finished high school	1940	0.077	0.267	1,530	20
In lab. force	1940	0.932	0.252	1,551	20
Occ. inc. score	1940	2.895	0.403	1,473	20
(log) Wage income	1940	4.345	2.825	1,454	20
Owns home	1940	0.651	0.477	1,551	20
House value	1940	5.363	1.392	1,000	20
White spouse	1940	0.329	0.470	1,551	20
Counted as 'White'	1940	0.362	0.481	1,411	20
Year of birth	1940	1900.161	6.312	1,551	20

The second generation sample in 1940 consist of male Native American children from households in the first generation sample born in the same year or after their reservation was first treated by an off-reservation school. Please see Table 1 for a description of the first generation sample. The sample is restricted to reservations with full cohort representation from 12 years before to 12 years after treatment.

generation), and in 1940 (second generation).

Simple comparisons of treated versus non-treated reservations, or younger versus older

cohorts, would be problematic in the (likely) presence of unobservable reservation characteristics or cohort trends that are correlated with the outcomes of interest. In order to estimate the causal effects of exposure to off-reservation schools, I adopt a cohort-based event study design. This design exploits two sources of variation: firstly, that reservations were exposed to off-reservation schools at different points in time (Figure 3), and secondly, that individuals already past schooling age when an off-reservation school first came to their reservation were less likely to be recruited.

I first identify the cohorts that, on the basis of their age, were least likely to be recruited when an off-reservation school first came to their reservation. I assign individuals that were over the age of 20 to this 'not treated' group. This is based on communications from the Office of Indian Affairs (e.g., Office of Indian Affairs 1902), and observed ages at enrollment in attendance records for Carlisle, Chemawa, and Haskell.[40] Figure 4 shows that there was a sharp drop in the number of admitted students aged over 18, and very few admits over the age of 20. Indeed, most students over the age of 20 were admitted in the early years of the off-reservation system, when age guidelines had not yet been formulated.

FIGURE 4. Ages of admitted students

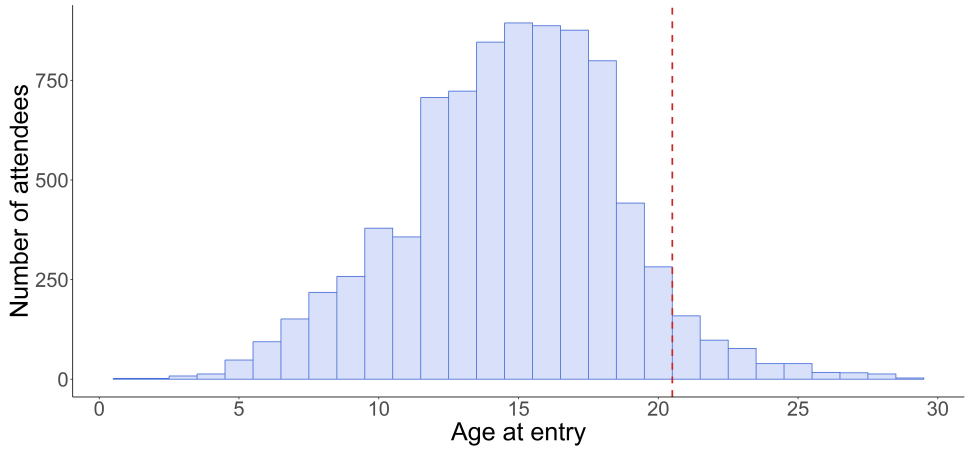


Figure shows the ages of students when admitted into one of Carlisle, Chemawa, or Haskell.
Source: Own calculations using attendance records for Carlisle, Chemawa, and Haskell.

4.2. Specifications

Firstly, I present my specification for the first generation:

$$y_{r,c} = \sum_{j;\, j \neq 22} \alpha_j \, age_at_exposure_{j(r,c)} + \alpha_r + \alpha_c + X'_{a(r),c}\gamma + \varepsilon_{r,c} \tag{1}$$

[40] Age at entry is not reported for attendees at Hampton.

Here, $y_{r,c}$ is the mean outcome (measured in 1910 or 1920) of an individual from reservation r of birth cohort c. $age_at_exposure_{j(r,c)}$ are event time indicators for the age of cohort c from reservation r when an off-reservation school first recruited from the reservation. In order to increase precision, I group these indicators into 2-year bins (e.g. $j = 22$ captures individuals that were 21 or 22 when an off-reservation school first recruited from their reservation). In line with the discussion in the previous section, I set $j = 22$ as the reference group. As specifications that include a full set of event time indicators (such as equation 1) require an additional normalisation (Borusyak, Jaravel, and Spiess 2023), I also omit $j = 30$ (the last event time indicator in my sample).

Since $age_at_exposure_{j(r,c)}$ varies at the reservation-by-cohort level, I am able to include fixed effects for reservation (α_r) and cohort (α_c), with the latter grouped into two-year bins. I also include pre-treatment agency-level literacy shares interacted with cohort effects ($X_{a(r),c}$), as this characteristic is correlated with the timing of treatment (the determinants of off-reservation school treatment years are discussed in the next section). I cluster standard errors at the reservation-level (i.e., the level of treatment).

The coefficients of interest are the α_j. These provide intent-to-treat estimates; I am not estimating the effect of actually attending an off-reservation school (since I do not observe attendance for the majority of schools), but instead inferring exposure on the basis of an individual's reservation and cohort. An advantage of this approach (apart from being the only feasible approach with my data) is that intent-to-treat estimates capture both the effect of attendance, as well as community-level effects on non-attendees (i.e., spillovers). I show in section 5 that my measure of exposure strongly predicts individual-level attendance in the sample of schools for which I *do* have attendance data.

The specification for the second generation takes a similar form to equation 1:

$$(2) \qquad y_{r,c,c'} = \sum_{j;\, j \neq 24} \alpha'_j father_age_at_exposure_{j(r,c)} + \alpha_r + \alpha_c + \alpha c' + X'_{a(r),c}\gamma + \varepsilon_{r,c,c'}$$

Here, $y_{r,c,c'}$ is the mean outcome (measured in 1940) of children belonging to birth cohort c', with fathers from reservation r and birth cohort c. $father_age_at_exposure_{j(r,c)}$ is analogous to $age_at_exposure_{j(r,c)}$, but defined with respect to father's reservation and cohort. As the 1940 sample is substantially smaller I group event time indicators into 3-year bins. Therefore, I set $j = 24$ as the reference group, and again drop the last event time indicator in my sample ($j = 33$) for the additional normalisation. In addition to fixed effects for father's reservation (α_r) and father's cohort (α_c), I also control for child's cohort ($\alpha_{c'}$). Both α_c and $\alpha_{c'}$ are grouped into 3-year bins. I continue to control for agency-level literacy shares interacted with (father's) cohort effects ($X_{a(r),c}$), and cluster standard errors at the

father's reservation-level.

4.3. Determinants of off-reservation school treatment years

As my identification strategy exploits staggered exposure to off-reservation schools, it is natural to ask what determined the timing of exposure. To the best of my knowledge, pre-treatment data on reservation characteristics are not available. However, I have digitised *agency-level* characteristics from statistical tables contained in Annual Reports of the Commissioner of Indian Affairs for the years 1877 to 1879. I examine variation at the *agency-level* in treatment years with respect to 6 characteristics: (log) population, the share of Native Americans in 'citizens dress', the share of Native Americans able to read, and indicators for the presence of a day school, reservation boarding school, and 'church buildings'. These characteristics are intended to measure baseline levels of assimilation and development on reservations; (log) population is included to address the possibility that off-reservation schools targeted larger reservations (with a larger pool of potential students). I regress agency treatment years on each characteristic separately, controlling for US regions.[41] Figure 5 shows the results. The timing of off-reservation school exposure is uncorrelated with most characteristics at conventional levels. However, there is evidence that communities with higher rates of literacy at baseline were targeted later during the roll-out. For this reason, I include interactions of agency literacy shares with cohort fixed effects in my main specifications (equation 1 and equation 2).[42]

4.4. Threats to identification

There are three main threats to identification. Firstly, my empirical strategy relies on the 'parallel trends' assumption. In my context, this means that outcomes on treated and yet-to-be-treated reservations would have evolved in a similar way in the absence of treatment. Since recruitment decisions are unlikely to have been random, a possible threat is that off-reservation schools targeted reservations where educational, socioeconomic and assimilation outcomes were already on an 'upwards' trajectory. I cannot categorically rule out this possibility, but I am able to provide evidence against it. In the results that follow, I document the absence of pre-trends in both first generation and second generation outcomes. As additional checks, I conduct diagnostics proposed by Roth (2022), and also assess the robustness of my main results to deviations from the parallel trends assumption (Rambachan and Roth 2023).

[41] Regions are based on 'Divisons' from the IPUMS variable REGION. These are: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain and Pacific.

[42] I group agency literacy shares into 10 bins when running regressions on outcomes measured in 1910. In linked samples, I group agency literacy shares into 4 bins, due to significantly smaller sample sizes.

FIGURE 5. Determinants of off-reservation school treatment years

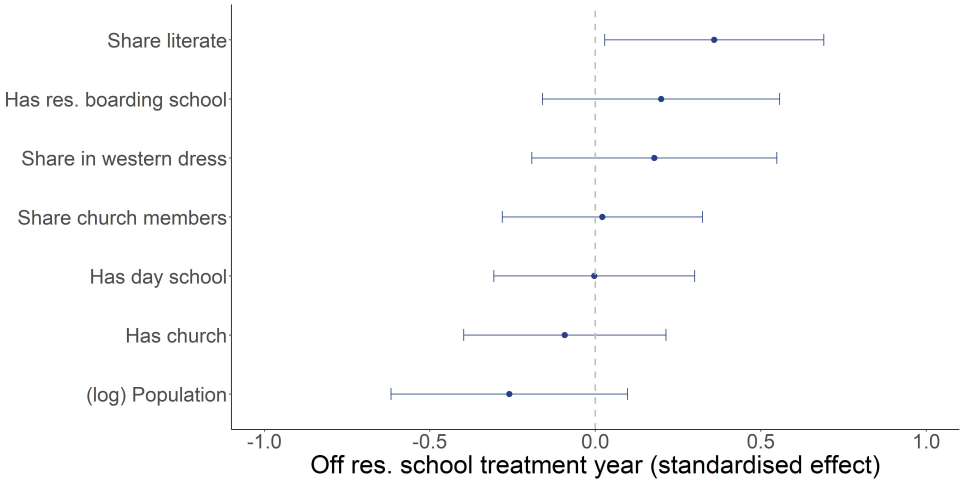


Figure shows OLS estimates and 95 per cent confidence intervals from regressions of off-reservation school treatment years on each characteristic, controlling for (modern) US division. Off-reservation school treatment years and agency characteristics are at the agency-level.
Source: Own calculations using data from Annual Reports to the Commissioner of Indian Affairs, 1877 - 1879.

A second threat to identification is the occurrence of other policies at the same time as the roll-out of the off-reservation system. The only other major policy targeting Native Americans during this period was the 'allotment' of reservations. This policy began in 1888, and sought to promote individual property rights by dividing and assigning reservation lands to individual Native Americans. It is unlikely that allotment had a meaningful effect on educational, socioeconomic and assimilation outcomes during the period of my study, since allotted lands were held in trust (i.e., could not be transferred) until the early-1900s (Dippel, Frye, and Leonard 2023). Nevertheless, I address this concern in two ways. Firstly, I digitise data on the dates of major allotments from a 1935 report by the Indian Office, and show that the timing of allotments was uncorrelated with the timing of off-reservation school treatment years. Secondly, I show that my main results hold when restricting the sample to reservations that were never allotted.

A third threat to identification relates to biases inherent in two-way fixed effects models. In 'dynamic' two-way fixed effects specifications such as equation 1 and equation 2, the estimated effects may be biased in the presence of treatment effect heterogeneity (Sun and Abraham 2021). In my context, this would occur if cohorts on different reservations experienced a different path of treatment effects. To address this concern, I present specifications that use the robust TWFE estimator proposed by Sun and Abraham (2021).

5. Results

In this section, I set out my results. I first show that my measure of exposure to off-reservation schools predicts attendance in the subset of schools for which I have attendance data. I then proceed to my main results, documenting that exposure to off-reservation schools led to cultural assimilation in the first generation, but that these effects reversed in the second generation.

5.1. Exposure strongly predicts attendance

I first restrict my sample to reservations that were treated by one of Carlisle, Chemawa, Hampton and Haskell (the schools for which I have attendance data) between 1879 and 1900, and estimate equation 1 using attendance as the outcome.[43] This can be thought of as a 'first stage'. Results are shown in panel (a) of Figure 6. The estimated effects lend support to my empirical strategy. There is no effect on attendance for individuals that were 21 and older when one of Carlisle, Chemawa, Hampton or Haskell first started recruiting from their reservation. However, the probability of attendance begins to increase for cohorts aged under 20, and stabilises at 5 percentage points for individuals aged 14 and younger. In

[43] Entry dates in attendance data for Chilocco are inconsistent with enrollment figures from school reports, so I do not use Chilocco in this exercise.

order to summarise the magnitude of this effect, column (1) of Table 3 presents a sample-weighted average of event time effects for $j >= 20$ ('Average 'post' effect'). This estimate indicates that individuals of schooling age when an off-reservation school first came to their reservation were around 3 percentage points more likely to attend (significant at the 5 per cent level).

5.2. Exposure led to cultural assimilation in the first generation

I now examine educational, cultural and labour market outcomes using the full first generation sample. Results for educational and cultural outcomes are shown in Panels (b) to (f) of Figure 6. Reassuringly, there are no visible pre-trends in any of these outcomes. Panel (b) shows that exposure to off-reservation schools did not lead to statistically significant effects on literacy for older treated cohorts, but there are clear effects on younger cohorts of around 20 percentage points. Panel (c) shows that exposure led to an increase in the probability of speaking English, with the effect reaching over 15 percentage points for the youngest cohorts. Moving to measures of cultural assimilation, I find that exposure to off-reservation schools increased the probability of intermarriage (i.e., having a White spouse), with the effects following a similar profile to educational outcomes (panel (d)). Panel (e) shows that children (specifically, the first male child born after the reservation was treated) were more likely to be given a 'western' name, as proxied by saint and biblical names. As naming practices are a strong signal of cultural identity (e.g., Abramitzky, Boustan, and Erikkson 2020), this result is indicative of cultural assimilation among treated cohorts.[44] Finally, panel (f) shows that treated cohorts were more likely to be enumerated as 'White' in the 1920 census. Being perceived as white is likely to have reflected a variety of assimilation-relevant factors (e.g., accent and dress), and therefore provides strong evidence that off-reservation schools led to cultural assimilation in the first generation.[45] Table C.2 in the Appendix shows that switching to 'White' was also present among 'non-movers' (i.e., individuals living in the same state / county in 1910 and 1920). This suggests that changes in racial classification were at least partially due to changes in how Native Americans were perceived *within* locations.

Table 3 summarises the magnitudes of the effects by presenting sample-weighted averages of event time effects. These estimates indicate that the effects of off-reservation schools on first generation educational and cultural outcomes were not only statistically significant,

[44] Since individuals in my first generation sample were aged between 19 and 65, the youngest cohorts may have been too young to have children in 1910. The median father's age at the birth of their first child in my full dataset (i.e., all Native Americans matched to reservations) is 28. Around 85 per cent of individuals in my first generation sample were 28 or older in 1910.

[45] As discussed in section 3.3.1, intermarriage and naming practices are standard measures of cultural assimilation in the Economic History literature. Changes in racial classification have not been used for this purpose, given that prior work has focused on the assimilation of (white) European migrants.

but also large in magnitude. The effects on English proficiency (column (3)), intermarriage (column (4)), naming practices (column (5)) and racial classification in 1920 (column (6)) represent changes of between 16 to 200 per cent over the sample mean of these outcomes.

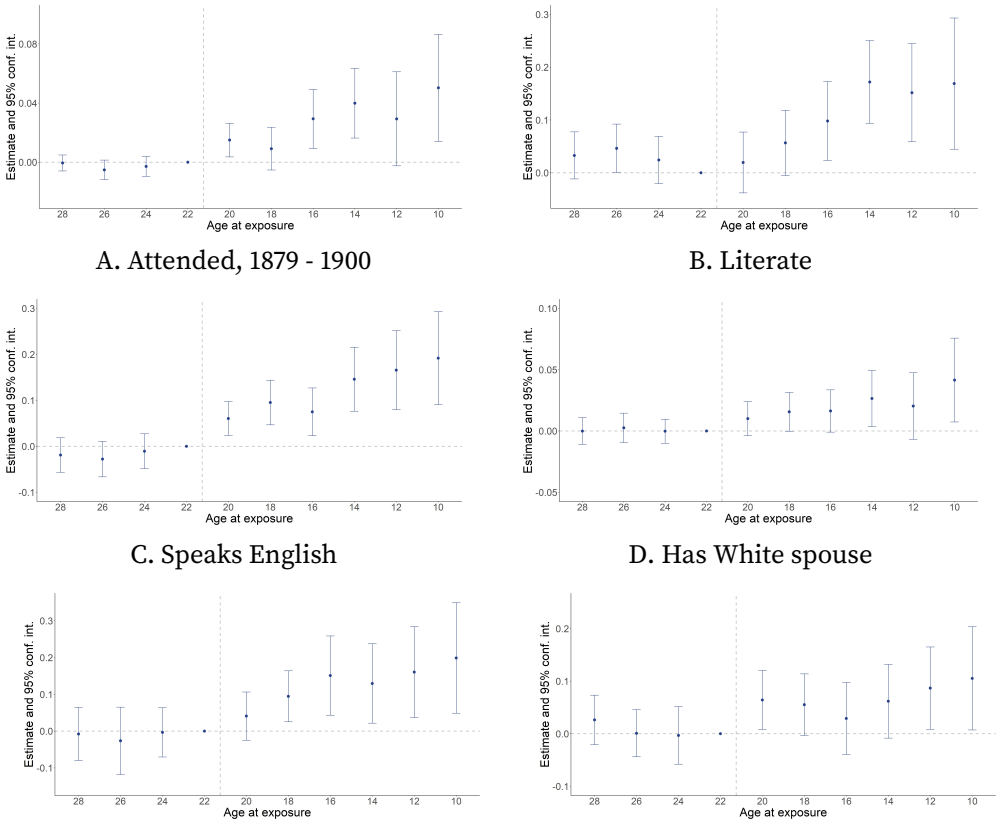
TABLE 3. First generation, educational and cultural outcomes

	Attended	Literate	Speaks English	White spouse	Child has western name	'White' in 1920
	(1)	(2)	(3)	(4)	(5)	(6)
Average effect	0.030	0.111	0.122	0.022	0.123	0.093
	(0.010)	(0.037)	(0.029)	(0.010)	(0.042)	(0.034)
	[0.005]	[0.004]	[0.000]	[0.053]	[0.081]	[0.006]
Mean dep. var	0.017	0.326	0.629	0.015	0.528	0.144
R2	0.088	0.301	0.353	0.129	0.259	0.115
No. reservations	62	69	69	69	47	38
No. cohorts	21	21	21	21	20	21
Obs.	9,562	10,533	10,622	9,367	5,180	2,805

Table shows estimates from Equation 1 in the first generation sample. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The sample in Column (1) is restricted to individuals from reservations that were treated by Carlisle, Chemawa, Hampton, and Haskell. The sample in Column (5) is restricted to individuals from the first generation sample that were linked to the 1920 census using the Census Tree. Columns (2) to (4) are estimated on the full first generation sample. 'Attended, 1879 - 1900' is an indicator for appearing in the attendance records of Carlisle, Chemawa, Hampton or Haskell, and being linked to the 1910 census. 'Literate' is an indicator for being able to read and write (measured in 1910), based on the IPUMS variable LIT. 'Speaks English' is an indicator for being able to speak English (measured in 1910), based on the IPUMS variable SPEAKENG. 'White spouse' is an indicator for having a White spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Child has western name' is an indicator equal to 1 if the eldest male child in the household's first name appears in the list of saint names and biblical names from Abramitzky, Boustan, and Eriksson (2016) (measured in 1910). 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

In Figure 7, I examine the effects of exposure on measures of economic integration and wealth. I find that off-reservation schools generated limited economic benefits. Treated cohorts were not more likely to be in the labour force (panel (a)), nor more likely to be employed (panel (b)), and they did not have higher-paying occupations (panel (c)). There is also no evidence that exposure to off-reservation schools led to higher wealth, as proxied

FIGURE 6. First generation, educational and cultural outcomes



A. Attended, 1879 - 1900

B. Literate

C. Speaks English

D. Has White spouse

E. Child has 'western' name

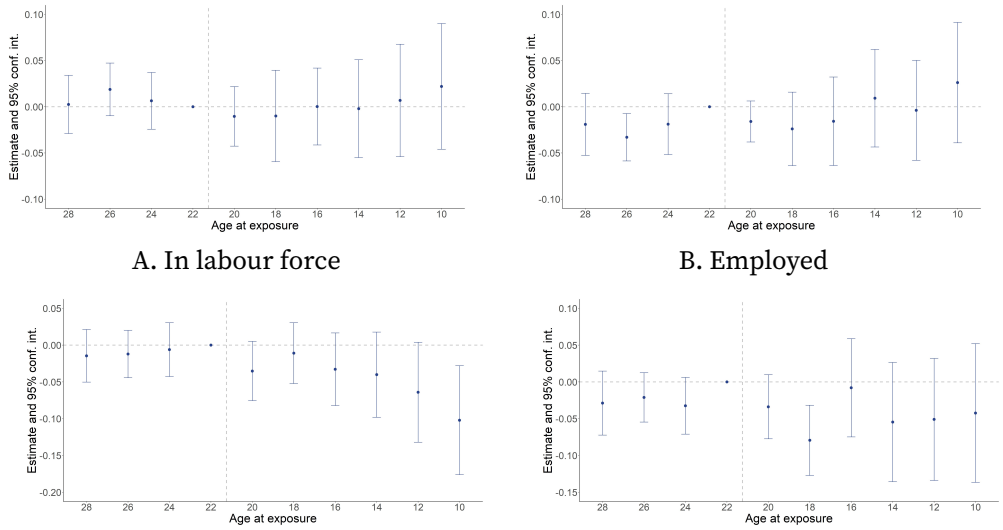
F. Counted as 'White' in 1920

Figure shows estimates from equation 1 in the first generation sample. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The sample in panel (a) is restricted to individuals from reservations that were treated by Carlisle, Chemawa, Hampton, and Haskell. The sample in panel (e) is restricted to first generation households with at least one male child in the household in 1910. The sample in panel (f) is restricted to individuals from the first generation sample that were linked to the 1920 census using the Census Tree. All other regressions are estimated on the full first generation sample. 'Attended, 1879 - 1900' is an indicator for appearing in the attendance records of Carlisle, Chemawa, Hampton or Haskell, and being linked to the 1910 census. 'Literate' is an indicator for being able to read and write (measured in 1910), based on the IPUMS variable LIT. 'Speaks English' is an indicator for being able to speak English (measured in 1910), based on the IPUMS variable SPEAKENG. 'Has White spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Child has western name' is an indicator equal to 1 if the eldest male child in the household's first name appears in the list of saint names and biblical names from Abramitzky, Boustan, and Eriksson (2016) (measured in 1910). 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

by home ownership (panel (d)).

Table 4 again summarises the magnitudes of event time effects with sample-weighted averages. Based on these estimates, exposure to off-reservation schools did not have statistically significant effects on labour force participation, employment, or home ownership; if anything, exposure led to a movement into *lower*-paying occupations.

FIGURE 7. First generation, labour market and economic outcomes



A. In labour force B. Employed

C. Occupational income score D. Owns home

Figure shows estimates from equation 1 in the first generation sample. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1910. 'In labour force' is an indicator for being in the labour force in 1910, based on the IPUMS variable LABFORCE. 'Employed' is an indicator for being employed, conditional on being in the labour force, based on the IPUMS variable 'EMPSTAT'. An individual was considered to be employed if they were at work on 15 April 1910. 'Occupational income score' is the log of the IPUMS variable OCCSCORE. 'Owns home' is an indicator for owning one's own home (rather than renting), based on the IPUMS variable OWNERSHP. All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Standard errors are clustered at the reservation-level.

To summarise, there is no evidence that exposure to off-reservation schools led to direct economic benefits for the first generation. However, the schools did generate substantial cultural assimilation.[46] In the next section, I investigate the extent to which these cultural effects persisted across generations.

[46]I do not find similar effects on educational or cultural outcomes when looking at the sample of first generation women (Table C.4 in the Appendix). One reason for this is that, during the roll-out of the off-reservation school system, female students were less intensely recruited than male students. To illustrate this point, my attendance data show that between 1879 and 1885 around 75 per cent of admitted students were male. This imbalance improved over the next 15 years, such that by 1900 around 45 per cent of students in off-reservation schools were female.

TABLE 4. First generation, labour market and economic outcomes

	In labour force	Employed	Occ. income score	Owns home
	(1)	(2)	(3)	(4)
Average effect	0.001	-0.004	-0.049	-0.044
	(0.023)	(0.022)	(0.025)	(0.030)
	[0.957]	[0.790]	[0.111]	[0.521]
Mean dep. var	0.854	0.926	2.745	0.817
R2	0.265	0.172	0.204	0.177
No. reservations	69	69	69	69
No. cohorts	21	21	21	21
Obs.	10,622	8,009	8,119	10,622

Table shows estimates from Equation 1 in the first generation sample. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1910. 'In lab. force' is an indicator for being in the labour force in 1910, based on the IPUMS variable LABFORCE. 'Employed' is an indicator for being employed, conditional on being in the labour force, based on the IPUMS variable 'EMPSTAT'. An individual was considered to be employed if they were at work on 15 April 1910. 'Occ income score' is the log of the IPUMS variable OCCSCORE. 'Owns home' is an indicator for owning one's own home (rather than renting), based on the IPUMS variable OWNERSHP. All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

5.3. Effects of exposure reversed in the second generation

I now examine the extent to which the effects of off-reservation schools on the first generation were transmitted to their *adult* children. This sample consists of male children from first generation households that could be linked to the 1940 census using the Census Tree. I focus on outcomes measured in 1940, since by this time the majority of children from 1910 had reached adulthood and formed their own households. As noted in section 3, I combine event time indicators into 3-year bins due to smaller sample sizes.

Results are shown in Figure 8. As in the first generation, there are no obvious pre-trends in any of the outcomes. Adult children from treated first generation households appear to have been no better educated than those from non-treated households (panel (a)). In fact, if anything, treated cohorts were less economically integrated. They were less likely to be in the labour force (panel (b)), did not have higher wages (panel (c)), and were less likely to be living in an urban area in 1940 (panel (d)). Moving to cultural outcomes, panel (e) and panel (f) show a striking reversal in the effects observed in the first generation. In

contrast to their fathers, adult children from treated first generation households were less likely to have a White spouse, and less likely to be counted as 'White' in the 1940 census (or equivalently, more likely to have a Native American spouse, and more likely to be counted as 'Indian').

As before, Table 5 summarises the magnitudes of these effects with sample-weighted averages. The effects on labour force participation (column (2)) are statistically significant at the 5 per cent level, though fairly small in magnitude relative to the mean rate. Column (4) to column (6) of Table 5 suggest that reduced geographical and cultural integration accompanied, and may have been responsible for, the labour market disadvantages of the second generation. The negative estimates on living in an urban area (column (4)), having a White spouse (column (5)), and being counted as 'White' in 1940 (column (6)), are all statistically significant and large in magnitude (representing changes of 66 to 100 per cent over their means).

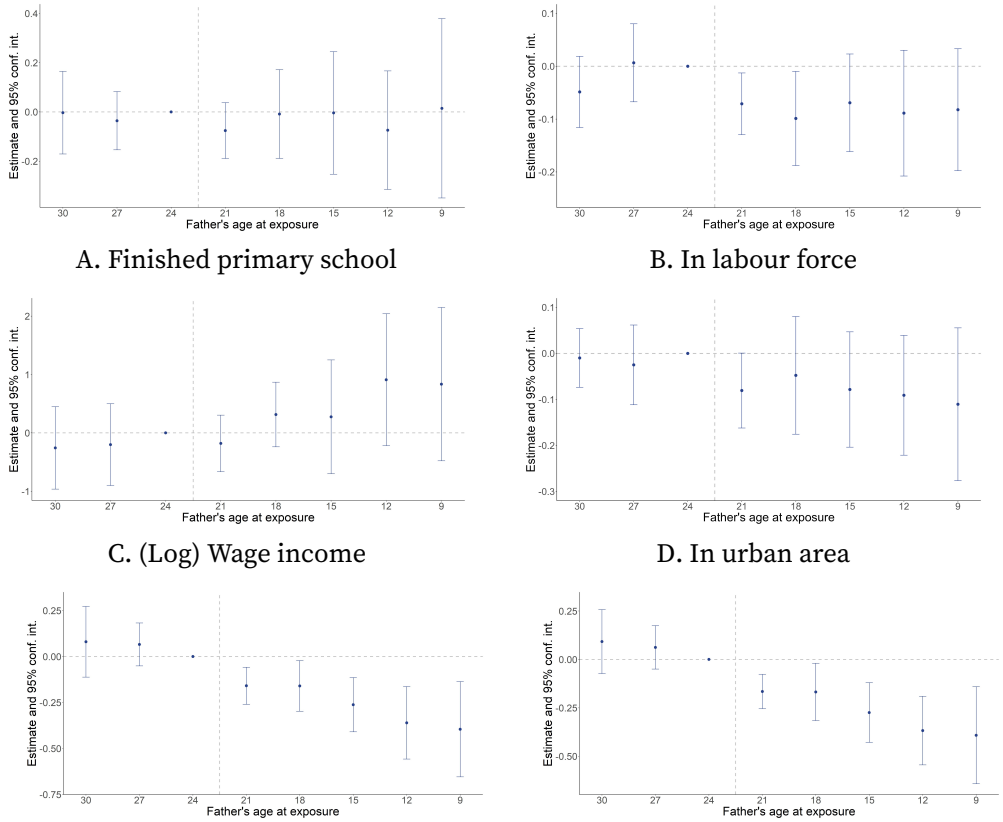
To summarise, I find that adult children from treated first generation households were less culturally assimilated (or equivalently, more identifiably 'Indian'), in contrast to their fathers.[47] In section 6, I show that resistance on the part of indigenous communities is likely to have played a role in this reversal.

5.4. Robustness

Robust TWFE estimation. The Econometrics literature has highlighted potential biases in two-way fixed effects models in settings with staggered treatment adoption. With respect to event study models such as equation 1 and equation 2, the main concern is that coefficient estimates are based on comparisons between not-yet-treated and already-treated units. In the presence of treatment effect heterogeneity, the estimated coefficients of a given lead or lag may be contaminated by the effects from other relative periods (Sun and Abraham 2021). To address this concern, I assess the robustness of my results to the estimation strategy proposed by Sun and Abraham (2021). Figure D.1 (first generation) and Figure D.2 (second generation) presents these results. In almost all cases, the pattern, magnitude, and statistical significance of estimates using the Sun and Abraham (2021) estimator are similar to those using the standard TWFE model. The single exception is the probability of living in an urban area in 1940 for the second generation, which is imprecisely estimated.

[47] One concern is that my second generation sample does not include children that were themselves in off-reservation schools or reservation boarding schools when the 1910 census was taken. The extent of this problem is likely to be limited, since individuals in my second generation sample were, on average, around 10 years of age in 1910 (around 4 to 5 years younger than the average off-reservation school recruitment age in my attendance data). Nevertheless, I show in Table C.1 that my results with respect to intermarriage and racial classification are present when restricting the sample to individuals aged under 14 in 1910.

FIGURE 8. Second generation outcomes



A. Finished primary school

B. In labour force

C. (Log) Wage income

D. In urban area

E. Has White spouse

F. Counted as 'White' in 1940

Figure shows estimates from equation 2 in the second generation sample. The second generation sample consists of male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Finished primary school' is an indicator for having completed primary school, based on the IPUMS variable EDUCD. '(Log) Wage income' is the log of wage income, based on the IPUMS variable INCWAGE. 'In urban area' is an indicator for residing in an urban area in 1940, based on the IPUMS variable URBAN. 'Has White spouse' is an indicator for having a white spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White' is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. All regressions include reservation fixed effects, cohort fixed effects, household head cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with household head cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

TABLE 5. Second generation outcomes

	Finished primary school	In labour force	Wage income	In urban area	White spouse	'White' in 1940
	(1)	(2)	(3)	(4)	(5)	(6)
Average effect	-0.023	-0.078	0.315	-0.102	-0.271	-0.298
	(0.104)	(0.041)	(0.375)	(0.060)	(0.094)	(0.101)
	[0.611]	[0.015]	[0.970]	[0.162]	[0.031]	[0.021]
Mean dep. var	0.451	0.934	4.463	0.146	0.383	0.343
R2	0.158	0.085	0.139	0.127	0.238	0.229
No. reservations	20	20	20	20	20	20
No. cohorts	14	14	14	14	14	14
Obs.	1,530	1,551	1,454	1,551	1,411	1,551

Table shows estimates from Equation 2 in the second generation sample. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Finished primary school' is an indicator for having completed primary school, based on the IPUMS variable EDUCD. '(Log) Wage income' is the log of wage income, based on the IPUMS variable INCWAGE. 'In urban area' is an indicator for residing in an urban area in 1940, based on the IPUMS variable URBAN. 'White spouse' is an indicator for having a White spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White' is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. All regressions include reservation fixed effects, cohort fixed effects, father's cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with father's cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

Pre-trends. Figure 6 and Figure 8 provide graphical evidence in favour of the parallel trends assumption. However, recent work has highlighted pitfalls of informal pre-trend assessments of this kind. Roth (2022) raises two concerns: firstly, that researchers may fail to detect pre-trends due to low power, and secondly, that conditioning on the absence of pre-trends can lead to additional bias. In section D.2, I conduct the diagnostics proposed by Roth (2022). In addition, I document the robustness of my main results to deviations from the parallel trends assumption using the methodology developed in Rambachan and Roth (2023).

Selection into the linked 1940 sample. In my main results, I find that exposure to off-reservation schools led to assimilation in the first generation, but that these effects re-

versed in the second generation. One concern is that these results may be driven by selection into the linked 1940 sample. For example, linking algorithms are more likely to link individuals with unique names, which may be correlated with their propensity to adopt identifiably 'Indian' traits. To address this concern, I show in Table D.2 that assimilation effects are still present in *first* generation households from which the second generation sample is drawn. This allays concerns that the reversal is due to the second generation sample being drawn from an 'atypical' set of first generation households.

Linking procedures. When linking individuals across census years, I rely on all links in the Census Tree. Since the Census Tree combines links from a range of sources (e.g., automated approaches, machine learning methods, and user-generated links), I examine whether my results are driven by a particular linking procedure. I restrict attention to three methods: the commonly-used ABE NYSIIS algorithm (Abramitzky, Boustan, and Eriksson 2014), user-generated links from FamilySearch.org (the Family Tree), and links from FamilySearch.org's proprietary algorithm ('Hints').[48] Sample sizes are considerably smaller in all cases, but regardless of the method used, the estimated coefficients follow a similar pattern to those in my main results.

Accounting for secular trends. Another concern is that the event study may be picking up regional changes in educational and cultural outcomes that are correlated with the timing of reservation-level exposure to off-reservation schools. To account for this possibility, I re-estimate equation 1 and equation 2 including region-by-cohort fixed effects.[49] Figure D.5 (first generation) and Figure D.6 (second generation) presents these results. Both first generation estimates and second generation estimates are largely unaffected in magnitude and statistical significance by the inclusion of time-varying regional controls.

Allotment of reservations. In an effort to promote private ownership among Native Americans, as well as opening reservations to White settlement, the US government began allotting parcels of reservation lands to individual Native Americans from 1887 onwards (Dippel, Frye, and Leonard 2023). This policy is unlikely to have affected my outcomes of interest, as allottees only obtained fully-transferable ownership rights from 1906 onwards. Nevertheless, the allotment process is a potential threat to identification. I address this

[48] ABE NYSIIS matches individuals across two datasets on the basis of state of birth, year of birth (allowing for a discrepancy of up to ±2 years), and New York State Identification and Intelligence System (NYSIIS) standardised first name and last name. Observations that cannot be *uniquely* matched across datasets are dropped from the sample, and the algorithm is only able to match men across datasets.

[49] Regions are based on divisions defined by the IPUMS variable REGION. There are 9 divisions: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific. For more information, please see: `https://usa.ipums.org/usa-action/variables/REGION`.

concern in two ways. Firstly, I digitise information on 'major allotments' of reservations from a 1935 report by the Indian Office (Office of Indian Affairs 1935). Figure D.7 shows binned scatterplots of off-reservation school treatment years and (first) major allotment years in the first generation samples (1910 and 1920) and second generation sample (1940). In all cases, the linear fit lines indicate a weak and non-statistically significant relationship between off-reservation school treatment years and allotment years. Secondly, I re-estimate equation 1 and equation 2 on a restricted sample of reservations that had not been allotted by the time outcomes were measured. The results in Table D.6 (first generation, educational and cultural outcomes), Table D.7 (first generation, labour market outcomes), and Table D.8 (second generation outcomes) show that my main conclusions continue to hold, though estimates in the second generation sample are imprecise.

Alternative sample windows. My main results restrict the sample to a relatively small window around reservation treatment years (10 years before / after in 1910 and 1920, and -15 years before / after in 1940). I make these restrictions in order to maximise the number of reservations in my sample, while also maintaining balance on the cohort dimension. While a relatively common practice, 'trimming' the sample in this way can generate biased estimates (Borusyak, Jaravel, and Spiess 2023). To address this concern, I re-estimate equation 1 and equation 2 including more distant leads and lags, while still maintaining a balanced sample. Figure D.8 (first generation) and Figure D.9 (second generation) shows results. While less precisely estimated due to substantially smaller sample sizes, I continue to find no evidence of pre-trends, and the patterns of coefficient estimates are generally very similar to those from my main results.

Native Americans counted as 'White' in 1910. As discussed in section 2, both the 1900 and 1910 censuses used the same 'Indian schedules'. Therefore, it is reasonable to expect that relatively few individuals switched from 'Indian' in 1900 to 'White' in 1910. However, individuals that *did* switch would not be included in my main estimates, since my baseline sample consists of all Native Americans in the 1910 census. If anything, the exclusion of Native Americans that were already counted as 'White' in 1910 would bias first generation estimates to zero. However, the exclusion of these individuals may affect my estimates with respect to second generation outcomes. To address this concern, I use the Census Tree to link Native Americans from the 1900 census to the 1910 census, define an analogous first generation sample, and examine the share that were classified as 'White' in 1910.[50] [51] I find that a negligible share (around 1 per cent) switched from 'Indian' in 1900 to 'White'

[50] Unfortunately, very few Native Americans are linked between these years, since the majority of Native Americans are missing names in the 1910 census. To illustrate, the Census Tree contains 93,000 links between 1910 and 1920, but only 18,000 between 1900 and 1910.

[51] In line with my main analysis, this first generation sample consists of male household heads from non-group quarters between the ages of 19 and 65 in 1910, that were not matched to the Union agency.

in 1910. The corresponding figure between 1910 and 1920 is 14 per cent. This suggests that sample selection due to changes in racial classification prior to 1910 is unlikely to affect my main conclusions.

6. Mechanisms

My results in section 5 show that exposure to off-reservation schools led to assimilation in the first generation, followed by a decline (or reversal) in cultural and economic integration in the second generation. In this section, I examine the drivers behind these results. Firstly, I provide evidence that the decline in social and economic integration in the second generation can be at least partially explained by resistance on the part of Native American communities. Secondly, I show that the decline was driven by second generation cohorts from less assimilated first generation households. Thirdly, in line with the previous finding, I show that increased discrimination against Native Americans in the second generation is unlikely to explain my results.

6.1. Community-level resistance to off-reservation schools

Community-level resistance to off-reservation schools is well-documented in the historical literature. On the one hand, concerns about educational content, as well as the health of their children, led parents, and sometimes entire communities, to refuse to send their children to schools off the reservation (Adams 2020, pp. 232 - 237). On the other hand, communities also made efforts to re-integrate students returning from off-reservation schools into the tribe; as noted in section 2 re-integration was sometimes enforced by way of economic sanctions or punishment (Office of Indian Affairs 1887, p. 168).

If community-level resistance played a role in the decline in second generation social and economic integration, this decline should have been more pronounced in communities with greater 'scope' to resist. This reasoning is consistent with the findings of Fouka (2019), who shows that 'backlash' to German language bans was more pronounced in areas with stronger German ethnic identity at the community-level. With this in mind, I examine whether second generation effects varied with respect to local social structures. In particular, using data from the Ethnographic Atlas (Murdock et al. 1999), I distinguish between reservations whose tribes were (historically) organised into extended family units (i.e., extended families or clans) versus those whose tribes were organised into independent families. I posit that communities organised into extended family units had greater scope to resist the cultural disruption brought about by the off-reservation school system. For example, the re-integration of returned students into the tribe is likely to have been easier to achieve when coordinated, and possibly enforced, by the tribe rather than by a single family.

To examine this empirically, I estimate a variation on equation 2:

$$
\begin{aligned}
y_{r,c,c'} = & \sum_{j;\, j \neq 24} \alpha'_j father_age_at_exposure_{j(r,c)} \\
& + \sum_{j;\, j \neq 24} \beta'_j father_age_at_exposure_{j(r,c)} \times has_extended_family_r \\
& + \alpha_r + \alpha_c + \alpha_{c'} + X'_{a(r),c}\gamma + \varepsilon_{r,c,c'}
\end{aligned} \tag{3}
$$

In addition to the variables defined in equation 2, equation 3 includes interactions between event time indicators and an indicator for whether an individual's reservation was organised into extended family units. The results in Table 6 show that second generation social and economic integration was indeed lower on reservations where tribes were organised into extended families. Second generation cohorts from such reservations were less likely to have a White American spouse in 1940 (column (1)), and less likely to be counted as 'White' (column (2)), than second generation cohorts from reservations organised into smaller, independent families. Consistent with the historical literature, column (3) suggests that one method of resistance was to withdraw children from (western) schools, though the estimated effect is not significant at conventional levels.

6.2. Effects driven by less assimilated first generation households

In order to interpret the decline in social and economic integration in the second generation, it is important to understand whether this decline was driven by (a) children from first generation households that assimilated (or attempted to assimilate) into White society returning to reservations, or (b) children from non-assimilated first generation households further withdrawing from White society.

In order to disentangle these possibilities, I estimate a variation on equation 3, where I interact event time indicators and an indicator for whether a second generation individual (as a child) was counted as 'White' in 1920 (my proxy for first generation integration). This amounts to splitting the sample on a first generation outcome, so the results that follow should be interpreted with this fact in mind.

Table 7 shows that, among second generation cohorts, the declines in intermarriage with White Americans (column (1)), the likelihood of being identified as 'White' (column (2)), and the probability of living in an urban area (column (3)), were driven by individuals from non-assimilated first generation households. While there are no differences in labour force participation (column (4)), individuals from non-assimilated first generation households earned lower wage income (column (5)).

TABLE 6. Heterogeneity by family structure

	White spouse	'White' in 1940	Finished primary school
	(1)	(2)	(3)
Average diff.	-0.176	-0.114	-0.101
(Ext. families - non-ext. families)	(0.061)	(0.062)	(0.063)
Mean dep. var	0.385	0.345	0.462
R2	0.221	0.221	0.167
No. reservations	24	24	24
No. cohorts	16	16	16
Obs.	1,304	1,438	1,420

Table shows estimates from equation 3 in the second generation sample. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Average diff.' is the difference in the average of post-treatment cohort effects on reservations where tribes are organised into extended families and reservations where tribes are organised into independent (nuclear) families. 'White spouse' is an indicator for having a White spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White'' is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. 'Finished primary school' is an indicator for having completed primary school, based on the IPUMS variable EDUCD. All regressions include reservation fixed effects, cohort fixed effects, father's cohort fixed effects. Standard errors are clustered at the reservation-level.

6.3. Changes in the attitudes of White Americans are unlikely to explain second generation effects

The results so far are consistent with resistance to off-reservation schools being a driver of the second generation reversal. An alternative explanation is that social and labour market assimilation in the first generation led to an increase in discrimination and / or social rejection of Native Americans that was borne by the second generation. One possibility, documented by Fouka, Mazumder, and Tabellini (2021) in the context of the 'Great Migration' of Black Americans to northern cities, is that greater exposure to Native Americans increased the salience of racial differences between White Americans and Native Americans, leading to subsequent discrimination.

This mechanism is unlikely to explain the reversal, as (a) second generation individuals from more assimilated households did not drive the decline in social and economic integration (as noted in the previous section), (b) there was not a large inflow of Native Americans into urban areas in the early-20th century (as was the case with Black Ameri-

TABLE 7. Heterogeneity by first generation assimilation

	White spouse	'White' in 1940	In urban area	In labour force	Wage inc.
	(1)	(2)	(3)	(4)	(5)
Average diff.	-0.332	-0.316	-0.146	0.014	-0.505
(Non-assim. first gen. - assim. first gen.)	(0.073)	(0.077)	(0.037)	(0.032)	(0.219)
Mean dep. var	0.385	0.345	0.145	0.933	4.475
R2	0.324	0.317	0.166	0.095	0.175
No. reservations	24	24	24	24	24
No. cohorts	16	16	16	16	16
Obs.	1,264	1,393	1,393	1,393	1,316

Table shows estimates from a variation on equation 3, as described in the text, in the second generation sample. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Average diff.' is the difference in the average of post-treatment cohort effects for individuals counted as 'Indian' in 1920 and individuals counted as 'White' in 1920 . 'White spouse' is an indicator for having a White spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White" is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. 'In urban area' is an indicator for residing in an urban area in 1940, based on the IPUMS variable URBAN. '(Log) Wage income' is the log of wage income, based on the IPUMS variable INCWAGE. All regressions include reservation fixed effects, cohort fixed effects, father's cohort fixed effects. Standard errors are clustered at the reservation-level.

cans) and (c) there is no evidence that broad public attitudes towards Native Americans deteriorated during the period of my study.

Owing to their small population size (between 280,000 and 345,000 over the period 1910 to 1940), it is unlikely that inflows of Native Americans to cities and urban areas affected the ethnic and racial fabric of these areas in the same way as the first Great Migration. Of the 86 metropolitan areas reported in the 1910 census, only three (Oklahoma City, Buffalo-Niagara Falls, and Los Angeles-Long Beach) received more than 500 Native Americans over the period 1910 to 1930. In all three cases, Native Americans accounted for less than 0.01 per cent of the population of these areas in 1930.[52] Given these magnitudes, it is implausible that inflows of Native Americans generated the same kinds of attitudinal

[52]These figures were calculated using the publicly available 1910 and 1930 full-count census data from IPUMS. The IPUMS variable 'METAREA' was used to identify metropolitan statistical areas.

shifts as inflows of Black Americans during the first Great Migration.

Following Fouka, Mazumder, and Tabellini (2021), I examine whether public attitudes towards Native Americans deteriorated over the period 1880 to 1940, as proxied by the language used in historical newspapers. I use Newspapers.com to obtain annual frequencies of Native American-related terms. I consider two measures: the frequency of the word 'Indian', and the frequency of co-occurences of the word 'Indian' and one of several derogatory terms used to describe Native Americans in Annual Reports of the Commissioner of Indian Affairs.[53] I then compute shares by dividing these counts by the total number of pages in a given year.[54] If majority attitudes towards Native Americans worsened over the 20th century, one would expect to see an associated increase in disparaging terms relating to them in the press. The trajectories of these outcomes, presented in Figure C.1, suggest that public attitudes towards Native Americans significantly *improved* over the period 1880 to 1910, and either improved or remained stable over the period 1910 to 1940. Taken together with the discussion above, it seems unlikely that the reversal in social assimilation documented in my main results was driven by discrimination or exclusion by White Americans.

7. Conclusion

The cultural assimilation of immigrants and ethnic minorities into the 'dominant' society has been, and remains, a divisive political issue in many countries. Throughout history, governments have sought to promote such assimilation with varying levels of coercion, and the intergenerational consequences of these efforts are not well known.

In this paper, I have studied the cultural effects of the off-reservation school system – arguably the most coercive assimilation effort in US history – across two generations. The schools were intended to reshape tribal identities and facilitate assimilation into western society: in the words of their architect, to 'kill the Indian… and save the man'. I have shown that off-reservation schools were effective in achieving substantial (outward) cultural assimilation in the first generation. But I also find that exposure generated a degree of resistance that was seemingly transmitted across generations. Ultimately, my findings suggest that off-reservation schools may have strengthened the identities they sought to erase.

My findings highlight the effectiveness of coercive assimilation policies, but also bring attention to their nuanced effects and potential reversal across generations. While studying the historical legacy of off-reservation schools is important in and of itself, my results

[53] These are: 'savage', 'uncivilized', and 'filthy'.

[54] In all cases, I exclude articles related to enslavements, marriages, and obituaries.

are also relevant to current debate on the assimilation of culturally diverse groups. An implication of my findings, that coercive assimilation efforts and / or derisive public discourse targeting certain groups can generate *intergenerational* backlash (Fouka 2019), is an important area for future study.

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Appendix A. Additional context and background

A.1. Additional information on off-reservation schools

A.1.1. Roll-out of off-reservation schools

The first off-reservation boarding schools, the Carlisle Indian School, was opened in 1879. Carlisle represented the culmination of efforts by an Army Officer, Richard Henry Pratt, to develop a new model of education for Native Americans. Pratt's interest in Native American education policy stemmed from his experience supervising prisoners of war at Fort Marion, Florida. He subsequently arranged for the transfer of some of these prisoners of war to be educated at the Hampton Institute, Virginia. Based on the success of this 'experiment', the decision was made of open Carlisle the following year (Office of Indian Affairs 1879, p. VIII). This model gained popularity with policymakers, and five off-reservation schools were opened over the next five years. These schools, in Kansas, Nebraska, New Mexico, Oklahoma and Oregon, were located closer to the communities from which they recruited, but were nonetheless located outside reservations. Schools continued to be opened over the next 20 years. Apart from Carlisle, Hampton and Haskell, all of these schools tended to recruit locally (within state or from adjacent states). Table A.1 shows the the names, locations, and opening years of off-reservation schools.

A.1.2. Curriculum

The 1890 Annual Report of the Commissioner of Indian Affairs includes an Appendix titled 'Rules for Indian Schools' (Office of Indian Affairs 1890). This document sets out the curriculum of Native American students, known as the 'course of study'. This curriculum was designed for reservation boarding schools, but was 'to be followed as far as practicable in day schools' (Office of Indian Affairs 1890, p. CLVI).

The course of study consisted of two four-year grades: primary grade and advanced grade. The first year of primary grade mainly consisted of English language instruction, basic reading and writing, and numbers from 1 to 10 (p. CLVI). The second year of primary grade built on the previous year's study of English, and added orthography, form and colour, penmanship and drawing, and geography (e.g., of the reservation or county). The third and fourth years of primary grade continued with the study of these topics, with arithmetic added in the fourth year (p. CLVIII).

The advanced grade covered similar content at a more advanced level. In terms of reading, each year was assigned a 'Reader' with increasing complexity. The list of Readers, at least in principle, common across schools.[55]. Content on US history, physiology and hygiene,

[55]The course of study was accompanied with a book list, set out on pp. CLXI-CLXII

TABLE A.1. Off-reservation school opening years

School	Location	Year opened
Hampton	*VA*	*1868*
Carlisle	PA	1879
Chemawa	OR	1880
Chilocco	OK	1884
Genoa	NB	1884
Albuquerque	NM	1884
Haskell	KS	1884
Grand Junction	CO	1886
Santa Fe	NM	1890
Fort Mojave	AZ	1890
Carson	NV	1890
Fort Stevenson	ND	1891
Pierre	SD	1891
Phoenix	AZ	1891
Fort Lewis	CO	1892
Fort Shaw	MT	1892
Perris	CA	1893
Flandreau	SD	1893
Pipestone	MN	1893
Mount Pleasant	MI	1893
Tomah	WI	1895
Wittenberg	WI	1895
Greenville	CA	1897
Morris	MN	1898
Chamberlain	SD	1898
Fort Bidwell	CA	1898
Rapid City	SD	1898
Riverside	CA	1902
Wahpeton	ND	1908
Bismark	ND	1908
Cushman	WA	1912

Table shows the names, location (state) and opening year of off-reservation schools. While not formally an off-reservation school, my dataset includes the Hampton Institute (VA), which was established in 1868 and first began taking Native American students in late-1878.
Sources: Adams (2020) and Gregg (2018).

and civil government were added in the final years of advanced grade (p. CLIX). In addition to academic work, students were provided with 'industrial training'. This covered topics such as farming, instruction in trades (for boys), housekeeping (for girls). At least half of the school day was to be devoted to industrial work (p. CLII).

The level of education at off-reservation schools, at least until the 1920s, was generally at the primary level. While the largest schools, such as Carlisle and Haskell did offer commercial and normal (teacher training) courses beyond the 8-year programme, they never aspired to provide a high-school education (Pratt 1912, p. 13). In fact, even these post-primary courses were scaled back when, in the early-1900s, school adminstrators were explicitly instructed *not* to provide instruction above the level of the eighth grade (Vuckovic 2008, p. 94). This reflected a change in Indian education policy in the early-1900s, which led to a shift from academic training to vocational work (Adams 2020, p. 172). It was not until the-late 1920s that the first off-reservation schools were authorised to teach senior high-school grades (i.e., grade 10 to grade 12) (Office of Indian Affairs 1926, p. 7).

Appendix B. Data appendix

B.1. Data sources

Annual Reports of the Commissioner of Indian Affairs. My main archival sources of data are annual reports by the Office of Indian Affairs to the Secretary of the Interior, known as the Annual Reports of the Commissioner of Indian Affairs. I focus on Annual Reports during the roll-out of the off-reservation school system, from 1879 to 1900. The content of Annual Reports varies from year to year, but usually includes individual reports by off-reservation school superintendents and Indian agents. School reports contain information on the general affairs of the school (e.g., buildings, staff, curriculum), as well as (but not always) information on the tribes and / or reservations from which schools recruited students. Though not their focus, agent reports sometimes include information on transfers of children made to off-reservation schools.

The Annual Reports contain statistical tables at the agency-level with information on educational and assimilation-related outcomes. The variables in the statistical tables include: population, literate population, English-speaking population, population in 'citizens' (western) dress, the presence of schools (day or reservation boarding) and church-going population. In order to obtain information on the pre-treatment characteristics of agencies, I digitise the available statistical tables for the years 1876 (the earliest available) to 1879.

In addition, each Annual Report contains a schedule listing all reservations in the continental United States in that year, along with the tribes that occupied those reservations. Figure B.1 shows an excerpt of from the 1890 Annual Report. I digitise these schedules from the years 1880 to 1900, and also create decennial crosswalks that allow me to track splits and merges of reservations over the period of my study. I supplement the schedules with Native American settlements that were not formal reservations, but were still targeted by off-reservation schools. Specificaly, I include settlements in the vicinity of (tribes in parentheses): Carson City, Nevada (Washoe), Fort Bidwell, California (Paiute, Pit River), Fort Mohave, Arizona (Mohave), the Chemehuevi Valley, Arizona / California (Chemehuevi), Greenville, California (Maidu, Concow and Washoe), Little Traverse Bay, Michigan (Ottawa), and Tomah, Wisconsin (Winnebago). Information on these settlements is obtained from Annual Reports and secondary sources.

FIGURE B.1. Schedule of Reservations in the United States, 1890

Schedule showing the names of Indian reservations in the United States, agencies, tribes occupying or belonging to the reservation, area of each reservation (unallotted) in acres or square miles, and reference to treaty, law, or other authority by which reservations were established.

Name of reservation.	Agency.	Name of tribe occupying reservation.	Area in acres.	Square miles (a)	Date of treaty, law, or other authority establishing reserve.
ARIZONA TERRITORY.					
Colorado River (b)	Colorado River	Hwalapai, Kemahwivi (Tantawait), Koahualla, Mokopa (c), Mohavi, and Yuma	*df*300,800	470	Act of Congress approved Mar. 3, 1865, vol. 13, p. 559; Executive orders, Nov. 22, 1873, Nov. 16, 1874, and May 15, 1876.
Gila Bend	Pima	Papaho	*e*22,391	35	Executive order, Dec. 12, 1882.
Gila River	do	Marikopa and Pima	357,120	558	Act of Congress approved Feb. 28, 1859, vol. 11, p. 401; Executive orders, Aug. 31, 1876, Jan. 10, 1879, June 14, 1879, May 5, 1882, and Nov. 15, 1883.
Hualpai		Hwalapai	730,880	1,142	Executive order, Jan. 4, 1883.
Moqui	Navajo	Moqui (Shinumo)	2,508,800	3,920	Executive order, Dec. 16, 1882.
Papago	Pima	Papaho	*e*70,080	109¼	Executive order, July 1, 1874, and act of Congress approved Aug. 5, 1882, vol. 22, p. 299.
Salt River	do	Marikopa and Pima	*f*46,720	73	Executive order, June 14, 1879.
Suppai		Suppai	*d*38,400	60	Executive orders, June 8, Nov. 23, 1880, and Mar. 31, 1882.
White Mountain	San Carlos	Aravapai, Chilion, Chirikahwa, Koiotero, Mienbre, Mogollon, Mohavi, Pinal, San Carlos, Santo, Tonto, and Yuma-Apache.	*d*2,528,000	3,950	Executive orders, Nov. 9, 1871, Dec. 14, 1872, Aug. 5, 1873, July 21, 1874, April 27, 1876, Jan. 26, and Mar. 31, 1877.
Total			6,603,191	10,317½	
CALIFORNIA.					
Hoopa Valley	Hoopa Valley	Hunsatung, Hupâ, Klamath River, Miskut, Redwood, Saiaz, Sermalton, and Tishtanatan.	*d*89,572	140	Act of Congress approved Apr. 8, 1864, vol. 13, p. 39; Executive order, June 23, 1876.
Klamath River	do	Klamath River	*e*25,600	40	Executive order, Nov. 16, 1855.
Mission (22 reserves)	Mission, Tule	Coahuila, Diegenes, San Luis Rey, Serranos and Temecula.	*f*182,315	284½	Executive orders, Dec. 27, 1875, May 15, 1876, May 3, Aug. 25, Sept. 29, 1877, Jan. 17, 1880, Mar. 2, Mar. 9, 1881, June 27, July 24, 1882, Feb. 5, June 19, 1883, Jan. 25, Mar. 22, 1886, Jan. 29, Mar. 14, 1887, and May 6, 1889.
Round Valley	Round Valley	Konkau, Little Lake, Pitt River, Potter Valley, Redwood, Wailakki, and Yuki.	*df*102,118	159½	Acts of Congress approved Apr. 8, 1864, vol. 13, p. 39, and Mar. 3, 1873, vol. 17, p. 634; Executive orders, Mar. 30, 1870, Apr. 8, 1873, May 18, 1875, and July 26, 1876.
Tule River	Mission, Tule	Kawai, Kings River, Monache, Tehon, Tule, and Wichumni.	*d*48,551	76	Executive orders, Jan. 9, Oct. 3, 1873, and Aug. 3, 1878.
Yuma	do	Yuma	*e*45,889	72	Executive order, Jan. 9, 1884.
Total			494,045	772	

Source: Office of Indian Affairs (1890).

B.2. Data construction

B.2.1. Indian censuses

As the full count censuses do not include information on tribes (which is needed to match individuals to reservations), I draw on more detailed state-level 'Indian censuses' conducted by Indian agents. Indian censuses were not conducted by every agency every year, so I construct a complete cross-section using censuses on or around 1910. The choice of year is based on (a) proximity to 1910 (since I match my Indian census cross-section to the 1910 population census) and (b) similarity between the reservation population reported in the Indian census and the reservation population reported in the 1910 Annual Report of the Commissioner of Indian Affairs (Office of Indian Affairs 1909), and (c) the availability of information needed for linking (i.e., names and birth years). Table B.1 shows the specific censuses used in the construction of the cross-section. Since Indian censuses were not taken for the Cherokee, Chickasaw, Choctaw, Creek and Seminole in Oklahoma, I obtain equivalent information from enumerations of these tribes in the Dawes Rolls, 1907. I obtain all Indian censuses, as well as the Dawes Rolls, from the relevant collections at Ancestry.com. I combine separate Indian censuses into a single dataset; in total, there are around 250,000 individuals in the combined census.

I then match individuals in the combined Indian census to their reservations. I do so by matching tribes (reported in the Indian census) to reservations as reported in reservation schedules from Annual Reports of the Commissioner of Indian Affairs (e.g., Figure B.1). In some cases, reservations are reported in the Indian censuses, which I match directly. In total, I am able to match 235,000 individuals in the combined Indian census to a unique reservation (around 94 per cent).

I construct an analogous cross-section of Indian censuses around the year 1930 in the same way. The cross-section consists of roughly 320,000 individuals, of which 160,000 are males. The sole use of the 1930 Indian census is to generate a measure of attachment to one's reservation or agency (described in Section B.3. I do not use the 1930 Indian census to match individuals to reservations.

TABLE B.1. Indian censuses used for 1910 cross-section

State	Year	Agency, Reservation or Tribe
Arizona	1896	Pima and San Xavier Papago, Pima Agency
Arizona	1905	Colorado River Agency
Arizona	1909	Camp McDowell Agency, San Carlos Agency
Arizona	1910	Fort Mojave Agency, Truxton Canon Agency
Arizona	1911	Havasupai, Kaibab Agency, Moqui Agency
Arizona	1916	Camp Verde Agency, Fort Apache Agency
California	1905	Digger
California	1910	Santa Rosa, Capitan Grande Agency, Hoopa Valley Agency, Martinez Agency, Mesa Grande Agency, Pala Agency, Pechanga, Rincon Reservation, Round Valley Agency, Soboba Agency, Tule River Agency
California	1911	Cahuilla
California	1913	Mission Creek, Malki Agency, Upper Lake Agency
California	1914	La Jolla Agency
California	1915	Fort Yuma Agency, Greenville Agency
California	1916	Potter Valley, Upper Lake Agency, Upper Lake, Upper Lake Agency
Colorado	1910	Navaho Springs Agency, Southern Ute Agency
Idaho	1910	Fort Hall Agency, Fort Lapwai Agency
Idaho	1915	Coeur d'Alene Reservation
Iowa	1910	Sac and Fox Agency, Iowa
Kansas	1906	Kickapoo Agency
Kansas	1910	Pottawatomie Agency
Michigan	1910	L'Anse, Ontonagon
Michigan	1915	Bay Mills Agency
Minnesota	1910	Fond du Lac Agency, Leech Lake Agency, Red Lake Agency, White Earth Agency
Minnesota	1911	Nett Lake Agency
Montana	1908	Crow Agency
Montana	1909	Fort Belknap Agency
Montana	1910	Flathead Agency, Fort Peck Agency, Tongue River Agency
Montana	1911	Blackfeet Agency
Nebraska	1910	Santee Agency, Winnebago Agency
Nevada	1910	Fallon Agency, Fort McDermitt Agency, Moapa River Agency, Nevada Agency, Walker River Agency, Western Shoshoni Agency
New Mexico	1907	Zuni Agency
New Mexico	1910	Jicarilla Agency, Santa Fe Agency
New Mexico	1912	Albuquerque Agency
New Mexico	1913	Mescalero Agency
New York	1910	Cayuga, Oneida, Onondaga, Seneca, Tuscarora
North Carolina	1910	Eastern Cherokee Agency
North Dakota	1910	Fort Berthold Agency, Devils Lake Agency, Standing Rock Agency
North Dakota	1907	Turtle Mountain Chippewa
Oklahoma	1899	Citizen Potawatomi
Oklahoma	1910	Cantonment Agency, Cheyenne and Arapaho Agency, Kaw Agency, Kiowa Agency, Osage Agency, Otoe Agency, Pawnee Agency, Ponca Agency, Red Moon Agency, Sac and Fox Agency, Seger Agency, Seneca Agency
Oklahoma	1915	Mexican Kickapoo
Oklahoma	1923	Apache at Fort Sill
Oregon	1910	Klamath Agency, Siletz Agency
Oregon	1911	Umatilla Agency, Warm Springs
South Dakota	1908	Crow Creek Agency
South Dakota	1910	Cheyenne River Agency, Lower Brule Agency, Sisseton Agency, Yankton Agency
South Dakota	1911	Flandreau Agency
South Dakota	1915	Pine Ridge Agency, Rosebud Agency
Utah	1910	Shivwits Agency, Uintah and Ouray Agency
Washington	1910	Colville Agency, Cushman Agency, Neah Bay Agency
Washington	1911	Muckleshoot, Tulalip, Suquamish, Yakima Agency
Wisconsin	1910	Keshena Agency, La Pointe Agency, Oneida, Wittenberg School
Wyoming	1910	Shoshone

Table shows years used to construct my cross-section of Indian censuses. The Navajo (Arizona, New Mexico, and Utah) were not completely enumerated until 1930, and are excluded. No complete enumeration was taken of the St Regis reservation (New York).
Sources: Own work.

B.2.2. Matching Native Americans to reservations in 1910

Around 260,000 Native Americans were counted in continental US states in the 1910 census.[56] I use information on locations in the 1910 census, as well as my cross-section of Indian censuses, to match around 75 per cent of these individuals to a unique reservation. I focus on the 1910 census because it is the first census in which the majority of the first generation to be exposed to off-reservation schools (i.e., born between roughly 1860 and 1890) had reached adulthood.[57]

In order to match individuals on the basis of location, I follow a similar strategy to Dippel and Frye (2020). This involves matching individuals to reservations using information on their place of residence or their enumeration district in the 1910 census.[58] While place of residence is not available in the publicly-available census from IPUMS, a string variable for standardised minor civil division (STDMCD), is available in the restricted version. I inspect each STDMCD with at least one Native American in 1910. In some cases, STDMCD directly refers to a reservation; in such cases, I match all individuals belonging to that STDMCD to the reservation. In other cases, STDMCD refers to a place or Public Land Survey System (PLSS) township.[59]. If STDMCD refers to a place, I search for the place using Google Maps, and overlay the geocoded place on historical reservation boundaries. If the place can be matched to a unique reservation within 100 kilometres, I match all individuals living in that place to the reservation. I conduct a similar exercise with PLSS townships. Firstly, I match townships to PLSS shapefiles from the Bureau of Land Management, and then overlay township centroids on historical reservation boundaries.

I repeat the process above using enumeration districts. Specifically, I obtain a list of enumeration districts with at least one Native American, and merge these to enumeration district descriptions from the Unified 1910 Census ED Finder (Morse and Weintraub 2011).[60] I then geocode place names or townships and match the geocoded points to historical reservation boundaries.

The method above is ineffective in two cases. Firstly, Native Americans that were living more than 100 kilometres from a reservation in 1910 are not matched. This presents a selection problem, since individuals that attended off-reservation schools may have

[56] This figure excludes roughly 20,000 Alaska Natives.

[57] Information on locations comes from enumeration district descriptions (obtained from `https://stevemorse.org/census/unified.html`) and standardised minor civil divisions (a string variable available in the restricted IPUMS data). While reservations are sometimes directly named, this is not always the case (e.g., a description may refer to a town inside a reservation).

[58] Enumeration districts are the most granular unit available for the entirety of the United States in the 1910 census.

[59] The PLSS is a method of surveying land in the United States for the purposes of sale or settlement, whereby land is divided into townships, ranges and sections

[60] The website can be accessed here: `https://stevemorse.org/census/unified.html`.

moved to urban areas after finishing their term, rather than returning to their reservations. Secondly, I cannot match locations to reservations if the location is sufficiently close to multiple reservations. This is particularly problematic in Washington and north-east Oklahoma, where there was a concentration of small reservations.

In both cases, I use record-linking methods to match individuals in my 1910 Indian census to the 1910 census.[61] Since I use a cross-section of Indian censuses around the year of 1910, I am able to match both males and females. I match around 50,000 individuals to a unique reservation with this procedure. In order to increase the number of matches, I also match within households (e.g., if the father in a household is matched, but the children are not, these children inherit the reservation of their father). In total, I am able to match around 80,000 individuals to a unique reservation using the 1910 Indian census.

Using locations and the 1910 Indian census, I am able to match around 234,000 individuals to a unique reservation. In my main results, I conservatively remove individuals that were matched to different reservations on the basis using location-based and Indian census-based matching. After applying this restriction, I am left with 194,000 individuals (around 75 per cent of all Native Americans in continental US states in 1910). Figure B.2 shows state population shares of male, Native American household heads (who form the basis of my sample) as counted in the 1910 census (blue circles) and in the sample matched to reservations (red circles). State-level population shares in the matched sample are generally close to state-level population shares in the 1910 census. Two exceptions are California and Michigan: in both states, a large number of Native Americans were not living on or near any formal reservations during the roll-out of the off-reservation system.

B.2.3. Off-reservation school treatment years

Using primary and secondary historical sources, I construct the first dataset on reservation-level exposure to off-reservation schools during the 19th and early-20th centuries.

Firstly, I read all annual school reports by the 26 off-reservation schools that were established prior to 1900, and identify the tribes recruited by each off-reservation school in a given year.[62] I match tribes recruited by off-reservation schools (as reported in school reports) to reservations using reservation schedules for the relevant year, taken from the Annual Reports of the Commissioner of Indian Affairs. I supplement this information from agents' reports, which sometimes made references to children from the agency being transferred to off-reservation schools.

[61] For my main results, I use the ABE JW method. This method requires exact matches on state of birth, minor spelling differences in names, and for birth years to differ by +/- 5 years between records. I use publicly available codes made available by the Census Linking Project (`https://censuslinkingproject.org`).

[62] Three off-reservation schools were established after 1900. Two schools (Bismarck and Wahpeton) opened in North Dakota in 1908, and another (Cushman) opened in Washington in 1912.

FIGURE B.2. Population shares in 1910 census and in sample matched to reservations, by state

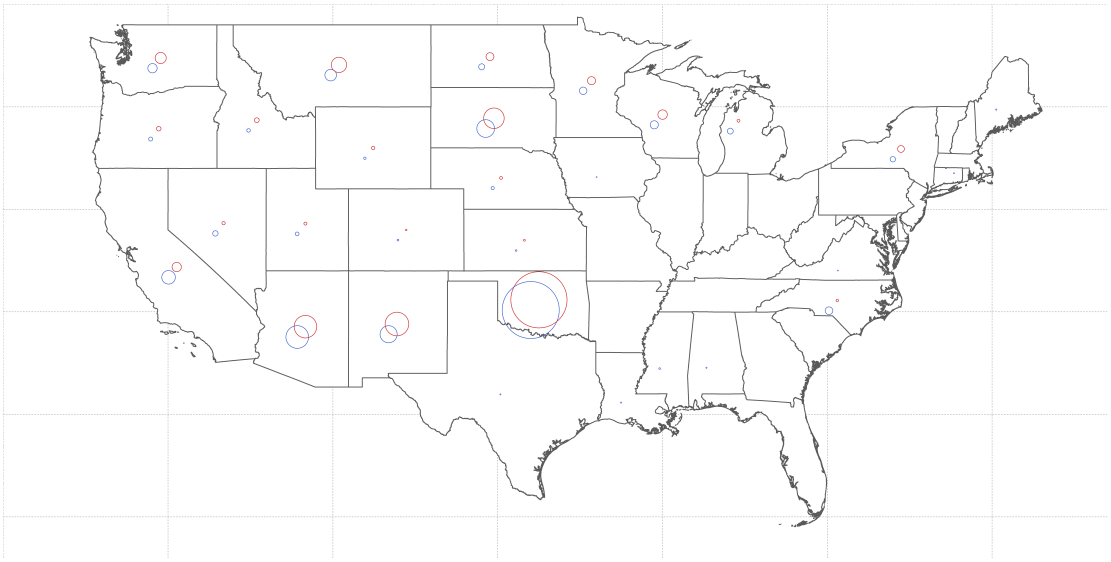


Figure shows state-level population shares of male, Native American household heads in the 1910 census (blue circles) and state-level population shares of male, Native American household heads in the sample matched to reservations (red circles).
Source: own calculations.

In order to identify the years that reservations were exposed to Carlisle, Chemawa, Hampton and Haskell (i.e., the schools for which I have attendance records), I use information on the tribes, home agencies and home addresses (if available) to match students to their home reservations.[63] I match tribes to reservation using the reservation schedules, as above. In order to match home addresses to reservations, I geocode home addresses using Google Maps, and overlay geocoded addresses on 1889 reservation boundaries. Finally, for individuals that cannot be matched to a unique reservation on the basis of the above information, I search their surname in the 1910 census (where individuals have already been matched to reservations). If the surname is associated with a unique reservation, I match the individual in the attendance data to that reservation. In total, I am able to match around 85 per cent of attendees to a reservation.

Finally, I supplement the treatment years above using information from secondary historical sources. These are typically research articles or theses focusing on a particular off-reservation school or reservation. Secondary sources are listed in the next subsection.

I combine matches from all the methods above, and identify the first year that a reservation sent students to an off-reservation school as that reservation's treatment year. Table B.2 lists the reservations treated each year, from 1879 to 1900. I do the same for schools in my attendance data (Carlisle, Chemawa, Hampton and Haskell). Table B.3 list the reservation treated by these schools, and the years in which they were treated.

[63] I do not use Chilocco attendance data for this purpose, because entry years are inconsistent with enrollment figures in school reports

TABLE B.2. Off-reservation school treatment years

Year	Newly-treated reservations
1879	Fort Berthold (North Dakota), Yankton (South Dakota), Cheyenne And Arapaho (Oklahoma), Kiowa And Comanche (Oklahoma), Pine Ridge (South Dakota), Rosebud (South Dakota)
1880	Puyallup (Washington), Warm Springs (Oregon), Colville (Washington)
1881	Creek (Oklahoma), Osage (Oklahoma), Southern Pueblo (New Mexico), Standing Rock (North Dakota), Lower Brule (South Dakota), Chehalis (Washington), Umatilla (Oregon), Wind River (Wyoming)
1882	Omaha (Nebraska), Pawnee (Oklahoma), Cheyenne River (South Dakota)
1883	Crow (Montana), Crow Creek (South Dakota), Skokomish (Washington), Lapwai (Idaho), Yakima (Washington)
1884	White Mountain (Arizona), Oneida (Wisconsin), Ponca (Oklahoma), Chippewa And Munsee (Kansas), Potawatomi (Kansas), Siletz (Oregon), Spokan (Washington), Wichita (Oklahoma)
1885	Potawatomi (Oklahoma), Klamath (Oregon), Snohomish Or Tulalip (Washington), Grande Ronde (Oregon), Iowa (Oklahoma), Sauk And Fox (Oklahoma), Winnebago (Nebraska)
1886	Shawnee (Oklahoma), Mescalero Apache (New Mexico), Ute (Colorado), Gila River (Arizona), Salt River (Arizona), Oakland (Oklahoma), Ponca (South Dakota)
1887	Chickasaw (Oklahoma), Wyandot (Oklahoma), Hupa Valley (California), Navaho (New Mexico), Choctaw (Oklahoma), Seminole (Oklahoma), Peoria (Oklahoma), Cherokee (Oklahoma), Fort Belknap (Montana), Niobrara (Nebraska), Pyramid Lake (Nevada), Walker River (Nevada)
1888	Seneca (Oklahoma), Port Madison (Washington), Klamath River (California), Uinta Valley (Utah)
1889	Isabella (Michigan), Little Traverse Bay (Michigan), Blackfeet (Montana), Potawatomi Of Huron (Michigan), Iowa (Nebraska), Turtle Mountain (North Dakota), Jocko (Montana)
1890	Fort Peck (Montana), Tuscarora (New York), Iowa (Kansas), Lummi (Washington), Fort Mohave And Surroundings (Arizona), Chemehuevi Valley (Arizona), Walapai (Arizona), Navaho (Arizona), Kansa (Oklahoma), Kickapoo (Oklahoma)
1891	Hopi (Moqui) (Arizona), Round Valley (California), Carson And Surroundings (Nevada), Duck Valley (Nevada), Northern Pueblo (New Mexico), Lake Traverse (South Dakota), Jicarilla Apache (New Mexico)
1892	Cattaraugus (New York), Pine Ridge (Nebraska), White Earth (Minnesota), Modoc (Oklahoma), Kickapoo (Kansas), Makah (Washington), Papago (Arizona), Gila Bend (Arizona)
1893	Qualla Boundary And Other Lands (North Carolina), Menominee (Wisconsin), Sauk And Fox (Iowa), Lanse (Michigan), Ontonagon (Michigan), Mission (California), Tule River (California), Tomah And Surroundings (Wisconsin), Northern Cheyenne (Montana)
1894	Black Bob (Kansas), Sauk And Fox (Kansas), Quapaw (Oklahoma), Greenville And Surroundings (California)
1895	Stockbridge (Wisconsin), Miami (Kansas), Fond Du Lac (Minnesota), Onondaga (New York)
1896	Lac Du Flambeau (Wisconsin), Ottawa (Oklahoma), Swinomish (Perrys Island) (Washington), Coeur Dalene (Idaho), Zuni (New Mexico)
1897	Fort Hall (Idaho), Lac Court Oreilles (Wisconsin), Osette (Washington), Hopi (Arizona)
1898	Tonawanda (New York), Red Lake (Minnesota), Crow Creek And Old Winnebago (South Dakota), Fort Bidwell And Surroundings (California), Mille Lac (Minnesota)
1899	Saint Regis (New York), La Pointe (Bad River) (Wisconsin), Uncompahgre (Utah), Quileute (Washington), Nisqualli (Washington), San Carlos (Arizona), Fort Apache (Arizona), Yuma (California), Colorado River (Arizona), Oto (Oklahoma)
1900	Muckleshoot (Washington)

Source: own work using data from Annual Reports of the Commissioner of Indian Affairs, 1879 - 1900, attendance records for Carlisle, Chemawa, Hampton and Haskell, and secondary sources.

TABLE B.3. Off-reservation school treatment years, Carlisle, Chemawa, Hampton and Haskell

Year	Newly-treated reservations
1879	Fort Berthold (North Dakota), Crow Creek (South Dakota), Cheyenne River (South Dakota), Yankton (South Dakota), Lower Brule (South Dakota), Standing Rock (North Dakota), Cherokee (Oklahoma), Cheyenne And Arapaho (Oklahoma), Kiowa And Comanche (Oklahoma), Menominee (Wisconsin), Lapwai (Idaho), Pawnee (Oklahoma), Ponca (Oklahoma), Seminole (Oklahoma), Pine Ridge (South Dakota), Rosebud (South Dakota), Lake Traverse (South Dakota), Wichita (Oklahoma), Potawatomi (Oklahoma)
1880	Iowa (Nebraska), Sauk And Fox (Nebraska), Southern Pueblo (New Mexico), Omaha (Nebraska), Winnebago (Nebraska), Puyallup (Washington), Nisqualli (Washington), Warm Springs (Oregon), Klamath (Oregon), Colville (Washington)
1881	Wind River (Wyoming), Creek (Oklahoma), Fort Belknap (Montana), Peoria (Oklahoma), Modoc (Oklahoma), Navaho (New Mexico), Osage (Oklahoma), Ottawa (Oklahoma), Gila River (Arizona), White Mountain (Arizona), Papago (Arizona), Chehalis (Washington), Umatilla (Oregon)
1882	Kansa (Oklahoma), Navaho (Arizona), Sauk And Fox (Oklahoma), Onondaga (New York)
1883	Crow (Montana), Snohomish Or Tulalip (Washington), Skokomish (Washington), Yakima (Washington), Port Madison (Washington)
1884	Oneida (Wisconsin), Stockbridge (Wisconsin), Leech Lake (Minnesota), Chippewa And Munsee (Kansas), Wyandot (Oklahoma), Potawatomi (Kansas), Miami (Kansas), Grande Ronde (Oregon), Makah (Washington), Siletz (Oregon), Spokan (Washington)
1885	Quapaw (Oklahoma), Seneca (Oklahoma)
1886	Shawnee (Oklahoma)
1887	Chickasaw (Oklahoma), Fort Peck (Montana), Round Valley (California), Hupa Valley (California)
1888	Ponca (Nebraska), Niobrara (Nebraska), Allegany (New York), Kickapoo (Kansas), Kickapoo (Oklahoma), Klamath River (California), Mission (California)
1889	Isabella (Michigan), Little Traverse Bay (Michigan), Blackfeet (Montana), Qualla Boundary And Other Lands (North Carolina), Potawatomi Of Huron (Michigan), Pyramid Lake (Nevada)
1890	Fort Hall (Idaho), Tuscarora (New York), Flandreau (South Dakota), Oto (Oklahoma), Iowa (Kansas), Sauk And Fox (Kansas), Sauk And Fox (Iowa), Lummi (Washington)
1891	Jocko (Montana), Tonawanda (New York), Pine Ridge (Nebraska), Hopi (Moqui) (Arizona), Iowa (Oklahoma)
1892	Saint Regis (New York), Cattaraugus (New York), White Earth (Minnesota), Oneida (New York), Columbia (Washington)
1893	Tomah And Surroundings (Wisconsin)
1894	La Pointe (Bad River) (Wisconsin), Black Bob (Kansas), Turtle Mountain (North Dakota), Red Lake (Minnesota), Coeur Dalene (Idaho)
1895	Fond Du Lac (Minnesota), Lac Court Oreilles (Wisconsin), Choctaw (Oklahoma), Devils Lake (North Dakota)
1896	Northern Cheyenne (Montana), Lac Du Flambeau (Wisconsin), Mdewakanton (Minnesota), Swinomish (Perrys Island) (Washington)
1897	Lanse (Michigan), Mille Lac (Minnesota), Red Cliff (Wisconsin), Muckleshoot (Washington), Osette (Washington)
1898	Jicarilla Apache (New Mexico), Duck Valley (Nevada), Carson And Surroundings (Nevada), Yuma (California), Uinta Valley (Utah), Mescalero Apache (New Mexico)
1899	San Carlos (Arizona), Northern Pueblo (New Mexico), Uncompahgre (Utah), Quileute (Washington), Quinaielt (Washington)

Source: own work using attendance records for Carlisle, Chemawa, Hampton and Haskell, and secondary sources.

B.2.4. Secondary sources used for off-reservation school treatment years

Albuquerque Indian School, New Mexico. I rely on McKinney (1945). The Albuquerque Indian School was opened as a contract school in 1881, and transferred to government ownership in 1886 (McKinney 1945, p. 118 and p. 121). To account for the fact that Albuquerque recruited from the so-called 'Southern Pueblos', I construct a new reservation called 'Southern Pueblo' that groups the Pueblos south of Santa Fe. I assume that reservations were only 'treated' by Albuquerque from 1886, when the school came under government control.

Carson Indian School, Nevada. I rely on Thompson (2013). Thompson (2013, p. 5) reports that the Carson 'enrolled children from Washoe, Western Shoshone, and Northern Paiute... The first class of thirty-seven students arrived on December 17, 1890'. Therefore, I assume that the corresponding reservations (Duck Valley, Pyramid Lake, and Walker River) were treated by Carson in 1891 (the end of the school's first year in operation). Since the Washoe did not live on a formal reservation, I construct a new reservation called 'Carson Colony' to account for them in the data (also treated in 1891). This addition is informed by the discussion in Thompson (2013, p. 25).

Chilocco Indian School. I rely on Lomawaima (1994). Lomawaima (1994), p. 10, reports that Chilocco recruited students from 'Cheyenne, Arapaho, Wichita, Comanche, and Pawnee tribes in 1884'. Therefore, I assume that the corresponding reservations in Oklahoma (Cheyenne and Arapaho, Kiowa, Comanche and Wichita, and Pawnee) were treated by Chilocco in 1884 (the school's opening year).

Phoenix Indian School. I rely on Trennert (1988). I follow the Trennert (1988)'s account of school recruitment patterns, which is based on primary historical sources. Phoenix focused on recruiting from nearby reservations in its early years, starting with Pima and Maricopa children in 1891. Accordingly, I assume that the Salt River and Gila River reservations were treated in 1891. The school also attempted to recruit 'Papagos from the south' in the 1892-1893 school year (Trennert 1988, p. 39). I cannot distinguish between Papago living on the Gila Bend reservation and those living on the Papago reservation, so I assume both were treated in 1892. Finally, the school expanded recruitment widely from 1897 onwards, targeting agencies in California, New Mexico and Oregon (Trennert 1988, p. 64). I assign treatment years to the corresponding agencies and reservations in my dataset accordingly.

Santa Fe Indian School. I rely on Gram (2015). Gram (2015, p. 177) reports that the Santa Fe Indian School primarily served the 'Northern Pueblos'; with the Pueblos south of Santa

Fe generally served by the Albuquerque Indian School. I construct a new reservation called 'Northern Pueblo' that groups the Pueblos north of Santa Fe, and assume that this reservation was treated by Santa Fe in 1891 (the end of the school's first year in operation).

B.2.5. Representativeness of linked samples and reweighting

In my main analysis, various results are obtained by linking individuals across census years using the Census Tree (i.e., adults linked from 1910 to 1920, and children linked from 1910 to 1940). Individuals are not randomly selected into these linked samples. Table B.4 and Table B.5 show differences in 1910 characteristics between the baseline and unweighted linked samples for the first generation and second generation, respectively. Column (3) of Table B.4 shows that adults linked from 1910 to 1920 had shorter names, were more likely to be literate and speak English, were more likely to be married to a white American, were more likely to be in the labour force, and were younger. There are similar differences between the pre-linked second generation sample in 1910 and the sample linked to the 1940 census (Column (3) of Table B.5).

In order to account for selection into the linked samples, I follow the literature and reweight observations by the probability of being linked (e.g., Bailey, Cole, and Massey 2020). For both generations, I estimate a probit model where the outcome is an indicator for being linked from 1910 to a later census. I use the following covariates: state of birth, year of birth, a quadratic in age, length of first name, and length of last name.[64] For my baseline results, I use this limited set of predictors because many commonly-used covariates (e.g., literacy or occupation) can be thought of as 'outcomes' in my setting. Using the results from the probit regression, I predict the conditional probability of being linked, p_i. I then construct inverse propensity weights IPW_i as the reciprocal of p_i.

The results for the first generation sample are shown in Table B.4. Individuals in the linked first generation sample are more likely to have been literate, to speak English, to have a white spouse, and to have been in the labour force in 1910 (Column (3)). After reweighting, differences in mean characteristics are smaller, but still present (Column (5)).

Corresponding results for the second generation sample are reported in Table B.5. Individuals in the linked second generation sample are more likely to have had fathers that were literate and could speak English, and to have a white mother (Column (3)). Once again, differences in mean characteristics between the pre-linked sample and the reweighted linked sample are smaller in magnitude, but remain present.

The fact that there are differences in mean characteristics after reweighting suggest that there is still a degree of selection into the linked samples. For this reason, I also present

[64] These are derived from the following IPUMS variables: STATEICP, BIRTHYR, NAMEFRST and NAMELAST.

results with a more 'aggressive' reweighting procedure that uses a broader set of covariates – including literacy, English proficiency, and marital status (or for the second generation, father's marital status) in 1910 – to calculate the conditional probability of being linked from the 1910 census to a later census. Differences in mean characteristics between the pre-linked and linked first generation samples are shown in Table B.6. Table B.7 shows the corresponding comparison between the pre-linked and linked second generation sample. The linked samples are now (mechanically) similar to their respective pre-linked samples on the basis of literacy and English proficiency – two important characteristics that are likely to have affected (a) the probability of being linked and (b) racial classification in later censuses.

To demonstrate that selection is not driving my results, I present results on 1920 racial classification in the first generation sample using different weighting schemes in Table D.9. Column (1) shows estimates without weighting, Column (2) shows my main estimates ('baseline' reweighting), and Column (3) shows estimates using extra covariates to predict links ('extended' reweighting). Regardless of the reweighting scheme, the coefficient estimates are largely unchanged. Table D.10 shows corresponding estimates in the second generation sample. Here, estimates in the reweighted samples are larger in magnitude, but my main conclusions are not affected by the reweighting scheme.

TABLE B.4. Reweighting, first generation sample

	Full sample	Linked samples			
		Unweighted		Weighted	
	Mean	Mean	Diff. in means	Mean	Diff. in means
	(1)	(2)	(3)	(4)	(5)
Length first name	6.338	5.928	-0.411	6.214	-0.124
			(0.029)		(0.032)
Length last name	6.931	6.687	-0.244	6.82	-0.111
			(0.031)		(0.033)
Literate	0.39	0.568	0.179	0.492	0.102
			(0.007)		(0.007)
Speaks English	0.681	0.828	0.147	0.772	0.091
			(0.005)		(0.006)
White spouse	0.018	0.031	0.013	0.027	0.008
			(0.002)		(0.002)
In labour force	0.864	0.891	0.027	0.888	0.024
			(0.004)		(0.004)
Occ. inc. score	2.744	2.746	0.002	2.737	-0.007
			(0.005)		(0.005)
Owns home	0.801	0.795	-0.006	0.81	0.009
			(0.005)		(0.005)
Age	39.817	38.434	-1.382	40.08	0.263
			(0.139)		(0.147)

Table shows differences in mean characteristics for the initial first generation sample and the subsample that was successfully linked to the 1920 census using the Census Tree. All characteristics are measured in 1910. Column (1) reports mean statistics for the first generation sample prior to linking. Column (2) reports mean statistics for the linked sample without weighting, and Column (3) shows the associated difference in means. Column (4) reports mean statistics for the linked sample with weighting, and Column (6) shows the associated difference in means.

TABLE B.5. Reweighting, second generation sample

	Full sample	Linked samples			
		Unweighted		Weighted	
	Mean	Mean	Diff. in means	Mean	Diff. in means
	(1)	(2)	(3)	(4)	(5)
Length first name	6.521	6.134	-0.387	6.323	-0.198
			(0.035)		(0.038)
Length last name	6.95	6.722	-0.227	6.789	-0.161
			(0.039)		(0.040)
Father literate	0.372	0.571	0.199	0.498	0.126
			(0.009)		(0.009)
Father speaks English	0.638	0.796	0.157	0.727	0.089
			(0.007)		(0.008)
White mother	0.016	0.039	0.022	0.029	0.013
			(0.003)		(0.003)
Father in labour force	0.903	0.914	0.012	0.908	0.005
			(0.005)		(0.005)
Father's occ. inc. score	2.748	2.758	0.010	2.747	-0.001
			(0.006)		(0.006)
Father owns home	0.824	0.817	-0.007	0.831	0.007
			(0.007)		(0.007)
Age	8.563	8.624	0.061	8.643	0.080
			(0.104)		(0.106)

Table shows differences in mean characteristics for the initial second generation sample and the subsample that was successfully linked to the 1940 census using the Census Tree. All characteristics are measured in 1910. Column (1) reports mean statistics for the second generation sample prior to linking. Column (2) reports mean statistics for the linked sample without weighting, and Column (3) shows the associated difference in means. Column (4) reports mean statistics for the linked sample with weighting, and Column (6) shows the associated difference in means.

TABLE B.6. Alternative reweighting, first generation sample

	Full sample	Linked samples			
		Unweighted		Weighted	
	Mean	Mean	Diff. in means	Mean	Diff. in means
	(1)	(2)	(3)	(4)	(5)
Length first name	6.338	5.928	-0.411	6.194	-0.145
			(0.029)		(0.032)
Length last name	6.931	6.687	-0.244	6.844	-0.087
			(0.031)		(0.033)
Literate	0.39	0.568	0.179	0.4	0.010
			(0.007)		(0.007)
Speaks English	0.681	0.828	0.147	0.689	0.009
			(0.005)		(0.006)
White spouse	0.018	0.031	0.013	0.024	0.006
			(0.002)		(0.002)
In labour force	0.864	0.891	0.027	0.882	0.018
			(0.004)		(0.004)
Occ. inc. score	2.744	2.746	0.002	2.729	-0.015
			(0.005)		(0.005)
Owns home	0.801	0.795	-0.006	0.811	0.011
			(0.005)		(0.005)
Age	39.817	38.434	-1.382	40.035	0.218
			(0.139)		(0.147)

Table shows differences in mean characteristics for the initial first generation sample and the subsample that was successfully linked to the 1920 census using the Census Tree. All characteristics are measured in 1910. Column (1) reports mean statistics for the first generation sample prior to linking. Column (2) reports mean statistics for the linked sample without weighting, and Column (3) shows the associated difference in means. Column (4) reports mean statistics for the linked sample with weighting using an extended set of covariates, and Column (6) shows the associated difference in means.

TABLE B.7. Alternative reweighting, second generation sample

	Full sample	Linked samples			
		Unweighted		Weighted	
	Mean	Mean	Diff. in means	Mean	Diff. in means
	(1)	(2)	(3)	(4)	(5)
Length first name	6.521	6.134	-0.387	6.294	-0.227
			(0.035)		(0.037)
Length last name	6.95	6.722	-0.227	6.79	-0.160
			(0.039)		(0.041)
Father literate	0.372	0.571	0.199	0.386	0.014
			(0.009)		(0.008)
Father speaks English	0.638	0.796	0.157	0.647	0.009
			(0.007)		(0.008)
White mother	0.016	0.039	0.022	0.026	0.010
			(0.003)		(0.003)
Father in labour force	0.903	0.914	0.012	0.903	0.001
			(0.005)		(0.005)
Father's occ. inc. score	2.748	2.758	0.010	2.736	-0.012
			(0.006)		(0.006)
Father owns home	0.824	0.817	-0.007	0.839	0.015
			(0.007)		(0.006)
Age	8.563	8.624	0.061	8.624	0.061
			(0.104)		(0.106)

Table shows differences in mean characteristics for the initial second generation sample and the subsample that was successfully linked to the 1940 census using the Census Tree. All characteristics are measured in 1910. Column (1) reports mean statistics for the second generation sample prior to linking. Column (2) reports mean statistics for the linked sample without weighting, and Column (3) shows the associated difference in means. Column (4) reports mean statistics for the linked sample with weighting using an extended set of covariates, and Column (6) shows the associated difference in means.

B.3. Outcomes

In this section, I provide definitions of the main outcomes used in my analysis. Where relevant, I also describe how I construct outcomes from underlying IPUMS variables. Variable names in uppercase letters refer to the IPUMS variables; variable names in bold are those used in the paper.

B.3.1. Educational and cultural outcomes

Attended. This outcome is based on attendance records for Carlisle, Chemawa, Hampton and Haskell. I code the outcome as 1 if an individual was linked from attendance records to the 1910 census using the ABE JW algorithm, and 0 otherwise.

Literate. This outcome is based on the IPUMS variable LIT. I code the outcome as 1 if an individual could read and write (LIT = 4), and zero if they could not read and write (LIT = 1). Since I am interested in English literacy, I code the outcome as 0 if an individual could was reported as being literate, but could not speak English (below).

Speaks English. This outcome in based on the IPUMS variable SPEAKENG. I code the outcome as 1 if an individual could speak English (SPEAKENG = 2), and zero if they could not speak English (SPEAKENG = 1).

White spouse. This outcome in based on the (constructed) IPUMS variable RACE_SP. I code the outcome as 1 if an individual (a) was married and (b) their spouse was white (RACE_SP = 1), and 0 if they were married and their spouse was non-white. For this reason, the outcome is only observed for individuals that were married in 1910 / 1940.

Child has 'western' name. This outcome is based on a list of biblical and saint (first) names compiled by Abramitzky, Boustan, and Eriksson (2016). I match these names to the 1910 restricted full count census data, and code the outcome as 1 if an individual's first name appears on the list of names. Otherwise, I code the outcome as 0.

Counted as 'White' in 1920 / 1940. This outcome is based on the IPUMS variable RACE. After linking individuals from the 1910 census to the 1920 / 1940 census, I code the outcome as 1 if their race in 1920 / 1940 was 'White' (RACE = 1), and 0 otherwise.

Finished primary school. This outcome is based on the IPUMS variable EDUCD. I code the outcome as 1 if an individual had completed grade 8 (EDUCD = 26), and 0 otherwise. I exclude missing values (EDUCD = 999).

In urban area. This outcome is based on the IPUMS variable URBAN. Generally, urban areas consist of cities and incorporated places with 2,500+ inhabitants. I code the outcome as 1 if an individual lived in an urban area, (URBAN = 2), and 0 otherwise.

Reported tribe. This outcome is based on the IPUMS variable MBPLSTR, which is only available in the restricted full count data. I clean the underlying variable and identify individuals that report a tribal affiliation. I code the variable as 1 if an individual reported a tribal affiliation, and 0 otherwise.

In 1930 Indian census. This outcome is based on linking between a cross section of Indian censuses around the year 1930, and the 1910 population census. I code the variable as 1 if an individual was successfully linked from 1930 Indian censuses to the 1910 population census, and 0 otherwise.

B.3.2. Economic and labour market outcomes

In labour force. This outcome is based on the IPUMS variable LABFORCE. I code the outcome as 1 if an individual was in the labour force (LABFORCE = 2), and as 0 if they were not in the labour force (LABFORCE = 1).

Employed. This outcome is based on the IPUMS variable EMPSTAT. I code the outcome as 1 if an individual was employed (EMPSTAT = 1) and as 0 if they were not employed (EMPSTAT = 2). For this reason, the outcome is only observed for individuals that were in the labour force in 1910 / 1940.

(Log) Occupational income score. This outcome is based on the (constructed) IPUMS variable OCCSCORE. OCCSCORE assigns a value to each occupation based on median total income (in hundreds of 1950 dollars) of all persons with that occupation in 1950. I use the log of this variable as my outcome.

Owns home. This outcome is based on the IPUMS variable OWNERSHP. I code the outcome as 1 if an individual (household head) owned or were paying off a mortgage on their home (OWNERSHP = 1), and as 0 if they were renting (OWNERSHP = 2).

(Log) Wage income. This outcome is based on the IPUMS variable INCWAGE. INCWAGE reports each respondent's total pre-tax wage and salary income for the previous year. I use the log if 1 + INCWAGE as my outcome.

Appendix C. Additional results

C.1. Accounting for children in boarding schools in 1910

TABLE C.1. Second generation, under 14 years of age in 1910

	Finished primary school	In labour force	Wage income	In urban area	White spouse	'White' in 1940
	(1)	(2)	(3)	(4)	(5)	(6)
Average effect	0.051	-0.062	-0.168	-0.003	-0.276	-0.313
	(0.099)	(0.039)	(0.516)	(0.086)	(0.104)	(0.099)
	[0.900]	[0.135]	[0.886]	[0.915]	[0.049]	[0.015]
Mean dep. var	0.485	0.944	4.719	0.157	0.376	0.331
R2	0.154	0.066	0.141	0.154	0.231	0.230
No. reservations	16	16	16	16	16	16
No. cohorts	14	14	14	14	14	14
Obs.	1,012	1,022	987	1,022	944	1,022

Table shows estimates from Equation 2 in the second generation sample, restricted to individuals that were under 14 years of age in 1910. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Has white spouse' is an indicator for having a white spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White' is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. All regressions include reservation fixed effects, cohort fixed effects, and father's cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

C.2. Changes in racial classification by nonmovers

TABLE C.2. First generation, nonmovers

	'White' in 1920		
	Full sample	Same state	Same county
	(1)	(2)	(3)
Average effect	0.093	0.081	0.061
	(0.034)	(0.032)	(0.031)
	[0.006]	[0.007]	[0.006]
Mean dep. var	0.144	0.121	0.091
R2	0.115	0.117	0.121
No. reservations	38	34	29
No. cohorts	21	21	20
Obs.	2,805	2,558	1,833

Table shows estimates from Equation 1 in the first generation sample that was linked to the 1920 census using the Census Tree. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample in Column (2) is restricted to individuals from the first generation sample that were living in the same state in 1910 and 1920. The sample in Column (3) is further restricted to individuals that were living in the same county in 1910 and 1920. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White'. Observations are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

TABLE C.3. Second generation, nonmovers

	'White' in 1940		
	Full sample	Same state	Same county
	(1)	(2)	(3)
Average effect	-0.298	-0.283	-0.115
	(0.101)	(0.121)	(0.124)
	[0.021]	[0.080]	[0.083]
Mean dep. var	0.343	0.302	0.330
R2	0.229	0.281	0.352
No. reservations	20	18	10
No. cohorts	14	14	14
Obs.	1,551	1,282	573

Table shows estimates from Equation 2 in the second generation sample. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. The sample in Column (2) is restricted to individuals from the second generation sample that were living in the same state in 1910 and 1940. The sample in Column (3) is further restricted to individuals that were living in the same county in 1910 and 1940 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White'. Observations are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

C.3. Effects on women

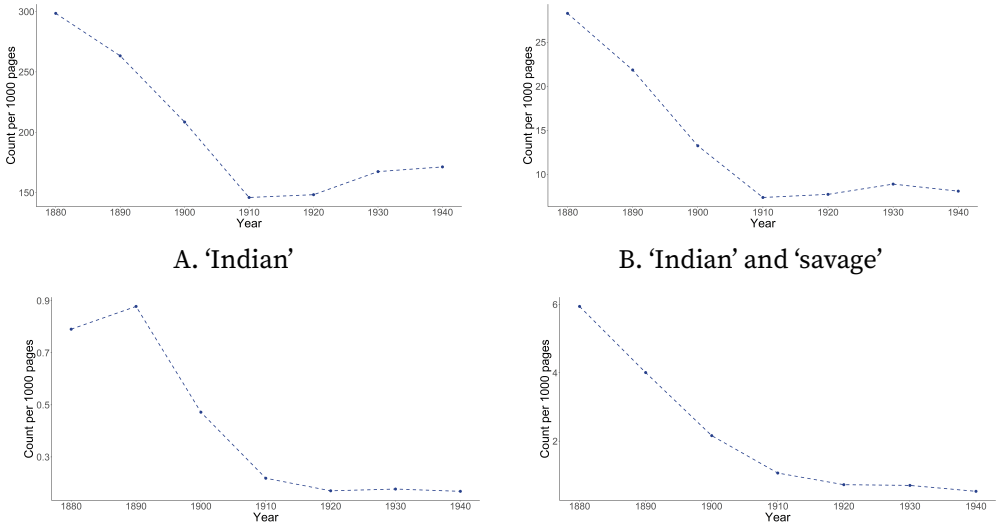
TABLE C.4. No effects on women in first generation

	Literate	Speaks English	White spouse	'White' in 1920
	(1)	(2)	(3)	(4)
Average effect	-0.001	0.030	0.003	-0.009
	(0.036)	(0.034)	(0.014)	(0.050)
	[0.295]	[0.515]	[0.950]	[0.802]
Mean dep. var	0.229	0.544	0.024	0.114
R2	0.380	0.377	0.076	0.177
No. reservations	64	64	64	29
No. cohorts	21	21	21	21
Obs.	8,974	9,047	9,040	1,884

Table shows estimates from Equation 1 in the first generation sample of women. The first generation sample consists of Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were spouses and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency.The sample in Column (5) is restricted to individuals from the first generation sample that were linked to the 1920 census using the Census Tree. All other columns are estimated on the full first generation sample. 'Literate' is an indicator for being able to read and write (measured in 1910), based on the IPUMS variable LIT. 'Speaks English' is an indicator for being able to speak English (measured in 1910), based on the IPUMS variable SPEAKENG. 'Has white spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

C.4. Public attitudes towards Native Americans

FIGURE C.1. Public attitudes towards Native Americans, 1880 to 1940



A. 'Indian'

B. 'Indian' and 'savage'

C. 'Indian' and 'uncivilized'

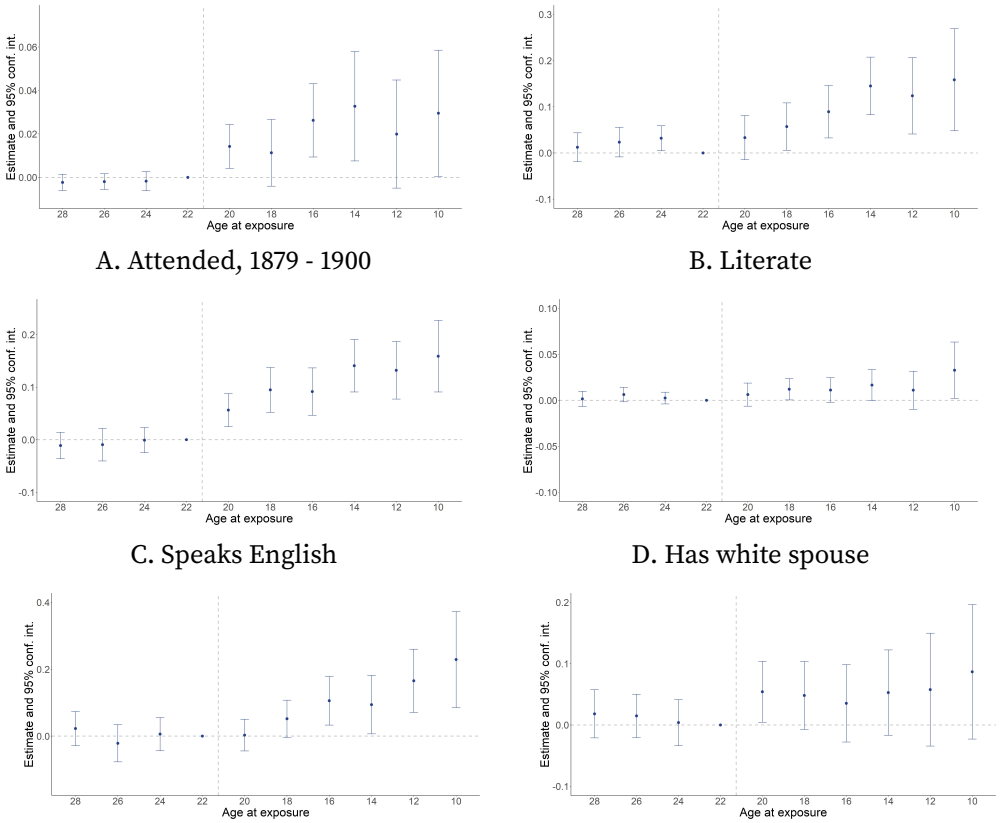
D. 'Indian' and 'filthy'

Figure shows the relative frequencies (per 1000 pages) of words or combinations of words related to Native Americans obtained from the website Newspapers.com. 'Indian' and 'X' indicates that both the word 'Indian' and the word 'X' appeared on the same page. Annual frequencies are summed by decade, and normalised by the total number of 'pages' in each decade.

Appendix D. Robustness checks

D.1. Robust TWFE estimation

FIGURE D.1. First generation outcomes, Sun and Abraham (2021) estimator



A. Attended, 1879 - 1900

B. Literate

C. Speaks English

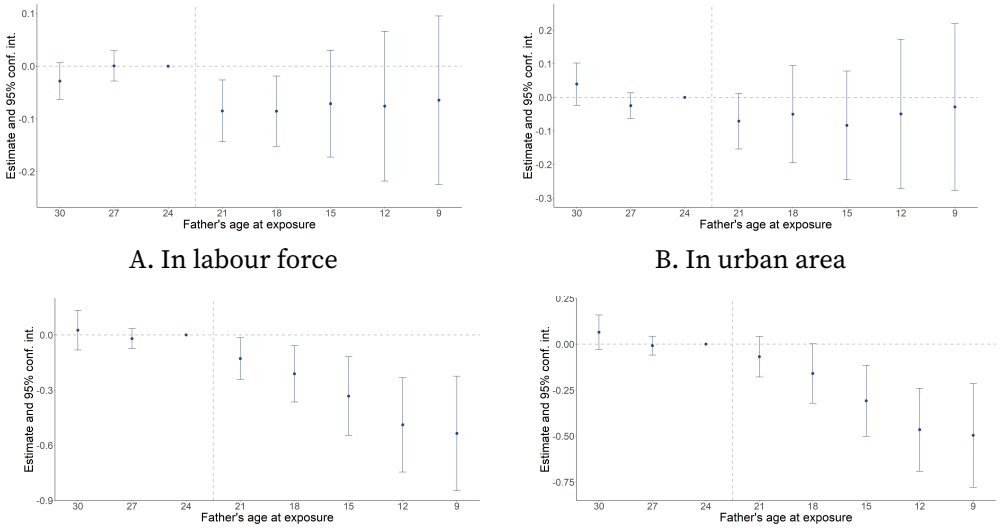
D. Has white spouse

E. Child has 'western' name

F. Counted as 'White' in 1920

Figure shows estimates from Equation 1 in the first generation sample using the estimator proposed by Sun and Abraham (2021). The last-treated groups (1899) serve as the control group. This consists of: Yuma (California), Colorado River (Arizona), Uncompahgre (Utah), Saint Regis (New York), Oto (Oklahoma), Quileute and Nisqualli (both Washington) and La Pointe (Wisconsin). In Panel (e) I combine reservations treated in 1898 and 1899 into a single last-treated group. The additional reservations are: Fort Bidwell (California), Mille Lac and Red Lake (both Minneosta), Tonawanda (New York) and Crow Creek (South Dakota). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

FIGURE D.2. Second generation outcomes, Sun and Abraham (2021) estimator



A. In labour force

B. In urban area

C. Has white spouse

D. Counted as 'White' in 1940

Figure shows estimates from Equation 2 using the estimator proposed by Sun and Abraham (2021). The last-treated groups (1899) serve as the control group. This consists of: Yuma (California), Colorado River (Arizona), Uncompahgre (Utah), Saint Regis (New York), Oto (Oklahoma), Quileute and Nisqualli (both Washington) and La Pointe (Wisconsin). All regressions include reservation fixed effects, cohort fixed effects, household head cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with household head cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

D.2. Pre-trends

Figure 6 and Figure 8 in the main text provide graphical evidence in favour of the parallel trends assumption. In this section, I provide additional evidence in favour of this identification assumption using daignostics and methods proposed by Roth (2022) and Rambachan and Roth (2023).

Recent work has highlighted pitfalls of standard 'tests' for pre-trends conducted in applied economic research (Roth 2022). Roth (2022) raises two concerns. Firstly, standard tests for pre-trends may have low power, and therefore fail to detect pre-trends that induce (potentially large) biases in post-treatment estimates. To address this concern, I follow Roth (2022), and, for each of my main outcomes, calculate a pre-trend that would be detected 80 per cent of the time ('hypothesised' trends). In Table D.1 I show that the observed pre-treatment coefficients are generally far more likely to have occurred under parallel trends than under the hypothesised trends (Column (1)). I also examine the magnitudes of my main estimates (Column (2)) relative to average biases implied by the hypothesised trends (Column (3)). The average biases are, with one exception, smaller in magnitude than the estimated effects. The takeaway from these results is that I am likely to detect pre-trends that lead to a meaningful amount of bias in the post-treatment periods.

TABLE D.1. Pre-trend diagnostics from Roth (2022)

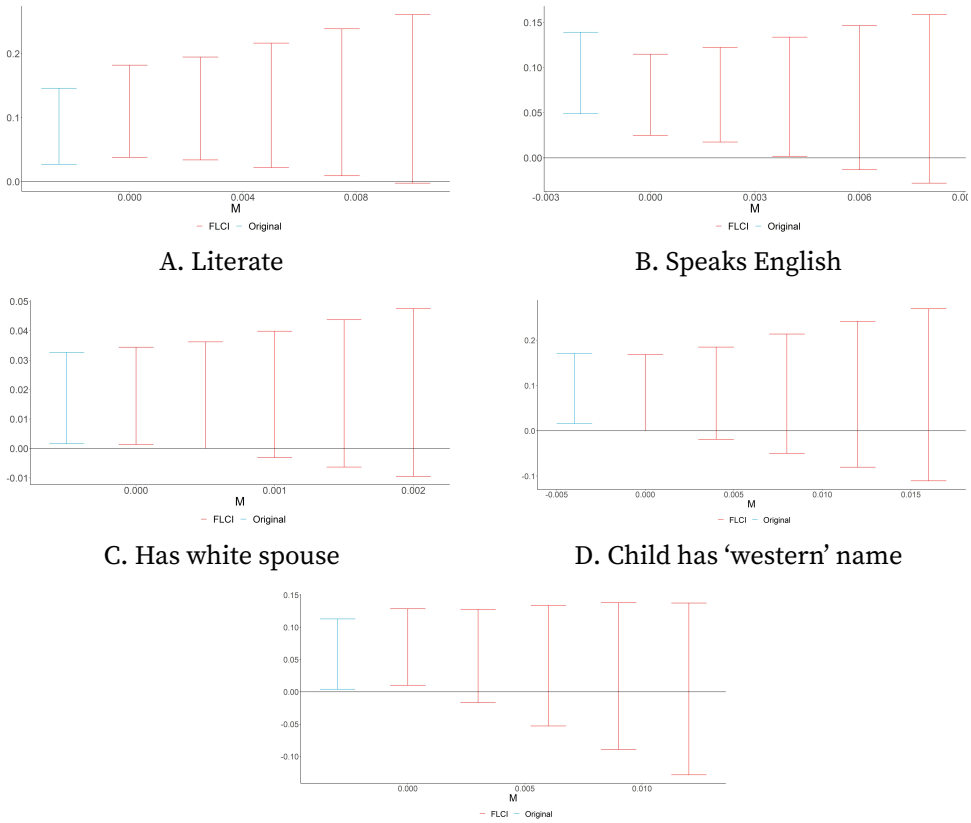
	Likelihood ratio	Estimate	Implied bias
	(1)	(2)	(3)
Panel (a), First gen.			
Literate	0.000	0.111	0.070
Speaks English	1.032	0.122	0.056
White spouse	0.024	0.022	0.017
Child has western name	0.218	0.123	0.108
White in 1920	0.124	0.062	0.084
Panel (b), Second gen.			
White spouse, 1940	0.156	-0.271	-0.368
White, 1940	0.145	-0.300	-0.319

Table shows diagnostics proposed by Roth (2022) using a hypothesised linear trend that would be detected 80 per cent of the time. Panel (a) shows results with respect to the first generation sample (measured in 1910 and 1920), and Panel (b) shows results with respect to the second generation sample (measured in 1940). 'Likelihood ratio' indicates the relative likelihood of observing the pre-treatment coefficients actually observed under the hypothesised (linear) trend as opposed to parallel trends. 'Estimate' is simple average of post-treatment coefficients. 'Bias' is the simple average of the hypothesised trend over the post-treatment period.

A related concern is that *post*-treatment differences in trends may lead to biased estimates. To assess the robustness of my main results to this concern, I adopt the methodology proposed by Rambachan and Roth (2023). This procedure involves estimating 95 per cent confidence sets that allow for per-period deviations from parallel trends, represented by M. Since there is generally an increasing profile of effects over the first four post-treatment periods, I consider inference on the average effect over these periods.

The results of this exercise are shown in Figure D.3 for the first generation and Figure D.4 for the second generation. My main estimates are generally robust to linear trends ($M = 0$), and in most cases to small non-linear deviations.

FIGURE D.3. Sensitivity analysis from Rambachan and Roth (2023), first generation



A. Literate

B. Speaks English

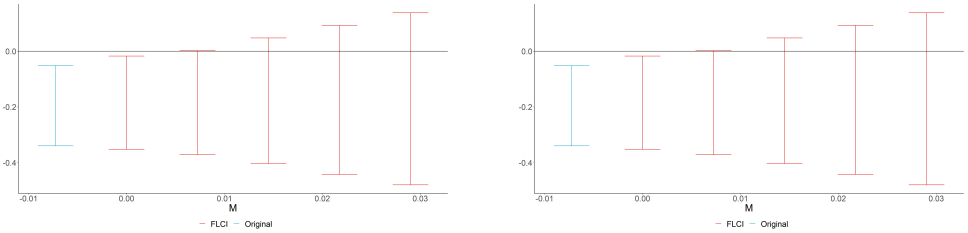
C. Has white spouse

D. Child has 'western' name

E. Counted as 'White' in 1920

Figure shows sensitivity of main results to violations of the parallel trends assumption using the methodology proposed by Rambachan and Roth (2023). 'Original' represents the 95 per cent confidence interval from estimates in the main text. 'FLCI' represent 95 per cent confidence intervals when allowing for per-period violations of parallel trends of up to M, where M = 0 indicates a linear violation parallel trends. Standard errors are clustered at the reservation-level. Figures are generated using the HonestDiD package.

FIGURE D.4. Sensitivity analysis from Rambachan and Roth (2023), second generation



A. Has white spouse

B. Counted as 'White' in 1940

Figure shows sensitivity of main results to violations of the parallel trends assumption using the methodology proposed by Rambachan and Roth (2023). 'Original' represents the 95 per cent confidence interval from estimates in the main text. 'FLCI' represent 95 per cent confidence intervals when allowing for per-period violations of parallel trends of up to M, where M = 0 indicates a linear violation parallel trends. Standard errors are clustered at the reservation-level. Figures are generated using the HonestDiD package.

D.3. Selection into the linked 1940 sample

In my main results, I find that exposure to off-reservation schools led to assimilation in the first generation, but that these effects reversed in the second generation. One concern is that these results may be driven by selection into the linked 1940 sample. For example, linking algorithms are more likely to link individuals with unique names, which may be correlated with their propensity to adopt identifiably 'Indian' traits. Among other things, these individuals might have been socialised differently in their households.

To address this concern, I examine whether there was also assimilation among *first* generation households from which the second generation sample is drawn. Finding effects in this restricted first generation sample that are similar to effects in the full first generation sample would allay concerns that the second generation sample was drawn from an atypical set of first generation households. The results from this exercise are shown in Table D.2. With the caveat that sample sizes are small, the estimated effects on assimilation outcomes are present, and if anything, stronger in the restricted sample. This suggests that 'negative selection' into the linked 1940 sample (with respect to assimilation outcomes) is unlikely to be driving the reversal.

TABLE D.2. First gen. assimilation effects still present in restricted sample

	Speaks English	White spouse	'White' in 1920
	(1)	(2)	(3)
Average effect	0.198	0.080	0.256
	(0.086)	(0.024)	(0.088)
	[0.028]	[0.015]	[0.010]
Mean dep. var	0.738	0.025	0.164
R2	0.384	0.162	0.160
No. reservations	22	22	14
No. cohorts	17	17	17
Obs.	1,317	1,231	677

Table shows estimates from Equation 1 in the first generation sample, where the sample is restricted to households from which the second generation sample is drawn. The full first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The sample in Column (3) is further restricted to individuals from the first generation sample that were linked to the 1920 census using the Census Tree. 'Speaks English' is an indicator for being able to speak English (measured in 1910), based on the IPUMS variable SPEAKENG. 'Has white spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with cohort fixed effects. Observations used in regression in Column (3) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

D.4. Alternative linking methods

TABLE D.3. Linking method: ABE-NYSIIS

	First generation	Second generation	
	'White' in 1920	'White' in 1940	White spouse in 1940
	(1)	(2)	(3)
Average effect	0.187	-0.332	-0.230
	(0.085)	(0.219)	(0.229)
Mean dep. var	0.177	0.366	0.411
R2	0.196	0.244	0.218
No. reservations	13	13	13
No. cohorts	20	14	14
Obs.	498	561	508

Table shows estimates from Equation 1 in the first generation sample and Equation 2 in the second generation sample.. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. Both samples are restricted to individuals linked to 1920 (first generation) or 1940 (second generation) using the ABE-NYSIIS algorithm. 'White spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked across census waves using the ABE-NYSIIS algorithm, and their race was reported as 'White' in the later census. All regressions include reservation fixed effects and cohort fixed effects (2-year bins in Column (1), and 3-year bins in Column (2) and Column (3)). Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

TABLE D.4. Linking method: User-generated links

	First generation	Second generation	
	'White' in 1920	'White' in 1940	White spouse in 1940
	(1)	(2)	(3)
Average effect	0.280	-0.438	-0.485
	(0.081)	(0.119)	(0.097)
Mean dep. var	0.191	0.299	0.337
R2	0.197	0.179	0.238
No. reservations	19	6	6
No. cohorts	20	13	13
Obs.	982	395	368

Table shows estimates from Equation 1 in the first generation sample and Equation 2 in the second generation sample.. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. Both samples are restricted to individuals linked to 1920 (first generation) or 1940 (second generation) using user-generated links from FamilySearch.org. 'White spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked across census waves, and their race was reported as 'White' in the later census. All regressions include reservation fixed effects and cohort fixed effects (2-year bins in Column (1), and 3-year bins in Column (2) and Column (3)). Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

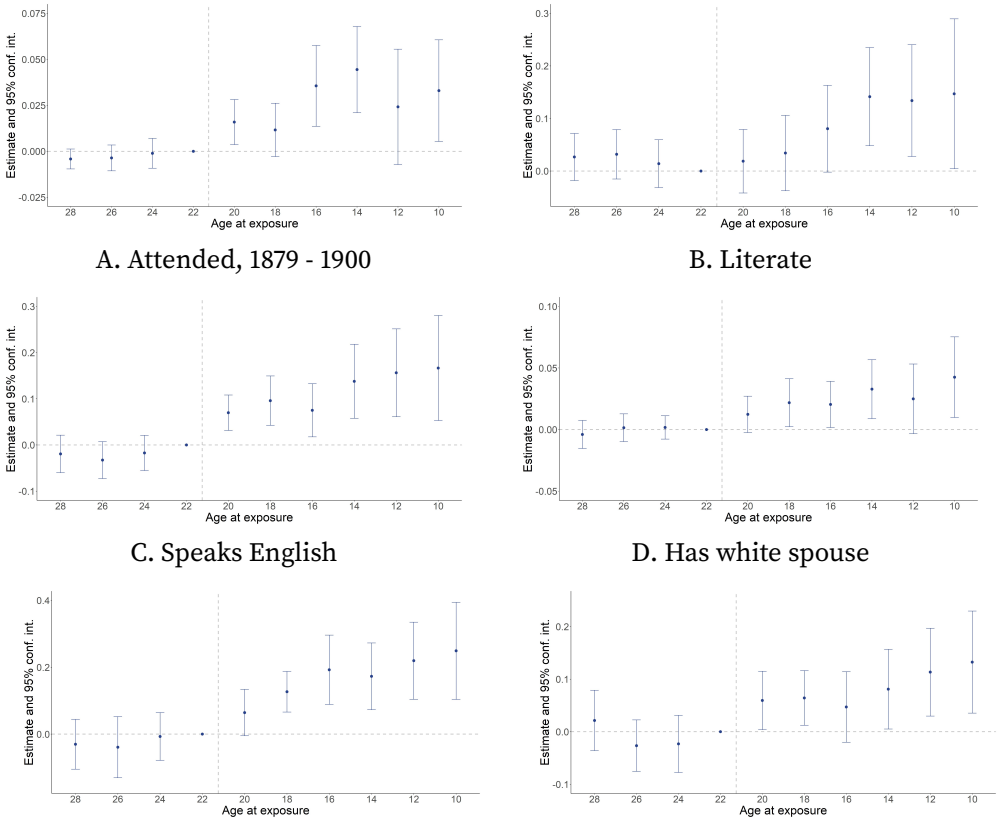
TABLE D.5. Linking method: FamilySearch 'hints'

	First generation	Second generation	
	'White' in 1920	'White' in 1940	White spouse in 1940
	(1)	(2)	(3)
Average effect	0.058	-0.224	-0.198
	(0.029)	(0.090)	(0.118)
Mean dep. var	0.142	0.309	0.359
R2	0.166	0.273	0.244
No. reservations	23	10	10
No. cohorts	20	14	14
Obs.	1,469	559	517

Table shows estimates from Equation 1 in the first generation sample and Equation 2 in the second generation sample.. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. Both samples are restricted to individuals linked to 1920 (first generation) or 1940 (second generation) using 'hints' from FamilySearch.org. 'White spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked across census waves, and their race was reported as 'White' in the later census. All regressions include reservation fixed effects and cohort fixed effects (2-year bins in Column (1), and 3-year bins in Column (2) and Column (3)). Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

D.5. Secular trends

FIGURE D.5. First generation, controlling for regional trends



A. Attended, 1879 - 1900

B. Literate

C. Speaks English

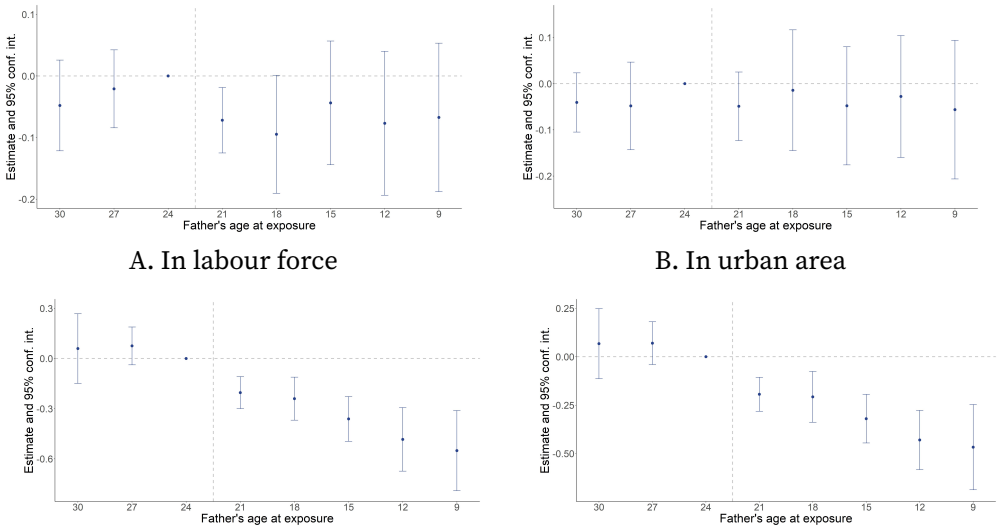
D. Has white spouse

E. Child has 'western' name

F. Counted as 'White' in 1920

Figure shows estimates from Equation 1 with the addition of region-by-cohort controls. Specifically, all regressions include 'Divisions' taken from the IPUMS variable REGION interacted with cohort fixed effects, in addition to baseline controls. All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

FIGURE D.6. Second generation, controlling for regional trends



A. In labour force

B. In urban area

C. Has white spouse

D. Counted as 'White' in 1940

Figure shows estimates from Equation 2 with the addition of region-by-cohort controls. Specifically, all regressions include 'Divisions' taken from the IPUMS variable REGION interacted with (1910 father's) cohort fixed effects, in addition to baseline controls. All regressions include reservation fixed effects, cohort fixed effects, household head cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with (1910 father's) cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

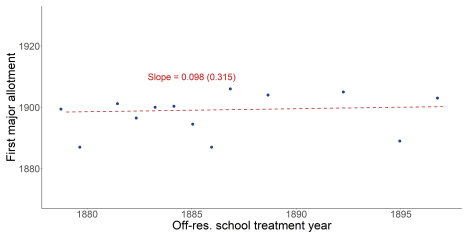
D.6. Allotment

In order to promote private property ownership among Native Americans, as well as open surplus reservation lands to settlers, the US Government initiated a land allotment policy with the Dawes Act (1887). The Dawes Act enabled the Indian Office to survey and allot reservation lands to individual Native Americans associated with the reservation (Dippel, Frye, and Leonard 2023). Alloted lands were initially to be held in trust (i.e., neither transferable nor alienable) for a period of 25 years. After this period, allottees were to be granted fee simple rights, enabling them to sell their land. These conditions underwent an important change in the early-1900s, with the Burke Act (1906). The Burke Act enabled authorities to immediately grant fee simple rights to Native Americans that were deemed 'competent and capable', without regard to the 25-year trust period (Prucha 1984, pp. 875-876). The process was ended by the 1934 Indian Reorganization Act. By this time, some reservations had been allotted (with allotted lands converted into fee simple), other reservations remained allotted in trust, and others were never allotted (Leonard, Parker, and Anderson 2020).

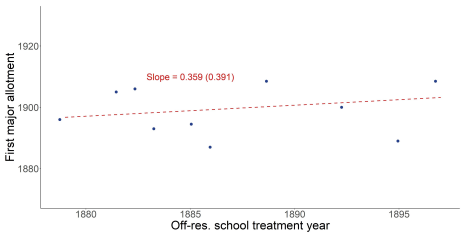
Given that the allotment process took place during the roll-out of the off-reservation school system, one concern is that the effects I find are partly driven by allotments. I address this concern in two ways. Firstly, I digitise information on 'major allotments' of reservations from a 1935 report by the Indian Office (Office of Indian Affairs 1935). Figure D.7 shows binned scatterplots of off-reservation school treatment years and (first) major allotment years in the first generation samples (1910 and 1920) and second generation sample (1940). In all cases, the linear fit lines indicate a weak and non-statistically significant relationship between off-reservation school treatment years and allotment years. This is particularly so in the 1910 first generation sample (Panel (a)) and the 1940 second generation sample (Panel (c)).

Secondly, I re-estimate Equation 1 and Equation 2 on a restricted sample of reservations that had not been allotted by the time outcomes were measured. Table D.6 shows that my main conclusions with respect to educational and cultural outcomes continue to hold in this restricted sample. In Table D.7, I continue to find no meaningful effects on labour market or economic outcomes in the first generation sample. Finally, Table D.8 shows results in the second generation sample; since outcomes are measured in 1940, I restrict attention to reservations that were *never* allotted. The estimated effects on intermarriage and classification as 'White' in 1940 are in line with my main results, though imprecisely estimated given the small sample size.

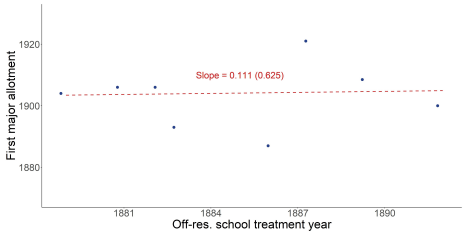
FIGURE D.7. Timing of allotments unrelated to off-res. school treatment years



A. First generation, 1910



B. First generation, 1920



C. Second generation, 1940

Figure shows the timing of first 'major' allotment years relative to the timing of off-reservation school treatment years.
Source: Own calculations using off-reservation school treatment years and 'major' allotment years reported in Office of Indian Affairs (1935).

TABLE D.6. First generation, non-allotted reservations

	Attended	Literate	Speaks English	White spouse	Child has western name	'White' in 1920
	(1)	(2)	(3)	(4)	(5)	(6)
Average effect	0.037	0.173	0.099	0.018	0.127	0.119
	(0.016)	(0.054)	(0.033)	(0.010)	(0.061)	(0.042)
	[0.002]	[0.060]	[0.008]	[0.244]	[0.041]	[0.034]
Mean dep. var	0.015	0.307	0.605	0.012	0.450	0.149
R2	0.125	0.357	0.399	0.178	0.307	0.121
No. reservations	34	40	40	40	27	23
No. cohorts	21	21	21	21	20	20
Obs.	5,668	6,416	6,482	5,683	3,321	1,555

Table shows estimates from Equation 1 in the first generation sample, restricted to reservations not allotted by 1910 (Column (1) to Column (5)) and reservations not allotted by 1920 (Column (6)). The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. The sample in Column (1) is restricted to individuals from reservations that were treated by Carlisle, Chemawa, Hampton, and Haskell. The sample in Column (5) is restricted to individuals from the first generation sample that were linked to the 1920 census using the Census Tree. Columns (2) to (4) are estimated on the full first generation sample. 'Attended, 1879 - 1900' is an indicator for appearing in the attendance records of Carlisle, Chemawa, Hampton or Haskell, and being linked to the 1910 census. 'Literate' is an indicator for being able to read and write (measured in 1910), based on the IPUMS variable LIT. 'Speaks English' is an indicator for being able to speak English (measured in 1910), based on the IPUMS variable SPEAKENG. 'Has white spouse' is an indicator for having a white spouse (measured in 1910), based on the IPUMS variable RACE_SP. 'Child has western name' is an indicator equal to 1 if the eldest male child in the household's first name appears in the list of saint names and biblical names from Abramitzky, Boustan, and Eriksson (2016) (measured in 1910). 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

TABLE D.7. First generation, non-allotted reservations

	In labour force	Employed	Occ. income score	Owns home
	(1)	(2)	(3)	(4)
Average effect	-0.008	0.010	-0.077	-0.096
	(0.028)	(0.025)	(0.025)	(0.043)
	[0.447]	[0.431]	[0.062]	[0.209]
Mean dep. var	0.876	0.934	2.734	0.795
R2	0.300	0.121	0.227	0.184
No. reservations	40	40	40	40
No. cohorts	21	21	21	21
Obs.	6,482	5,013	5,098	6,482

Table shows estimates from Equation 1 in the first generation sample, restricted to reservations not allotted by 1910. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1910. 'In lab. force' is an indicator for being in the labour force in 1910, based on the IPUMS variable LABFORCE. 'Employed' is an indicator for being employed, conditional on being in the labour force, based on the IPUMS variable 'EMPSTAT'. An individual was considered to be employed if they were at work on 15 April 1910. 'Occ income score' is the log of the IPUMS variable OCCSCORE. 'Owns home' is an indicator for owning one's own home (rather than renting), based on the IPUMS variable OWNERSHP. All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

TABLE D.8. Second generation, non-allotted reservations

	Finished primary school	In labour force	Wage income	In urban area	White spouse	'White' in 1940
	(1)	(2)	(3)	(4)	(5)	(6)
Average effect	-0.000	0.048	-0.651	0.041	-0.226	-0.261
	(0.120)	(0.106)	(0.712)	(0.102)	(0.130)	(0.155)
	[0.359]	[0.406]	[0.047]	[0.094]	[0.328]	[0.359]
Mean dep. var	0.469	0.957	4.775	0.232	0.574	0.541
R2	0.237	0.152	0.210	0.172	0.206	0.172
No. reservations	7	7	7	7	7	7
No. cohorts	13	13	13	13	13	13
Obs.	486	495	476	495	443	495

Table shows estimates from Equation 2 in the second generation sample, restricted to un-allotted reservations. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Finished primary school' is an indicator for having completed primary school, based on the IPUMS variable EDUCD. '(Log) Wage income' is the log of wage income, based on the IPUMS variable INCWAGE. 'In urban area' is an indicator for residing in an urban area in 1940, based on the IPUMS variable URBAN. 'Has white spouse' is an indicator for having a white spouse in 1940, based on the IPUMS variable RACE_SP. 'Counted as 'White' is an indicator equal to 1 if an individual was successfully linked to the 1940 census using the Census Tree, and their race was reported as 'White'. All regressions include reservation fixed effects, cohort fixed effects, father's cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with father's cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. p-values from the wild cluster bootstrap are reported in brackets.

D.7. Alternative sample windows

FIGURE D.8. First generation outcomes, extended sample

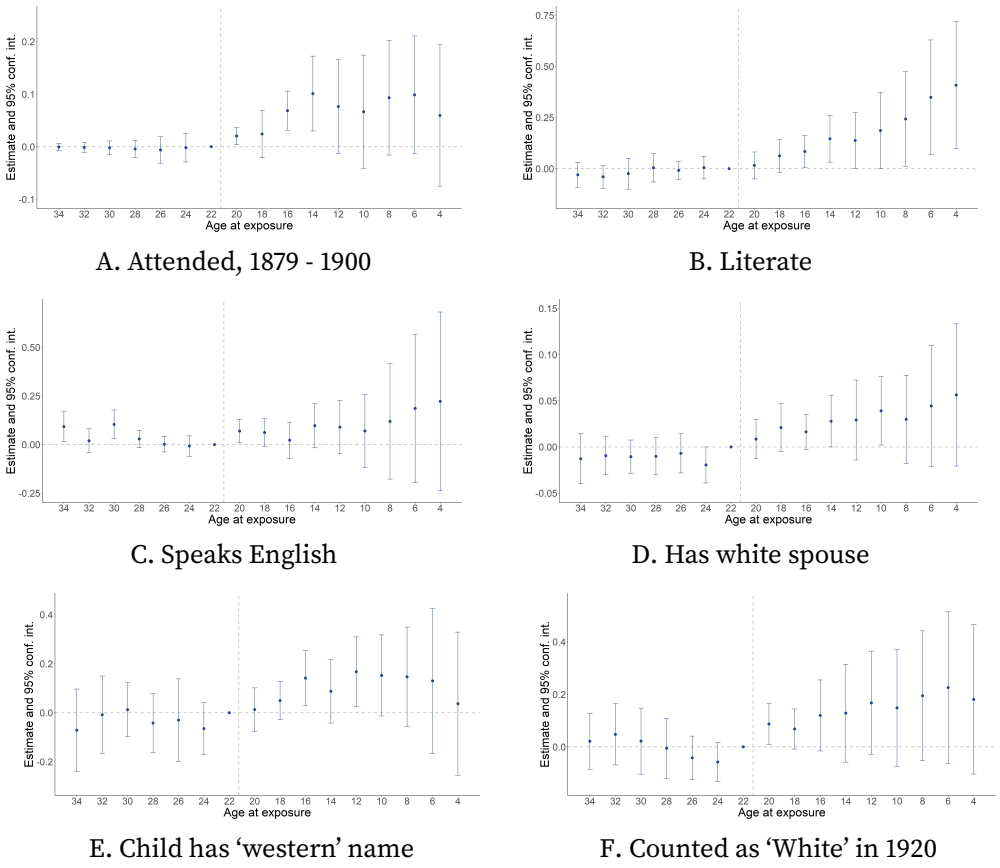
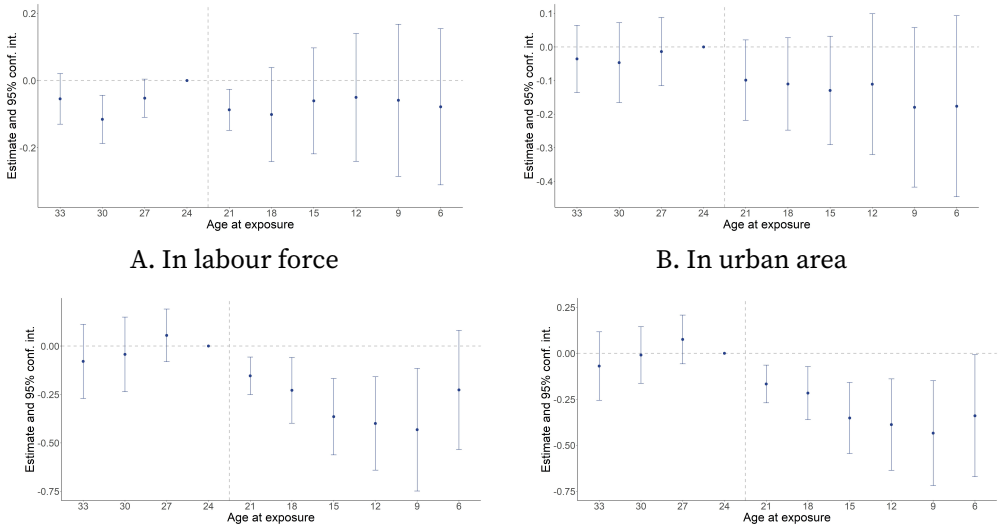


Figure shows estimates from Equation 1 in the first generation sample. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

FIGURE D.9. Second generation outcomes, extended sample



A. In labour force

B. In urban area

C. Has white spouse

D. Counted as 'White' in 1940

Figure shows estimates from Equation 2 in the second generation sample. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. All regressions include reservation fixed effects, cohort fixed effects, household head cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with household head cohort fixed effects. Observations in all regressions are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level. Please see main text for sample and variable descriptions.

D.8. Alternative reweighting schemes

TABLE D.9. Reweighting does not change results, first gen.

	'White' in 1920		
	Unweighted	Reweighted, baseline	Reweighted, extended
	(1)	(2)	(3)
Average effect	0.095	0.093	0.093
	(0.033)	(0.034)	(0.038)
Mean dep. var	0.144	0.144	0.144
R2	0.104	0.115	0.115
No. reservations	38	38	38
No. cohorts	21	21	21
Obs.	2,805	2,805	2,805

Table shows estimates from Equation 1 in the first generation sample with different reweighting schemes. No weights are used in the regression in Column (1). In Column (2), weights are obtained from a probit regression using the following covariates: length of first name, length of surname, state of birth, year of birth, and a quadratic in age. In Column (2), weights are obtained from a probit regression using the previous covariates plus factor variables for: literacy, speaking English, and marital status. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910. The sample excludes all individuals matched to reservations under the Union agency. 'Counted as 'White"is an indicator equal to 1 if an individual was successfully linked to the 1920 census using the Census Tree, and their race was reported as 'White' (measured in 1920). All regressions include reservation fixed effects, cohort fixed effects (2-year bins), and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 10 bins) interacted with cohort fixed effects. Observations used in regression in Column (5) are weighted by the inverse of the probability of being linked. Standard errors are clustered at the reservation-level.

TABLE D.10. Reweighting does not change results, second gen.

	White spouse		
	Unweighted	Reweighted, baseline	Reweighted, extended
	(1)	(2)	(3)
Average effect	-0.217	-0.271	-0.280
	(0.088)	(0.094)	(0.092)
Mean dep. var	0.383	0.383	0.383
R2	0.219	0.238	0.248
No. reservations	20	20	20
No. cohorts	14	14	14
Obs.	1,411	1,411	1,411

Table shows estimates from Equation 2 in the second generation sample with different reweighting schemes. No weights are used in the regression in Column (1). In Column (2), weights are obtained from a probit regression using the following covariates: length of first name, length of surname, state of birth, year of birth, and a quadratic in age. In Column (2), weights are obtained from a probit regression using the previous covariates plus factor variables for: father's literacy, father's English proficiency, and and father's marital status. The second generation sample consists male children from first generation households that were linked to the 1940 census using the Census Tree, and that were household heads in 1940. The first generation sample consists of male Native Americans matched to reservations that were treated by an off-reservation school between 1879 and 1900, that were household heads and aged between 19 and 60 in 1910; the sample excludes all individuals matched to reservations under the Union agency. All outcomes are measured in 1940. 'Has white spouse' is an indicator for having a white spouse in 1940, based on the IPUMS variable RACE_SP. All regressions include reservation fixed effects, cohort fixed effects, father's cohort fixed effects, and the agency-level share of individuals that were literate (averaged 1878 - 1879, in 4 bins) interacted with father's cohort fixed effects. Standard errors are clustered at the reservation-level.