Language of Instruction, Integration, and Identity: Evidence from Sweden's Tornedalians

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Abstract

How does language of instruction shape social integration and national identity? I study the introduction of primary schools using Swedish, rather than Finnish, as the language of instruction in late nineteenth-century Tornedalen, a predominantly Finnish-speaking region of Sweden bordering Finland. Using linked censuses and variation across villages and cohorts, I estimate the effects of exposure to Swedish-only schooling. Exposure increased social integration, as reflected in marriage patterns and reduced residential segregation. At the same time, changes in naming patterns and military employment indicate that exposure weakened Swedish identity and strengthened Finnish identity. I find no effects on income, further education, or language use at home. Evidence suggests that intergroup contact promoted integration and Swedish-only instruction made linguistic differences more salient, thereby shaping identity. Nation-building through education can thus promote social integration while weakening identification with the state. At the community level, school openings increased the adoption of Swedish names and decreased Finnish language use at home, revealing a broader cultural shift toward Swedish despite the contrasting identity responses among individuals exposed to schooling.

Keywords: nation-building, language, education, identity, integration

JEL codes: J15, Z13, I28

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1. INTRODUCTION

How does language of instruction shape social integration and national identity? It has been estimated that around 40 percent of children globally lack access to education in their first language, despite evidence that such instruction facilitates learning (UNESCO, 2025). The choice of language of instruction, however, is not only pedagogical or administrative; it is often political and used as a tool of nation-building, with the aim of strengthening social cohesion by improving intergroup relations and fostering a shared sense of national belonging (Rohner and Zhuravskaya, 2023). Such policies have been used in diverse settings, from the spread of French through mass schooling in nineteenth-century France to the imposition of Russian-language instruction in occupied Ukraine today. Existing evidence points to both successful nation-building and countervailing responses (Fouka, 2020; Blanc and Kubo, 2025).

In this paper, I study the effects of a language-of-instruction reform on social integration and national identity in Tornedalen, Sweden, at the turn of the twentieth century. Tornedalen is a region of Sweden bordering Finland with a distinct regional identity. Prior to the reform, Finnish was the main language of instruction, reflecting its status as the language spoken by most of the population, while Swedish-speaking children received separate instruction. As part of a broader nation-building effort aimed at assimilating the region economically, socially and culturally, the state offered funded schools, with funding conditional on using Swedish as the language of instruction; Finnish was not to be used.

The setting is particularly well suited to studying these questions because multiple full-count population censuses are available around the introduction of the Swedish-only schools. I link individuals across these censuses at high rates, allowing me to follow them from childhood into adulthood. The censuses also contain highly granular information on place of residence, which I geocode to pinpoint exposure to the schools and trace changes in small local communities over time. Together, these features allow me to study both the effects of individual exposure to Swedish-only schooling and the broader consequences of school openings for the communities in which they were introduced.

In my main analysis, I identify the causal effect of individual exposure to Swedish-only schooling using a difference-in-differences design. The design leverages variation within cohorts by comparing villages that received a Swedish-only school with those that did not, and within villages by comparing cohorts young enough to be exposed with older cohorts who were too old to be directly affected. I use propensity-score matching to select comparable villages without a Swedish-only school.

Exposure to Swedish-only schooling increased social integration within the region, as measured by

marriage patterns and residential segregation in adulthood. Treated individuals are about 6 percentage points less likely to marry individuals born in Finland, corresponding to approximately 67% of the unconditional mean (9%), and about 8 percentage points more likely to marry individuals born in Tornedalen, corresponding to approximately 9% of the unconditional mean (85%). These findings suggest greater social integration within Tornedalen relative to cross-border ties with Finland. An alternative surname-based measure of the partner's ethnolinguistic background further corroborates that these changes in marriage patterns reflect increased intermarriage within the region. Treated individuals are also more likely to live in ethnolinguistically diverse villages, as measured by a fractionalization index (Alesina et al., 2003), rather than villages dominated by a single ethnolinguistic group. The index ranges from 0 to 0.5, and exposure increases it by about 0.02 points, corresponding to approximately 16% of the unconditional mean (13%) and a 2 percentage-point increase in the probability that two randomly selected residents belong to different ethnolinguistic groups. This effect is unlikely to be driven by migration to more ethnolinguistically diverse villages, as treated individuals are more likely to remain in the village where they lived before treatment.

Increased social integration was not coupled with increased expressions of Swedish identity. Instead, I find increased expression of Finnish identity and decreased expression of Swedish identity on average. My main measure of national identity is naming choices.[1] Working in the military is used as an alternative proxy for national identity.[2] Treated individuals give their children less Swedish and more Finnish names, measured using a Cultural Name Score (CNS) based on the relative frequency of names across groups (Bazzi et al., 2026). Exposure increases the Finnish CNS by about 1.5 points on a 0–100 scale, corresponding to about 7.5% of the unconditional mean, and decreases the Swedish CNS by 1.2 points (6%). In addition, treated individuals are less likely to work in the military, consistent with weaker attachment to the Swedish nation-state.

Because the Swedish-only schools were introduced on top of an existing school system, the relevant counterfactual is first-language education with more limited access to schooling. This is a policy-relevant margin as it captures the effect of government intervention relative to the locally organized system that would otherwise have prevailed, rather than the effect of education alone or a pure change in language of instruction. At the same time, the bundled nature of the intervention makes it important to examine

[1] Naming choices are a common proxy for cultural change and have been used to study individualism (Bazzi et al., 2020), collectivism (Knudsen, 2025), religiosity (Andersen and Bentzen, 2025), as well as cultural assimilation and ethnic identification (Fryer and Levitt, 2004; Fouka, 2020; Gagliarducci and Tabellini, 2025).

[2] Working in the military serves as a proxy for national identity, reflecting attachment to the nation. Different forms of military participation have been used to proxy for national identity (Fouka, 2020; Blanc and Kubo, 2025; Qian and Tabellini, 2026; Bazzi et al., 2026), though these studies focus on voluntary participation.

how the reform affected educational uptake, labor-market outcomes, and language. I measure language using language use at home, proxied by whether an individual was classified as "Finnish" by census enumerators, a classification largely based on the language spoken at home.

I find no statistically significant average effect on educational uptake. However, in villages where pre-treatment educational supply was limited, the opening of a Swedish-only school increased primary-school uptake, indicating that exposed individuals did take up the new schooling when it expanded access. Despite this, I find no statistically significant effects on income, educational attainment beyond primary school, or language use at home. Together, these results suggest that improvements in human capital, broadly defined, are unlikely to explain the main effects of the reform.

In two complementary analyses, I explore community responses to the opening of Swedish-only schools. The first examines the immediate cultural response to school openings using names given to children, while the second examines how communities evolved over subsequent decades in terms of language use and educational provision. I proxy educational provision using teacher presence, classifying teachers as Finnish or Swedish based on whether they were recorded as "Finnish" by census enumerators. These analyses allow me to investigate whether the shift toward Finnish identity among individuals directly exposed to Swedish-only schooling was part of a broader cultural resistance to the schools, and how the effects of individual exposure and school openings jointly shaped communities over time.

Leveraging within-family variation over time and variation across villages, I examine naming patterns before and after school openings. I find that children born after school openings received names that were more Swedish and less Finnish, indicating a community-level naming response to the opening of a Swedish-only school that moved in the opposite direction from the effect of individual exposure to Swedish-only schooling.

Comparing treated and control villages in the two decades before and after school openings, I find that the share of households using Finnish as the main language at home declined, in contrast to the absence of an average effect among individuals exposed to Swedish-only schooling. Moreover, the prevalence of Finnish teachers did not increase after school openings, while the prevalence of Swedish teachers did, providing little support for the idea that alternative Finnish-language schooling emerged in response to the reform.[3] Taken together, these results suggest that the Finnish identity response among individuals directly exposed to Swedish-only schooling was not part of a broader cultural resistance to

[3]The results also suggest that, relative to control villages, the introduction of Swedish-only schools expanded overall educational provision rather than simply replacing schools staffed by Finnish teachers, at least during the period of study.

the schools. Instead, community-level responses moved in the opposite direction, and the shift away from Swedish identity among exposed individuals did not overturn the broader changes in language use that followed school openings.

I next turn to the mechanisms underlying the effects of exposure to Swedish-only schooling. Taken together, the individual exposure results highlight an important distinction between social integration and national identity as two related but distinct dimensions of assimilation.[4] These findings need not be contradictory. The schools may have increased interaction among Swedish-born individuals in the region who differed in language background, either through joint school attendance or through changes in language skills, without necessarily altering their national identity. As a result, interactions and matches may have increased directly through network effects or indirectly if greater contact reduced prejudice, in line with contact theory (Allport, 1954; Bursztyn et al., 2024; Corno et al., 2022; Lowe, 2025; Enos, 2014).

I first consider whether changes in language skills can account for the increase in social integration. There is little evidence that broader improvements in human capital or language account for the results, as I find no effects on income, education beyond primary school, or language use at home. This does not imply that language was unimportant for social interactions; rather, improvements in Swedish language skills do not appear to be the primary channel. Children were already learning Swedish before the reform but were generally instructed through their first language. Switching to Swedish-only instruction therefore did not necessarily improve their acquisition of Swedish and may even have hindered learning.

Instead, several results point toward an important role for increased intergroup contact in promoting social integration. First, the social integration effects are present both among individuals from families using Finnish at home before treatment and among those using Swedish. The effects are therefore not concentrated among Finnish-speaking families, for whom the potential improvement in Swedish language skills was greatest. Second, the effects are larger in places where both a Finnish teacher and a Swedish teacher were present nearby before treatment. This pattern supports the interpretation that the reform increased mixing in schools by bringing together children who had previously received separate instruction based on their first language. Finally, the effects on social integration are stronger in areas with weaker Finnish ethnolinguistic attachment, measured by the share of individuals using Finnish at home. In these areas, a larger share of individuals speaking Swedish at home plausibly created greater scope for intergroup contact in schools.

[4]Social integration captures the social dimension of assimilation, while national identity captures part of its cultural dimension. Assimilation entails not only greater social integration between groups but also convergence in cultural attributes, such that the groups become increasingly indistinguishable from one another.

The identity responses to exposure to Swedish-only schooling are consistent with the schools making the language dimension of the regional ethnicity more salient, thereby strengthening Finnish attachment and weakening Swedish attachment (Shayo, 2009, 2020; Fouka and Tabellini, 2026). An alternative mechanism is increased parental cultural transmission, for which I find limited support.[5] Specifically, the effects are not larger in communities where the minority group is smaller, as commonly predicted by models of cultural transmission. Moreover, the community-level naming response moves in the opposite direction, suggesting that increased cultural transmission by parents or the broader community is unlikely to explain the individual exposure effects. Instead, the increase in Finnish naming is larger in communities where the nearest Swedish teacher was located farther away before the reform. In these communities, the introduction of Swedish-only schooling represented a larger change in the local presence of the Swedish language, potentially making the linguistic distinction between Finnish and Swedish more salient, altering perceived distance to Finns across the border, and thereby shifting expressions of identity.

This paper contributes to the literature on nation-building through education. Existing work studies both bundled reforms (Blanc and Kubo, 2025; Hu and Luo, 2026; Clots-Figueras and Masella, 2013) and reforms that isolate components, including curriculum (Cantoni et al., 2017) and language (Fouka, 2020; Lleras-Muney and Shertzer, 2015). Nation-building policies are often bundled, with joint effects that need not equal the sum of their parts (Carlitz et al., 2025). I study a reform that combined school expansion with a change in language of instruction, but not curriculum content, providing a novel setting in which to examine how these dimensions interact when implemented jointly. The bundled nature of the reform is also informative because it changed not only access and language of instruction but also, indirectly, opportunities for contact across ethnolinguistic groups, changes that are often inherent in real-world language-of-instruction policies. I also contribute to this literature by studying both the effects of direct individual exposure to the new schooling regime and broader community responses to the policy. My results show that these responses need not move in the same direction, highlighting the importance of considering both when evaluating nation-building policies.

Relatedly, I contribute to our understanding of how assimilation policies shape integration and identity, and when they generate counter-assimilatory responses. Existing work documents such responses ranging from the creation of alternative schools (Bazzi et al., 2026) and withdrawal from formal ed-

[5] In the paper, I also discuss alternative interpretations of the results, namely whether the expressions of identity reflect a signaling response following increased social integration, and whether the identity effects capture a stronger regional rather than specifically Finnish identity. I find limited support for these alternative interpretations.

ucation (Meyersson, 2014) to changes in identity among individuals who remain within the intended educational system (Fouka, 2020). Theoretically, these identity responses among individuals who remain within the educational system may arise through increased cultural transmission by parents or communities (Bisin and Verdier, 2001; Carvalho et al., 2024), or because assimilation policies alter individuals' social identity choices by changing perceived group distance or the relative status of ethnicities (Shayo, 2009, 2020; Fouka and Tabellini, 2026). I find limited support for cultural transmission and instead provide evidence consistent with linguistic differences becoming more salient and thereby altering perceived distance between ethnic groups. Related work has studied how intergroup conflict (Atkin et al., 2021) and migration-induced changes in group composition (Fouka and Tabellini, 2022; Fouka et al., 2022; Fouka and Serlin, 2026) can alter the salience of group attributes or membership and therefore affect social identity through perceived distances between groups. My results suggest that similar changes can arise when policy makes a particular dimension of ethnicity more salient. More broadly, my results show that greater social integration can coexist with a reduction in national identity. This complements recent work showing that assimilation along some dimensions can coexist with persistent cultural attachment (Maruthiah, 2025).

Finally, I contribute to the literature on how language of instruction interacts with education expansion. Although historical, my analysis offers lessons for contemporary low-income countries, where expanding basic literacy and numeracy remains a central policy priority (Aghion et al., 2025), while also allowing me to study long-run outcomes that are often difficult to examine experimentally. Existing studies find negative (Angrist and Lavy, 1997), positive (Taylor and von Fintel, 2016), or null effects (Angrist et al., 2008) of language of instruction on human capital. These differences may reflect how language policies are implemented (Lloyd and Yang, 2025) and children's familiarity with the language of instruction (Laitin and Ramachandran, 2016). My setting allows me to study a change in language of instruction implemented alongside an expansion in school access. I find increased primary-school uptake where prior educational supply was limited, but no corresponding effects on further educational attainment, income, or occupational upgrading. My findings also highlight that the consequences of language-of-instruction policies extend beyond human capital, underscoring the importance of considering opportunities for intergroup contact alongside learning when designing such policies.[6]

[6]This relates to recent work examining integrated versus separate schooling for refugee children and potential trade-offs between assimilation and human capital accumulation (Pavese et al., 2026).

2. BACKGROUND

2.1 The language divide in Tornedalen

Finland was part of Sweden for almost 700 years before becoming an autonomous part of the Russian Empire in 1809. The border was drawn along the Torne River, splitting the culturally connected region, where the majority spoke Finnish, between two states. When Finland gained independence in 1917, the border remained unchanged, and the influence of the majority culture in each state continued to affect the region differently even though the border itself was relatively easy to cross. Today, shared history and lifestyle still culturally bind Tornedalen, even though the majority language on the Finnish side is Finnish and the majority language on the Swedish side is Swedish. Even so, some people on the Swedish side speak Finnish as a second language, and there is a local variety of Finnish, Meänkieli, which is a recognized minority language in Sweden.[7]

2.2 Education and language policy in Tornedalen

From 1842, every parish in Sweden was required to provide basic education, with minimum requirements in arithmetic, reading, writing, and scripture. However, early provision was limited due to funding constraints. Financing was primarily local, but from the 1860s onward, the state introduced grants covering about 30% of total costs to increase provision. Provision was further expanded by allowing different types of schools, including fixed primary schools offering six years of education, ambulatory schools in which teachers travelled between villages in rural areas, and minor schools enrolling students for only two years, the latter playing a key role in the spread of mass education (Andersson and Berger, 2019).

Before 1889, Finnish was the main language of instruction in Tornedalen, and there was little political will to change this, as it was considered self-evident in Sweden that participation in religion, politics, and education should take place in one's own language (Hyltenstam and Salö, 2023). The removal of Finnish as a language of instruction began with the introduction of state-funded schools: 4 announced on November 2, 1888; 12 on March 17, 1894 (of which 4 were ambulatory); 4 on September 21, 1894; and 1 on September 28, 1895 (Tenerz, 1960; Tillförordnade Folkskoleinspektörer, 1900). In Appendix A.2, I summarize information on these 21 schools.

Instruction in Swedish was a requirement for state funding. These Swedish-only schools were not

[7] Individuals who speak Meänkieli are often referred to as Tornedalians, although the definition can be extended to include all individuals living in the geographic region and their descendants.

forcibly imposed; participation was voluntary, and local communities were largely motivated by the economic benefits, as the region was very poor. By 1919, almost 40% of all primary schools in the region were state-funded, and the practice of using only Swedish as the language of instruction later spread to parish-led schools (Elenius, 2023). During this period, Finnish was also gradually removed as a subject.[8]

The approach of forbidding Finnish in schools was known locally as the "All-Swedish Method", and it required Finnish-speaking children to use only Swedish from their very first day of school.[9] Previously, a bilingual approach known as the "Translation Method" had been used in Tornedalen. This method relied on students' native language to support the learning of Swedish, using textbooks with parallel texts in Swedish and Finnish (Elenius, 2023).

2.3 Educational attainment before and after the introduction of Swedish-only schools

At the time of the introduction of the Swedish-only schools, many children left school around ages 11–12 to work, and even those enrolled often attended irregularly. Absences were common due to work at home, long travel distances, teacher shortages, and "parental indifference". Attendance rates, measured as the share of eligible children who at some point attended school, varied substantially across parishes, ranging from almost 90% in the more urban parish of Haparanda to as low as 40% in the rural parish of Karesuando (Tillförordnade Folkskoleinspektörer, 1894).

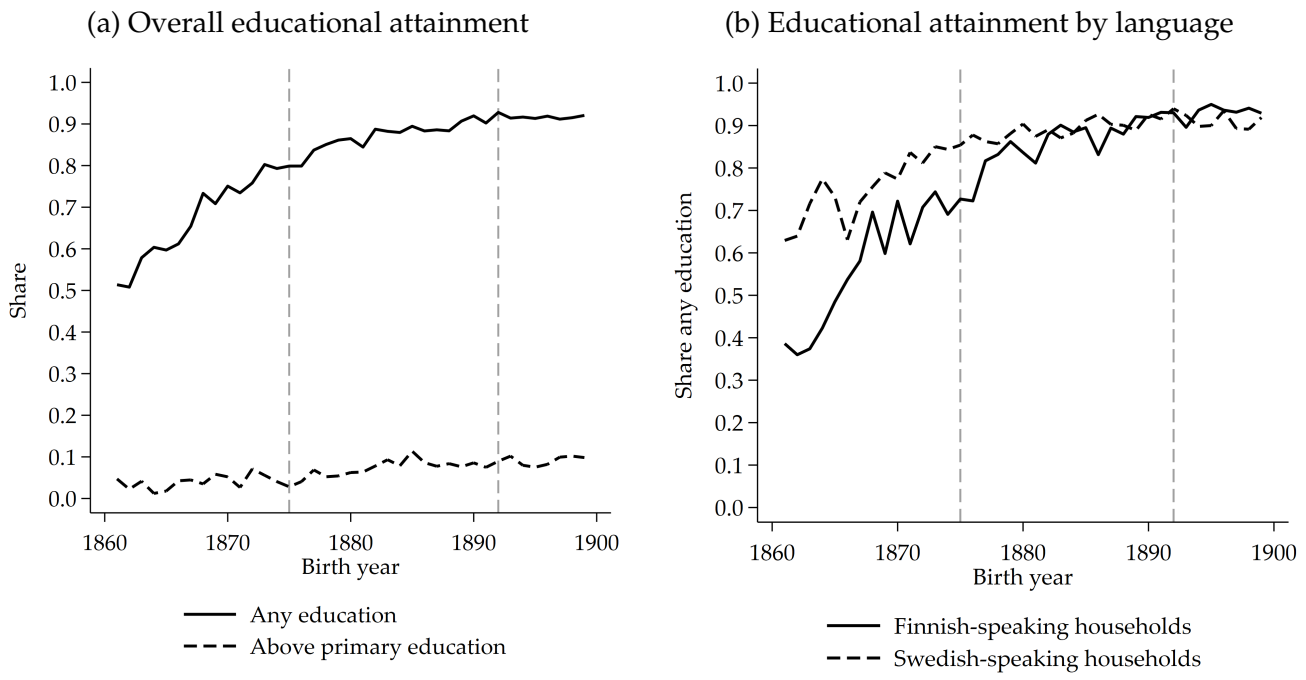
To provide a sense of educational attainment over time, Figure 1 presents levels of education reported in the 1930 census for different birth cohorts. The dashed vertical lines indicate the cohorts affected by the introduction of the Swedish-only schools studied in this paper.

Panel (a) shows that most of the increase in schooling occurred prior to the introduction of the Swedish-only schools, reaching roughly 80% with any education. Thereafter, schooling levels increased more slowly and stabilized at around 90%. The share of individuals receiving education beyond the primary level increases somewhat for exposed cohorts but remains modest, at around 10%. Panel (b) shows that among exposed cohorts, educational attainment among individuals recorded in 1890 as "Finnish" gradually converges toward those recorded as "Swedish".

[8] Both Finnish and Swedish were taught as subjects in Tornedalen. Specifically, in the early 1870s, five hours per week were dedicated to Swedish and four to Finnish. By 1885, Swedish had double the allocated hours of Finnish (8 vs. 4 hours). Finnish was further reduced to three hours in 1892, two hours in 1893, and just one hour in 1905. Finally, in 1912, Finnish was entirely removed from the curriculum, both as a subject and as a language of instruction (Hyltenstam and Salö, 2023).

[9] It was also referred to as the "Berlitz Method", named after N. D. Berlitz, who developed it at a language institute in New York in the 1870s.

FIGURE 1
Educational attainment over time



Note: The figure shows educational attainment by birth cohort using information from the 1930 census in the region of interest. The area between the dashed vertical lines indicates the cohorts exposed to the introduction of Swedish-only schools. Panel (a) reports the share of individuals with any educational attainment and the share with education beyond the primary level, while Panel (b) reports the share with any educational attainment by whether individuals were recorded as "Finnish" or "Swedish" in the 1890 Census, using a linked sample.

2.4 Motivations and implementation of nation-building

There were several motivations behind the nation-building efforts in Tornedalen (Elenius, 2023). First, the presence of a culturally distinct population along the border with Russia was viewed as a military concern, particularly given Sweden's history of conflict with Russia and ongoing assimilation efforts on the Russian side. Second, the regional religious movement Laestadianism, in which Finnish functioned as the religious language, was perceived as challenging the authority of the state church. Finally, these developments unfolded during a period of rising nationalism in Europe, when the ideal of a monolingual nation-state was particularly strong.

Education became the central instrument of these nation-building efforts, although other forces, such as railroad expansion and industrialization, also contributed to the diffusion of Swedish language and culture through increased mobility and in-migration of Swedish-speaking labor (Elenius, 2001; Cermeño

et al., 2022; Hyltenstam and Salö, 2023).

The policies introduced in the late nineteenth century remained in place for decades and were later characterized as suppressive (Regeringskansliet, 2023). Swedish-only state schools were not abolished until 1940, and language prohibitions were lifted only in 1957. Over time, assimilationist policies grounded in the ideal of a one-language nation-state were reinforced by racialized thinking, which also affected other minorities in Sweden, including the Sámi and Romani. Such ideas were reflected in pseudo-scientific efforts to classify and rank populations, practices largely abandoned after the Second World War.

3. CONCEPTUAL FRAMEWORK

The effect of the reform on social integration and national identity is ex ante ambiguous. The reform affected both the language of instruction and school access, making it a bundled treatment whose individual components and overall effect may operate through several different channels. In this section, I first define identity and how I think about integration, and then discuss potential mechanisms and theoretical predictions of the effects of Swedish-only schooling, assuming there was take-up. Figure 2 summarizes the framework.

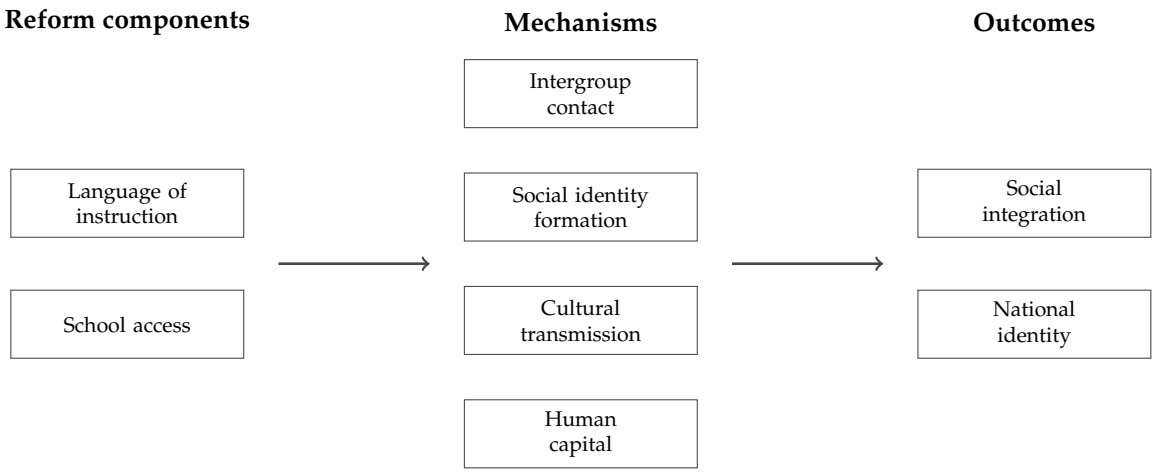
3.1 Defining social integration and national identity

Identity refers to a person's sense of self and may respond to the social environment (Akerlof and Kranton, 2000). I draw on social identity theory and self-categorization theory from social psychology both to conceptualize national identity as a specific form of social identity and to understand the mechanisms through which schooling may shape it. National identity is therefore highlighted as an outcome, while social identity formation is included as a mechanism in Figure 2. Social identity is derived from membership in social groups, and individuals may categorize themselves in terms of particular group memberships depending on the social context. Shayo (2009, 2020) formalize social identity as a choice that depends on the relative status of a group compared with other social groups, as well as the perceived distance between an individual and the group. In my setting, individuals may have both Swedish and Finnish ethnic backgrounds without necessarily identifying with either nation, as identification is subjective. Moreover, the attributes that determine perceived distance between an individual and a group may depend on their salience in a given social environment. Social identity can, in turn, affect behavior in two ways: individuals may favor actions that improve the status and outcomes of groups with which

they identify, and they may adjust their behavior in ways that are more consistent with the characteristics and norms of those groups.

Like assimilation, social integration is an equilibrium outcome, as it depends not only on the choices of minority individuals themselves but also on the behavior of members of the majority group (Fouka, 2022). Whether social integration occurs thus depends on an individual's choice to integrate and on whether members of the majority accept the minority individual. However, social integration is distinct from assimilation. I use social integration to refer to stronger social relations across group boundaries without requiring individuals to give up their distinct culture, of which identity is a key component. Assimilation instead refers to convergence toward the majority group, such that distinctions between groups diminish, including differences in cultural practices and expressions of identity. Given these definitions, national identity and social integration need not move together.

FIGURE 2
Conceptual framework



Notes: The figure summarizes the conceptual framework linking the two components of the reform—language of instruction and school access—to social integration and national identity. Both components may affect these outcomes through several potentially overlapping mechanisms. Intergroup contact may shape intergroup relations through network formation and changes in prejudice, as emphasized by contact theory. Social identity formation suggests that changes in the relative status of groups, perceived group distance, and the salience of group characteristics may affect national identity. Cultural transmission captures how parents may respond to assimilation pressure by strengthening the transmission of minority culture within the household. Finally, human capital accumulation, including language skills, may affect economic opportunities and interactions across groups. These mechanisms may affect both outcomes directly or indirectly, as changes in social integration may influence national identity and vice versa.

3.2 *Potential mechanisms*

Swedish as the language of instruction may affect social integration through changes in language skills and the resulting opportunities for intergroup contact. If the schools improved Swedish language skills, this could expand who individuals were able to interact with and thereby strengthen intergroup relations. However, learning in an unfamiliar language may also hamper learning, so the effects could go in the opposite direction.

Beyond these language-skill effects, the change in language of instruction may also have affected social integration through intergroup contact in schools. Opportunities for intergroup contact changed, as children from different language groups were now more likely to meet in school, while teachers were more likely to speak only Swedish. Who individuals meet may affect intergroup relations, both directly through the formation of social networks and indirectly through reduced prejudice, as emphasized in contact theory (Allport, 1954; Bursztyn et al., 2024; Corno et al., 2022; Lowe, 2025). Here, the nature of the interactions matters. If relationships within schools are characterized by discrimination, exclusion, or boundary maintenance by the majority group, greater contact may weaken intergroup relations. In contrast, if interactions are more equal and cooperative, greater contact may instead strengthen intergroup relations.

Language of instruction may also affect national identity through social identity formation. First, language skills may affect perceived distance to the broader Swedish population and thereby alter national identity. Second, because the language change was mandated by the state, it may have altered the perceived status of Swedish relative to Finnish, again affecting national identity. Third, the reform may also have made differences from the rest of Sweden, and similarities to Finland, more salient. Prior to the reform, children were allowed to learn in Finnish within the Swedish educational system, which may have signalled institutional acceptance of both languages. The reform instead clearly signalled that Swedish was the state-supported language, potentially making language a more salient component of Swedish ethnicity and altering perceived distance between individuals and the two groups. In particular, this may have increased perceived distance from the Swedish majority while reducing perceived distance to Finns across the border.

Language of instruction may also affect national identity through cultural transmission. Parents or the community at large could respond to assimilation pressure by increasing cultural transmission, potentially generating backlash (Carvalho et al., 2024). This could, for example, occur through increased parental transmission of cultural traits within the household (Fouka, 2020) or through the opening of

alternative schools (Bazzi et al., 2026). This mechanism is not entirely distinct from social identity formation, as increased transmission of cultural traits may alter perceived distances. However, in Figure 2, it is highlighted as a separate mechanism.

Access to education can also affect human capital more broadly, beyond the language skills discussed above. Greater human capital accumulation may lead to different job opportunities and greater economic integration. This may influence social identity through changes in perceived proximity to different groups. It may also affect social integration through contact with different groups.

It is also important to note that national identity and social integration may influence each other. Changes in national identity may affect social integration through homophily, while social integration may in turn shape national identity through greater exposure to other groups (Bazzi et al., 2019; Fouka and Tabellini, 2026).

These considerations motivate the empirical analysis that follows. I first examine educational outcomes, income, and a proxy for language use at home to assess whether the reform affected school participation and human capital, broadly defined. I then study social integration and expressions of national identity separately, allowing me to distinguish between the two. Finally, I discuss and examine evidence on the potential mechanisms outlined here and summarized in Figure 2.

4. DATA

In this section, I present my data sources, describe how I measure identity and integration, and explain the construction of the analysis samples. This involves linking individuals across historical Swedish population censuses, geocoding villages of residence, and identifying comparable villages.

4.1 Sources

Population census. My main data source is the Swedish population censuses, which are available through IPUMS for the years 1880, 1890, 1900, and 1910 and cover the entire Swedish population. The 1930 census is partly digitized (approximately two-thirds) and available from the Swedish National Archives. See Appendix A.1 for details on how I use weights in the analysis to account for this incompleteness. Importantly, the 1930 census is complete for the counties in which all individuals in my sample were born and where most continue to reside throughout their lives.

The censuses report individual-level information such as occupation, names, date of birth, village of residence, and parish of birth. Individuals are also linked within households, making it possible

to identify partners and children and to construct household-level characteristics, such as household size and the presence of servants. The 1930 census additionally contains information on income and education.[10]

Importantly, the censuses contain information on whether an individual was recorded as "Finnish". Between 1880 and 1910, census enumerators were instructed to record whether individuals belonged to an ethnic minority, such as the Finnish minority or the Sami. In the absence of a better principle, enumerators were instructed to infer ethnicity from the language commonly spoken in the household. What constituted a "better principle" is less clear. For the Sami, occupation, such as reindeer herding, provided an additional observable marker of ethnicity. For the Finnish minority, however, language was arguably the most salient observable characteristic available to enumerators, making this measure a useful, albeit imperfect, proxy for language.

Schooling. Information on Swedish-only school openings is drawn from secondary and primary sources (Tenerz, 1960; Tillförordnade Folkskoleinspektörer, 1900; Borgh, 1889; Lindell, 1896). A summary of this information is presented in Appendix A.2, while information on other types of schooling is derived from occupational data in the 1890 census. I identify individuals listed as working full-time or part-time as teachers and use their village of residence to determine which locations had educational supply in 1890. I validate this information with teacher registers from 1889 and 1895 (Borgh, 1889; Lindell, 1896). The reason I do not rely solely on these registers is that they contain information only on primary schools and not on minor schools (the first two years of primary school), which were widely used during this period.

Emigration. Individual-level information on emigrants from Tornedalen is from the Electronic Emigrant MUSeum, EMUS, a database created by Herbert Wirlöf and maintained by the Haparanda Torneå Genealogical Society that combines different sources on migrants away from the Tornedalen region into a database. This source contains information on the name of the emigrant, birth year, emigration year, the parish from which they emigrated, and the country to which they emigrated. It makes no claim to include all emigrants. However, after comparison with aggregate parish-level data based on parish registers kept by priests (Demografiska databasen, CEDAR, Umeå universitet, 2022), the source seems to be relatively complete. For details and a discussion of this comparison, see Appendix A.3.

[10] Income data are primarily drawn from individual declarations and tax registers and are reported in units of 100 crowns. Income is reported for all individuals earning above a set threshold, which in 1930 was 600 crowns, ensuring coverage even among relatively low-income groups. For reference, male agricultural laborers earned slightly more than 1,000 crowns, while female laborers earned somewhat less than 900 crowns (Jayes et al., 2025). The tax information was recorded in registers collected annually by the tax authority for fiscal purposes. Enumerators were also instructed to record the highest level of education attended and whether the individual had graduated.

4.2 Measuring social identity and social integration

I measure identity using names given to children and information on whether an individual worked in a military profession, and I measure integration using marriage patterns and residential segregation. Below, I describe how these measures are constructed.

Children's names. To capture individuals' own assimilation decisions and the vertical transmission of cultural traits from parents to children, I use given names reported in the 1910 and 1930 censuses. I interpret these decisions and this transmission as expressions of identity and revealed preferences. Each name is assigned a Finnish, Tornedalian, and Swedish Cultural Name Score (CNS), which measures the extent to which a name is associated with a particular cultural group relative to the set of cultural groups present in the region, including the Sami and immigrants.

A common approach in the literature is to construct these scores by comparing one group with another, for example Black versus White (Fryer and Levitt, 2004), or by comparing one group with a broader "other" category, for example German versus non-German (Fouka, 2020) or Italian versus non-Italian (Gagliarducci and Tabellini, 2025). However, I am interested in expressions of identity toward a particular group relative to the full set of cultural groups present in the region, rather than relative to a single "other" category.

In my setting, there is no clear single out-group because of the cultural diversity of the region and its historical ties to Finland. In addition to the regional Tornedalian minority, the region included a Sami minority and immigrants. Moreover, the Tornedalian population had ties both to a broader Swedish ethnicity and to Finnish ethnicity, and it is not clear ex ante how expressions of identity toward Sweden, Finland, or Tornedalen would respond to the reform. My main measure therefore follows the generalization in Bazzi et al. (2020), which allows the name score to account for multiple ethnic groups. Adapted to my setting, the score becomes:

$$CNS_{n,c,t} = 100 \times \frac{P_t(n \mid c)}{\sum_{g \in \{\text{Swedish, Finnish, Tornedalian, Sámi, Immigrant}\}} P_t(n \mid g)}. \tag{1}$$

where $P_t(n \mid c)$ denotes the prevalence of name n within cultural group c, calculated using names given during the 20 years preceding census year t, and g indexes the five cultural groups included in the denominator: Swedish, Finnish, Tornedalian, Sami, and immigrant. Thus, the cultural association of a name is allowed to vary over time and reflects contemporary naming patterns rather than a fixed classification. The CNS ranges from 0 to 100, with higher values indicating that the name is more strongly

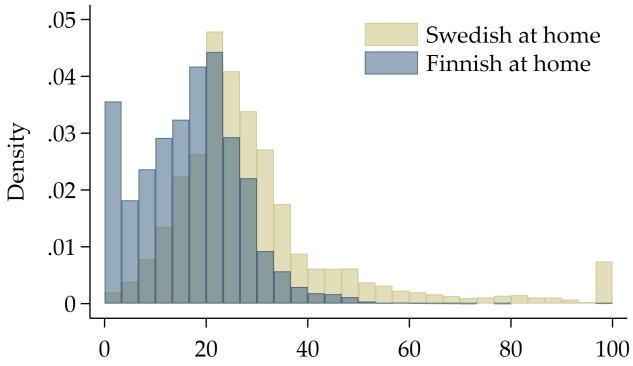
concentrated within the cultural group of interest.[11]

By comparing each name across all relevant cultural groups simultaneously, the CNS captures distinctiveness within the full regional naming landscape without relying on a single, potentially arbitrary comparison group. More details of the construction and examples of high scoring names are provided in Appendix A.4. With that said, I also use a more standard measure as a robustness check, comparing Swedish with non-Swedish names by combining the Sami, immigrant, Tornedalian, and Finnish groups into a single "other" category.

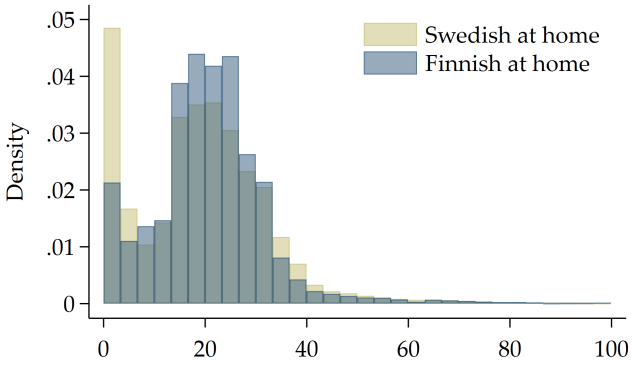
FIGURE 3

Distributions of Cultural Name Scores 1890 by ethnicity

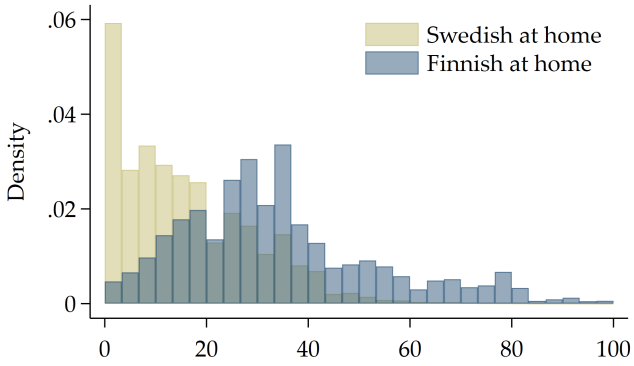
(a) Swedish CNS



(b) Finnish CNS



(c) Tornedalian CNS



Notes: The figure shows the distributions of the Swedish, Finnish, and Tornedalian Cultural Name Scores (CNS), respectively, for individuals in Sweden in 1890. Distributions are shown separately by whether an individual is recorded as "Finnish" or "Swedish" by census enumerators.

Figure 3 shows the distributions of the CNSs before the opening of the schools in 1890, by language

[11]The Finnish CNS is calculated using the prevalence of the name among individuals born in Finland. The Swedish CNS is calculated using the prevalence of the name among individuals born in Sweden, excluding individuals born in Tornedalen and individuals classified as Sami. Sami individuals are identified using census information. Tornedalians are defined as individuals born in Tornedalen and residing there at the time of the census.

use at home, proxied by whether individuals were recorded as "Finnish" or "Swedish". Panel (a) shows the distribution of the Swedish CNS, which is clearly shifted toward higher scores for individuals speaking Swedish at home compared to those speaking Finnish at home. The Finnish CNS, shown in panel (b), displays a different pattern. Here, the distributions are relatively similar, except for a higher density of individuals speaking Swedish at home with a Finnish CNS of 0. This suggests that although the minority was Finnish-speaking, they did not necessarily have more Finnish names in 1890. However, they did have fewer Swedish names, seemingly in favor of regional names, whose distribution is shown in panel (c).

Military profession. Different forms of military participation have previously been used to measure national identity, as they constitute a high-stakes commitment to the nation. For example, Fouka (2020) uses volunteering in World War II to study German versus U.S. identity, and Blanc and Kubo (2025) uses data on Resistance heroes and Nazi collaborators during World War II to measure French identity. Moreover, Qian and Tabellini (2026) show that racial discrimination and political exclusion likely affected the volunteer enlistment of Black Americans after the attack on Pearl Harbor.

Although Sweden remained neutral during World War I, it maintained a military with professional personnel alongside conscription, and the censuses contain occupational data. I therefore leverage this information to construct an indicator for ever having worked in the military, pooling all available occupational information, including both current and previously reported occupations in 1910 and 1930.[12] Working in the national military would have required functioning in a Swedish-speaking national institution and presumably reflects at least some attachment to the Swedish state. It is also an interesting outcome because one of the goals of nation-building policies is to strengthen the country against foreign threats, and maintaining a strong military is central to that objective.

Marriage patterns. To capture effects on intergroup relations, I study the propensity to have a partner observed in the 1910 and/or 1930 censuses who was born in Finland, Tornedalen, or Sweden outside Tornedalen.

I also construct alternative continuous measures based on surnames to capture the propensity to have a partner with stronger attachment to either Finnish or Swedish ethnicity or a more ethnically mixed background, rather than relying only on the partner's place of birth. Specifically, I assign each surname observed in the region pre-treatment a probability of being ethnically Finnish rather than Swedish based on the "Finnish" classifications recorded by census enumerators. I then use this measure to impute the

[12]Sweden transitioned to universal male conscription in 1901. Conscription required military service from Swedish men, but the outcome used here captures military employment reported as an occupation rather than compulsory military service.

ethnic attachment of partners born within Tornedalen, while assigning a value of zero to partners born in Sweden outside Tornedalen and one to partners born in Finland. I refer to this measure as predicted Finnish attachment. Using this measure, I also construct predicted ethnic mixedness as $2p(1-p)$, where p denotes predicted Finnish attachment. The measure is lowest for partners with strong predicted attachment to either the Finnish or Swedish group and highest for partners whose predicted attachment lies between the two groups.

Residential segregation. I measure village-level segregation versus integration using a fractionalization index following Alesina et al. (2003), which captures the probability that two randomly selected individuals belong to different groups. Formally, the index is defined as $F_l = 1 - \left(p_l^2 + (1-p_l)^2\right)$, where p_l denotes the share of Finnish individuals in village l, measured using the "Finnish" classification in the census and/or country of birth. The index equals zero when the village is fully homogeneous and is maximized at $p_l = 0.5$, with higher values indicating greater ethnic diversity and, in this setting, greater residential integration. This measure is only available in 1910 because I do not observe the minority classification in 1930.

4.3 Census linking

For my main empirical strategy, I require individual-level information before treatment. Therefore, I link individuals across census waves. I use automated record linkage following Abramitzky et al. (2021), but adapt the approach to the Swedish census, which differs in key aspects such as the accuracy of year of birth and the reporting of last names. My method also draws inspiration from Wisselgren et al. (2014) and Berger et al. (2025), who link the Swedish census over time.

I make some adjustments to these approaches. The main ones are that I conduct two rounds of matching: first using full first names, including middle names, and then, for individuals who remain unlinked, excluding middle names. Second, because individuals may have multiple alternative surnames, I require a match to be unique across all surname alternatives. Thus, if one surname links an individual to one person and another surname links the same individual to a different person, I do not consider the match. I obtain backward linkage rates of approximately 82% for both genders when linking the 1890 census to the 1910 and 1930 censuses, respectively. Details are provided in Appendix A.5.

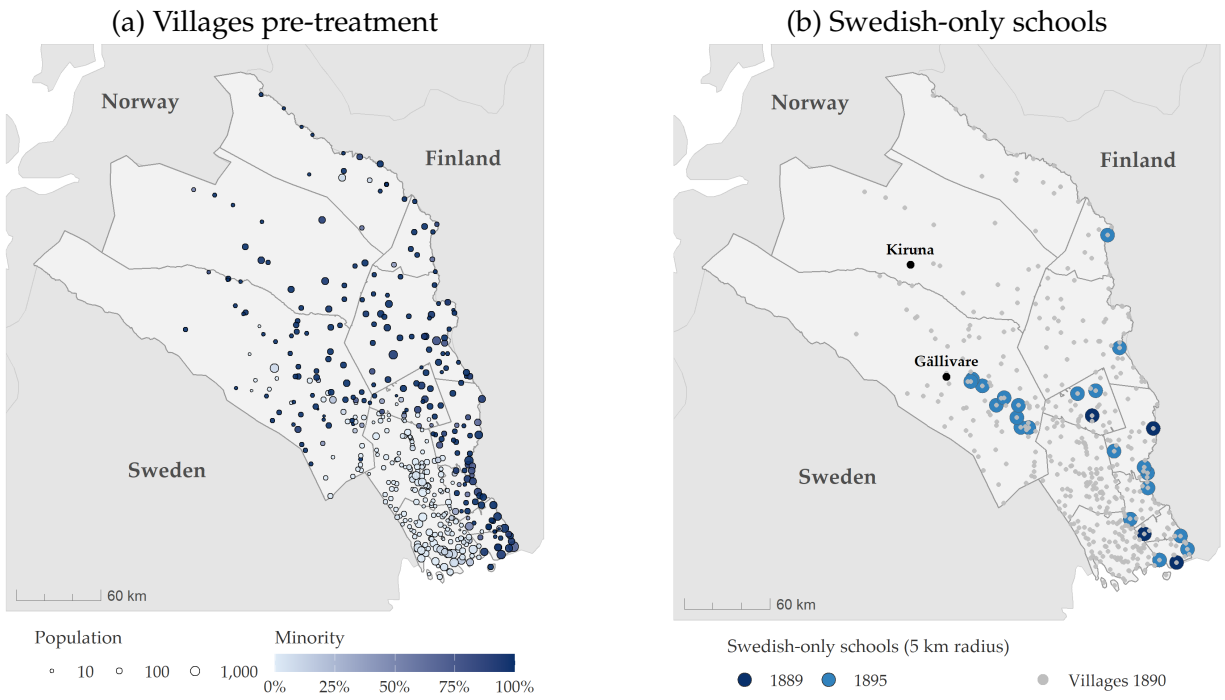
4.4 Treated and control villages

In this section, I describe how I define my treated and control villages, terms that I use even though my main analysis is at the individual level and also leverages variation within villages. I use these terms

because treatment assignment is partly based on village of residence prior to the relevant school opening.[13] The set of villages I consider consists of those located within an extended definition of Tornedalen that includes all parishes with at least one individual recorded as "Finnish" in the census.[14]

I define a village as treated if it is located within 5 km of a Swedish-only school, a cutoff chosen based on historians' assessments of typical travel distances (Pers et al., 2022), as well as evidence from my own archival work showing that children often traveled up to approximately 5 km to attend school. To calculate these distances, I manually geocoded all villages within my definition of Tornedalen for all relevant census waves. Figure 4 shows the spatial distribution of villages in 1890 and the Swedish-only schools, as well as the 5-km radius around them.

FIGURE 4
Villages in 1890 and Swedish-only school locations



Notes: The left panel (a) shows the spatial distribution of villages prior to the opening of Swedish-only schools, with marker sizes weighted by population and shading indicating the share of individuals recorded as part of the "Finnish" minority. The right panel (b) shows the location of Swedish-only schools in the region of interest in relation to the villages, together with a 5-km radius.

I do not use all villages in Tornedalen as controls for three reasons. First, including all villages would introduce substantial noise, as some are not appropriate counterfactuals for treated villages. Second, my

[13] In general, the year after announcement; see Appendix A.2.

[14] The parishes of Nederkalix and Överkalix are included in my extended definition of Tornedalen, although they are not always considered part of Tornedalen. I include them because some schools are located near the borders of these parishes and some villages in these parishes have a few individuals recorded as "Finnish".

main identification strategy relies on the assumption that cohorts in treated and control villages would have evolved similarly in the absence of treatment (parallel trends). To strengthen the plausibility of this assumption, it is helpful to use villages that are ex ante as comparable as possible to treated villages. Third, in a complementary analysis, I specifically study the effects of a Swedish-only school opening in a village, which also requires comparable control villages.

In addition, to prevent contamination of the control group, I restrict controls to villages located more than 10 km from any school opening, thereby reducing potential attenuation bias arising from partially treated villages if children traveled beyond the 5-km treatment radius.

Specifically, I select control villages using propensity score matching with replacement, choosing each village's five closest neighbors in propensity score space as matched controls. Propensity scores are estimated using village-level economic and demographic characteristics measured in 1890 (log population, mean age, share of males, labor force participation of men and women, share of farm households, average household size, and log distance to the industrializing mining town of Gällivare), cultural characteristics (minority classification, cultural name scores, and log distance to the border), and measures of educational provision (teacher presence in 1880 and 1890).

Descriptive statistics for untreated villages, matched control villages, and treated villages are reported in Table 1. The results show that matching substantially improves comparability between treated and control villages for the majority of characteristics. Importantly, the cultural characteristics improve following matching and display a high degree of balance between treated and control villages. Figure A2 in the Appendix shows the geographical distribution of the matched control villages.

While the identifying assumption allows for level differences between treated and control villages, it becomes less credible when villages differ substantially in observable characteristics. This concern is particularly relevant given that school openings are unlikely to have been random; absent matching, one might worry that younger cohorts in treated villages were already on a different assimilation trajectory than those in control villages, and that such differences influenced the decision to establish schools.

5. EMPIRICAL STRATEGIES

I first introduce the empirical strategy for my main analysis, which identifies the effects of individual exposure to Swedish-only schooling. I then present two additional strategies to examine community-wide responses.

TABLE 1
Comparability of treated and control villages in 1890

	Not treated	Matched control	Treated
Economic and demographic			
Population (mean)	76.07	192.26	175.66
Population (log)	3.48	4.55	4.42
Age (mean)	25.91	25.21	26.22
Males (share)	0.52	0.51	0.51
Females in the labor force (share)	0.08	0.06	0.07
Males in the labor force (share)	0.49	0.48	0.54
Farm households (share)	0.65	0.53	0.58
Household members (mean)	6.16	5.61	5.55
Distance to Gällivare (log)	4.71	4.52	4.72
Education			
Any teacher 1880	0.07	0.09	0.13
Any teacher 1890	0.11	0.30	0.28
Culture			
Finnish at home	0.46	0.73	0.72
Finnish CNS (mean)	18.26	19.29	19.70
Swedish CNS (mean)	18.66	17.89	17.86
Tornedalian CNS (mean)	28.12	32.02	32.39
Distance to Finnish border (log)	3.50	2.79	2.62
Villages	405	104	47

Notes: The table reports village-level characteristics measured in the 1890 census. Treated villages are defined as those located within 5 km of a Swedish-only school opening. Matched control villages are located more than 10 km from any school opening and consist of the five nearest neighbors selected using propensity score matching based on all variables reported in the table, except population size, for which the log of population is used.

5.1 Individual exposure to Swedish-only schooling

5.1.1 Empirical specification

To estimate the individual-level causal effect of exposure to Swedish-only schooling, I use a difference-in-differences design. Specifically, I compare individuals who were at or below school age (3–14) with cohorts that were too old for primary school (15–22) at the time of the school opening, across treated and control villages. My main specification is

$$y_{i,l,c} = \beta \left(\text{treated_village}_l \times \text{school_age}_{l,c} \right) + \alpha_l + \alpha_c + \epsilon_{i,l,c} \tag{2}$$

where $y_{i,l,c}$ is an outcome for individual i from village l and birth cohort c, treated_village_l is an indicator equal to one for villages located within 5 km of a Swedish-only school opening, and $\text{school_age}_{l,c}$ is an indicator equal to one for individuals aged 3–14 at the relevant opening date. α_l are village fixed

effects and α_c are cohort fixed effects, accounting for time-invariant differences across villages and systematic differences across cohorts.[15] The identifying assumption is parallel trends, which in this setting requires that, in the absence of Swedish-only schools, outcomes across cohorts would have evolved similarly in treated and control villages. The coefficient of interest β can be interpreted as the causal effect of exposure to the introduction of Swedish-only schools under the parallel trends assumption. I cluster standard errors at the village level throughout. The results are robust to allowing for spatial correlation in the error terms using the method of Conley (1999).

Outcomes are measured in the 1910 and 1930 censuses, either separately or combined. Individuals in the estimation sample are 18–43 years old in 1910 and 38–63 years old in 1930, allowing me to observe outcomes in adulthood.

Because Swedish-only schools were introduced at different points in time (1889 and 1895), relying on a two-way fixed-effects model to estimate Equation (2) can bias the estimates in the presence of heterogeneous treatment effects. To alleviate this concern, I use a stacked difference-in-differences approach to estimate the model (Wing et al., 2024).[16] I construct a separate dataset for each school-opening wave, comparing treated villages with eligible control villages, and then append these datasets. The resulting specification includes stack-specific village and cohort fixed effects. To obtain an estimand with a clear interpretation, Wing et al. (2024) suggest using a weighting scheme. I follow their approach, and therefore my estimated effects can be interpreted as the average treatment effect on the treated (ATT), referred to in the paper as the "trimmed aggregate ATT."

In an alternative specification, I replace the school-age indicator in the treatment interaction with the number of years treated, thereby allowing the effect to vary with treatment intensity. This is useful because individuals aged 7–14 are already of school age when the school opens and are therefore exposed for fewer years, implying a potentially weaker treatment, whereas individuals aged 3–6 are below school age at the time of the opening and are perhaps more likely to experience a stronger treatment effect, as their entire period of school eligibility occurs under the new schools. One year corresponds to a school opening when an individual is age 14, while the maximum of twelve years corresponds to exposure

[15] For ease of exposition, I suppress the stack index in Equation 2. As described below, Swedish-only schools were introduced in different waves, in 1889 and 1895, and I therefore implement the specification using a stacked difference-in-differences design. In the stacked specification, $\text{school_age}_{l,c}$ is defined relative to the opening year of the relevant stack, and the village and cohort fixed effects are stack-specific.

[16] Several approaches have been developed to address treatment-effect heterogeneity in staggered difference-in-differences designs (Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). The stacked approach is convenient in my setting because treatment is assigned at the village level, outcomes are observed at the individual level, and the selection of control villages with replacement requires the use of weights. For a similar application, see, for example, Bentzen et al. (2026), who use a stacked design with individual-level weights and repeated cross-sectional data, which is comparable to a cohort difference-in-differences design where age serves as the time dimension.

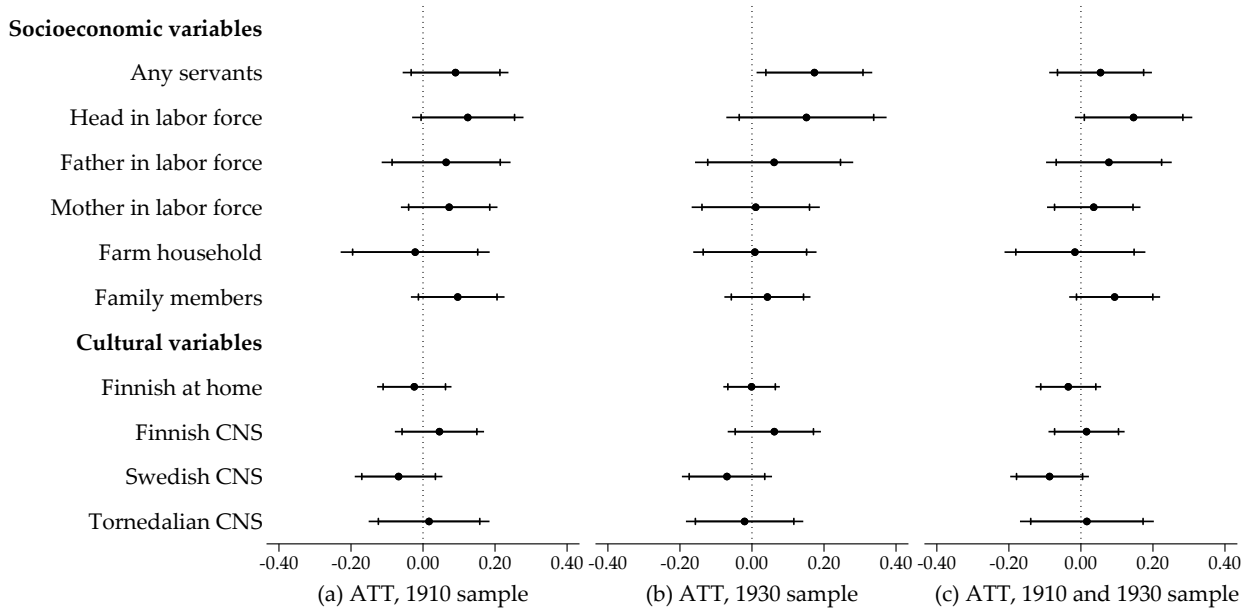
beginning at age 3.

I also present event-study versions of Equation 2 to illustrate the dynamic effects of exposure and to evaluate pre-trends. In this specification, treatment age is binned into four-year intervals to retain statistical power.

5.1.2 Testing the identifying assumption

To increase confidence in the identifying assumption, I conduct placebo tests by estimating Equation (2) using characteristics of the individual and his/her family measured before the opening of the Swedish-only schools. If cohorts are evolving in parallel, I should find no effects on characteristics determined before exposure once village and cohort fixed effects are included. Figure 5 shows the results of this exercise.

FIGURE 5
Placebo



Notes: The figure reports placebo estimates using pre-treatment characteristics measured in the 1890 census as outcomes. Panel (a) uses the sample linked to the 1910 census, Panel (b) uses the sample linked to the 1930 census, and Panel (c) combines the 1910 and 1930 samples. For the Finnish, Swedish, and Tornedalen Cultural Name Scores, the sample is additionally restricted to individuals with normalized age greater than four, since younger individuals were not yet born in 1890. All specifications include village-stack and age-stack fixed effects. Outcomes are standardized to have mean zero and unit standard deviation for ease of comparison. The figure includes 90% and 95% confidence intervals. Standard errors are clustered at the village level.

Panel (a) presents results for individuals linked to the 1910 census, Panel (b) for individuals linked to the 1930 census, and Panel (c) for individuals linked to either or both the 1910 and 1930 censuses. All coefficients are standardized to have mean zero and standard deviation one to facilitate comparability across outcomes. Importantly, treated individuals are not more likely to use Finnish at home, nor are

they more likely to have Swedish, Finnish, or Tornedalian names, as measured by the CNS. In addition, treated individuals are not more likely to live in farm households, have larger families, or have a mother or father participating in the labor force in 1890.

However, in the sample linked to the 1930 census, the likelihood of having servants in 1890 is statistically significant at the 5 percent level, which is potentially a threat to the identification strategy. At the same time, one would expect some estimates to be statistically significant at conventional levels simply by chance. Nevertheless, to address concerns that this imbalance may drive the results, all regressions using only the 1930-linked sample control for any servants in 1890 and allow age effects to differ flexibly by this variable.

5.2 Community responses to Swedish-only school openings

To shed light on the impact of Swedish-only school openings on the community at large, I use two complementary empirical specifications. The first examines the immediate cultural response to school openings using names given to children, while the second examines how communities evolved over subsequent decades in terms of language use and educational provision.

To examine the immediate cultural response, I analyse naming patterns before and after the opening of a Swedish-only school. I estimate Equation 3:

$$\text{CNS}_{i,l,c} = \sum_{j \neq [1,2]} \beta_j \left(\text{treated_village}_l \times \text{age_at_opening}_{j(l,c)} \right) + \alpha_c + \alpha_l + \alpha_f + \epsilon_{i,l,c}, \tag{3}$$

where $\text{age_at_opening}_{j(l,c)}$ are indicator variables for the age of an individual in village l and cohort c relative to the Swedish-only school opening, with ages grouped into two-year bins to retain statistical power. The age-at-opening indicators are defined for individuals in both treated and control villages, while their interaction with treated_village_l captures exposure to the school opening. α_l are village fixed effects, α_c are cohort fixed effects, and α_f are family fixed effects (sibling fixed effects).[17] The family fixed effects absorb time-invariant differences across families, such that identification comes from comparing siblings born at different times relative to the opening. The omitted category consists of children aged 1–2 at the time of the school opening. I consider children aged up to eight years when the school opened and children born up to five years after the opening (age -5 at school opening). This analysis is conducted using the matched-villages sample, and outcomes are measured in the 1900 census.[18]

[17] Technically, village fixed effects are redundant when family fixed effects are included, but I keep them in the specification for clarity and because I later drop the family fixed effects in alternative specifications.

[18] As before, for ease of exposition, I suppress the stack index in Equation 3. As described earlier, Swedish-only schools were

To examine how communities evolved over subsequent decades, I implement a village-level difference-in-differences design. I leverage the fact that I observe villages in 1880, 1890, 1900, and 1910. I use 1880 and 1890 as the pre-treatment period and 1900 and 1910 as the post-treatment period and estimate the following regression:

$$y_{l,t} = \alpha_l + \alpha_t + \beta \left(\text{treated_village}_l \times \text{post}_\text{t} \right) + \varepsilon_{l,t} \tag{4}$$

where $y_{l,t}$ is the outcome in village l in decade t. The main outcomes are the share of individuals using Finnish at home, an indicator for the presence of a Swedish teacher within 5 km, and an indicator for the presence of a Finnish teacher within 5 km. α_l are village fixed effects, and α_t are decade fixed effects. treated_village_l indicates whether village l was treated, and post_t is an indicator equal to one in 1900 and 1910. The coefficient of interest is β. Standard errors are clustered at the village level.

6. RESULTS

6.1 Individual effects of exposure to Swedish-only schooling

6.1.1 Education, income and language use at home

In this section, I examine whether exposure to Swedish-only schooling increases the uptake of primary education, education above the primary level, income, and language use at home, proxied by whether the individual is recorded as "Finnish" by census enumerators.[19]

Column (1) of Table 2 shows that there is no average effect on the uptake of primary education following the introduction of the Swedish-only schools, using either the main specification in Panel A (Equation 2) or years treated instead of a simple treatment dummy in Panel B. However, Panel C shows an augmented specification that interacts treatment with an indicator for pre-existing educational supply, proxied by the presence of a teacher within 5 km prior to the Swedish-only school opening. This interaction is important, as I cannot detect take-up of Swedish-only schooling in areas with prior schooling if students simply switched from one school to another. In contrast, areas without a nearby teacher prior to the opening provide a clearer indication of whether individuals attended the new schools. In areas without a teacher within 5 km, the uptake of primary education increased by 14 percentage points

introduced in different waves, in 1889 and 1895, and I therefore implement the specification using a stacked difference-in-differences design. In the stacked specification, $\text{age_at_opening}_{j(l,c)}$ is defined relative to the opening year of the relevant stack, and the fixed effects are stack-specific.

[19] Examining the uptake of primary education serves both as a measure of human capital and, since I do not have actual attendance records for Swedish-only schools, as a proxy for the first stage capturing the take-up of Swedish-only education.

from an unconditional mean of 87 percent.

TABLE 2
Swedish-only schooling, education, income and language use at home

	Any education (1)	Further education (2)	Any income (3)	Log income (4)	Finnish at home (5)
Panel A.					
Treated	0.041	-0.009	-0.026	-0.007	-0.014
	(0.048)	(0.024)	(0.037)	(0.096)	(0.034)
Panel B.					
Years treated	-0.001	-0.001	-0.001	0.003	-0.000
	(0.004)	(0.002)	(0.003)	(0.014)	(0.003)
Panel C.					
Treated	0.140***	0.004	-0.024	0.034	-0.011
	(0.053)	(0.022)	(0.046)	(0.150)	(0.047)
Treated × Teacher before	-0.162***	-0.020	-0.005	-0.063	-0.005
	(0.040)	(0.040)	(0.062)	(0.158)	(0.038)
Village-Stack FE	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓
Dep. var. mean	0.869	0.081	0.438	7.241	0.443
N	4,163	4,163	4,163	1,917	5,133

Notes: The education and income variables are measured in 1930 and the language variable in 1910. Further education refers to above primary education. The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 and the 1910 or 1930 censuses, and restricted to the matched village sample using weights reflecting matching with replacement. As the 1930 census is incomplete, weights accounting for this are also included for relevant outcomes. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). *Teacher before* is an indicator equal to one if there was a teacher within 5 km of the village of residence in 1890. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Turning to human capital accumulation more broadly, the effect on human capital is ex ante ambiguous, even conditional on uptake of Swedish-only schooling. For example, human capital may have decreased if cultural resistance or the language component constrained the extent to which children benefited from and responded to the Swedish-only schools. Conversely, human capital may have improved if Swedish-only schools were better organized than existing alternatives or if Swedish teachers had better training.

Columns (2)–(4) of Table 2 show small and statistically insignificant average effects of Swedish-only schools on educational attainment above the primary level and on income. This is the case both when using the main specification and when considering years of treatment, which may be particularly relevant for human capital outcomes. Results are reported pooling genders.

To increase confidence in the identifying assumption, I report event-study plots for the human capital outcomes in Appendix Figure B1. Although the estimates are relatively imprecise, they do not indicate systematic pre-trends.

As discussed, pre-existing educational supply may mask the effects on human capital. For example, in locations with pre-treatment educational supply, the treatment effect may reflect a change in the potential quality of education, whereas in places without prior supply, the counterfactual is the absence of formal schooling nearby. However, as evident in Panel C, there is again no evidence supporting effects on human capital.

In the Appendix, I report results separately by gender in Tables B1 and B2, as economic opportunities differed substantially between men and women at the time.[20] For women, I find a statistically significant reduction in log income in areas with no prior schooling; however, this estimate is based on the small subset of women with recorded positive income. More interestingly, the effect on the uptake of primary education is stronger and only statistically significant for men, indicating that men were more responsive to the Swedish-only school openings.[21]

Turning to language use at home, Column (5) shows no statistically significant average effect on whether Finnish was spoken in the household under either specification. This is consistent with limited effects on language skills. More generally, the absence of improvements in further education and income already suggests that there were no major improvements in language skills. Substantial improvements in Swedish language skills could arguably be expected to increase educational and labor market opportunities by improving access to both, as well as affecting language use at home.

As an additional test for human capital improvements, I examine occupational upgrading using the main specification in Equation (2), both pooling genders and separately by gender.[22] The results are reported in Appendix Table B3 and show no indication of occupational upgrading or downgrading following treatment.

What might explain the limited evidence of human capital effects? First of all, there might be human

[20] This is evident from the historical context, but also from the unconditional means for having any income: 10 percent for women compared to 75 percent for men, based on the share with income reported in the tax registers.

[21] This does not seem to be driven by women in general being less likely to take up education, as the unconditional mean is comparable across genders, at 87 percent for both.

[22] Using occupational classifications based on HISCLASS, I investigate whether treatment changes the likelihood that individuals work as farmers and fishermen or in higher-skilled, medium-skilled, lower-skilled, and unskilled occupations (van Leeuwen and Maas, 2011). The 1910 census from IPUMS contains occupational codes that can be converted to HISCLASS, whereas the 1930 census does not. Suvi Heikkuri kindly provided hand-coded HISCO codes for the 1930 census, which I use for this analysis. I also examine a continuous measure of occupational skill based on an ordinal ranking of these skill categories. As individuals might be selected into having a reported occupation, which could bias the analysis, I also test whether treatment affects the likelihood of having an occupation that can be classified according to HISCLASS and find no indication of differential selection.

capital effects that are simply too small to be detected with these data. Another potential reason is that primary schooling during this period was neither sufficient nor relevant to altering these outcomes. Finally, the counterfactual is not necessarily no schooling, but schooling in one's mother tongue, which might mean that the education the children received was not more efficient but rather hindered learning.

6.1.2 Social integration

Exposure to Swedish-only schooling increased social integration within the region, as reflected in marriage patterns and residential mixing. Table 3 reports estimates from the main specification defined in Equation (2), presented in Panel A. Column (1) shows a negative but statistically insignificant effect on having a partner born in Sweden but outside Tornedalen (Swe. excl. Torn.). Columns (2) and (3) show that treated individuals are more likely to have a partner born in Tornedalen and less likely to have a partner born in Finland, respectively. The latter effect is especially large, corresponding to a reduction of 6.3 percentage points relative to a mean of 9.1 percent. Together, these results suggest that the reform increased social integration within the region, primarily through a shift from Finland-born partners to partners born in Tornedalen, rather than through increased marriage with individuals from other parts of Sweden. Column (4) reports effects on ethnolinguistic fractionalization in the village of residence in adulthood. Treated individuals are more likely to live in ethnolinguistically mixed villages, providing further evidence of increased social integration.[23] The alternative continuous specification presented in Panel B, using years of exposure rather than a binary treatment indicator, yields qualitatively similar results and indicates that the effects increase with the length of exposure.

Two potential concerns for the identification of the effects on social integration are differential pre-trends and selection into partnership. Appendix Figure B2 reports event-study estimates, which do not indicate systematic pre-trends and are consistent with effects that increase with the length of exposure. Furthermore, Appendix Table B4 shows no statistically significant effect on the likelihood of having a partner, suggesting that the marriage results are not driven by differential partnership formation.

I also address a concern regarding the interpretation of the social integration results, namely whether the increase in Tornedalen-born partners reflects greater integration across ethnolinguistic groups within the region or a shift toward partners with stronger Swedish attachment. While the estimated effect on partnerships with individuals born in Sweden outside Tornedalen is negative, it is not statistically significant, and birthplace alone does not capture ethnic attachment. Appendix Table B6 reports results for

[23] Appendix Table B5 shows no statistically significant effect of treatment on village-level ethnolinguistic shares, measured as the share of individuals in one's village of residence who were born in Finland or classified as belonging to the minority in 1910. This suggests that the fractionalization results are not capturing changes in minority shares.

TABLE 3
Swedish-only schooling and social integration

	Partner born Swe. excl. Torn. (1)	Partner born Tornedalen (2)	Partner born Finland (3)	Village fractionalization (4)
Panel A.				
Treated	-0.026	0.077**	-0.063**	0.021**
	(0.027)	(0.037)	(0.025)	(0.010)
Panel B.				
Years treated	-0.003	0.008**	-0.007***	0.002**
	(0.004)	(0.004)	(0.003)	(0.001)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	0.077	0.846	0.090	0.128
N	4,069	4,069	4,069	5,700

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

predicted Finnish attachment and ethnolinguistic mixedness of partners, as described in Section 4. I find no statistically significant effects on Finnish attachment under either the main specification or the specification using years of treatment. In contrast, the effects on ethnolinguistic mixedness are positive under both specifications, although statistically significant only at the 10 percent level in the main specification. These findings are consistent with increased social integration across ethnolinguistic groups within the region.

A natural question in light of these results is the role of migration. To examine this, I estimate the effect of treatment on the probability of remaining in the 1890 village of residence, migration distance, and the probability of leaving Tornedalen. The results are presented in Table B7 in the Appendix. Treated individuals are 8 percentage points more likely to remain in their 1890 village, compared with an unconditional mean of 47 percent, as shown in Column (1). Treated individuals also migrate shorter distances, as reported in Columns (2) and (3), using migration distance in kilometres and its logarithmic transformation, respectively. There is also no evidence that treatment affected migration out of Tornedalen on average, as reported in Column (4). When years of treatment are used as the explanatory variable instead, the coefficients are generally in the same direction as the binary treatment estimates but are statistically indistinguishable from zero, providing limited evidence that the effects increase linearly with

additional years of exposure.[24] These results suggest that social integration was not driven by treated individuals migrating.

Taken together, the results point to greater social integration, particularly between ethnolinguistic groups within the region. This is reflected in greater ethnolinguistic mixing, both in terms of partner choice and residence choice.

6.1.3 National identity

Exposure to Swedish-only schooling shifted expressions of national identity toward Finnish and away from Swedish, as measured by names given to children and employment in the military. Table 4 presents the results estimated using the main specification defined in Equation (2). Columns (1) and (2) of Panel A show an increase in the Finnish CNS of 1.5 and a decrease in the Swedish CNS of 1.24, corresponding to changes of approximately 7.5 and 6.1 percent relative to their respective unconditional means. Column (3) reports the results for the Tornedalian CNS. The effect is positive but statistically insignificant, suggesting that the shift is primarily toward Finnish rather than regional naming. These results on naming practices indicate a stronger transmission of Finnish identity relative to Swedish identity to the next generation among treated individuals. Column (4) shows a decrease in the likelihood of working in the military, providing additional support for the interpretation that the effects pertain to a reduction in Swedish national identity. The alternative continuous specification presented in Panel B yields qualitatively similar results and suggests that the effects become stronger with the length of exposure to Swedish-only schools.

Two potential concerns related to the identification of the effects on national identity are differential pre-trends and selection into having children. Appendix Figure B4 reports event-study estimates, which do not indicate systematic pre-trends and are consistent with effects that increase with the length of exposure. Furthermore, Appendix Table B8 shows no statistically significant effect on the likelihood of having a child, suggesting that the naming results are not driven by differential fertility.

The preferred CNS differs from the standard approach by accounting for the full set of cultural groups present in the region. Specifically, the denominator is constructed as the sum of the name's prevalence across each alternative cultural group rather than by pooling all non-target groups into a single comparison category. This distinction is particularly relevant in Tornedalen, where several culturally distinct populations coexist and naming traditions overlap across groups. By summing group-specific

[24] Event-study estimates reported in Figure B3 similarly suggest that the effects appear already among individuals with relatively limited school-age exposure and do not display a clear gradient with the number of years exposed, while also providing little evidence of differential pre-trends.

TABLE 4
Swedish-only schooling and national identity

	Finnish CNS (1)	Swedish CNS (2)	Tornedalian CNS (3)	Military (4)
Panel A.				
Treated	1.539**	-1.237**	1.057	-0.035**
	(0.707)	(0.509)	(0.774)	(0.014)
Panel B.				
Years treated	0.224**	-0.154**	0.120	-0.001
	(0.092)	(0.075)	(0.078)	(0.001)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	20.520	20.458	26.704	0.029
N	3,925	3,925	3,925	3,071

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

prevalences, the measure effectively assigns equal conceptual importance to each cultural group, regardless of its population size, and captures the distinctiveness of a name within the broader cultural landscape. Nevertheless, Table B9 and Figure B5 show that the results are robust to using the Swedish CNS relative to all other cultural groups pooled into a single comparison category.

Taken together, the results show that although exposure to Swedish-only schooling increased social integration within the region, including a shift in partner choice away from individuals born in Finland, this was not accompanied by a strengthening of Swedish national identity. Instead, expressions of national identity shifted away from Swedish and toward Finnish. These changes occurred despite limited evidence of improvements in human capital and labour market outcomes and illustrate that social integration can occur without a corresponding shift toward majority identity.

6.2 Robustness and threats to identification

Throughout the paper, I have discussed outcome-specific robustness checks and threats to identification. This section addresses additional threats to identification and summarizes further robustness checks. Specifically, I assess whether differential emigration from Sweden poses a problem, examine whether concurrent policies could explain the estimated effects, and evaluate the robustness of the results to sample selection and alternative approaches to statistical inference.

6.2.1 *Emigration*

A main concern with the causal interpretation of the results is differential emigration from Sweden by treatment status, as emigrants to other countries leave my sample. In general, my goal is to estimate the causal effects within my sample of stayers and not the causal effects for both stayers and emigrants, as I do not have enough information on emigrants to measure outcomes for them. My target group (stayers) is an interesting group to study by itself, as presumably this is the group that policymakers care most about. However, if there is differential emigration in response to my treatment, my estimates would be biased.

To assess this concern, I link a sample of Swedish emigrants to the 1890 pre-treatment census and construct an indicator distinguishing individuals identified as emigrants from those observed in the 1930 census, allowing me to examine whether treatment affected the likelihood of emigration. The number of individuals in this sample corresponds closely to the aggregate number of individuals reported to have emigrated from the region, as shown in Appendix Section A.3.[25] Table B10 in the Appendix reports the effects on emigration. I find no statistically significant effects on total emigration (to any country) or emigration to Finland, which alleviates concerns about selective outmigration.

6.2.2 *Other concurrent events*

A key assumption for a causal interpretation is that no other events coinciding with the Swedish-only school openings differentially affected individuals aged 3–14 relative to those aged 15–22. To the best of my knowledge, there is no salient event that coincides with the staggered introduction of the schools. However, two important developments during this period could potentially have affected my main outcomes.

The first is the connection of the railroad to the mine in Gällivare in 1887, which led to rapid industrialization and in-migration and could therefore have affected national identity and intergroup relations. Although this development does not have an obvious age gradient, it may have affected younger and older individuals differently. To assess this concern, I calculate the distance from each municipality to Gällivare and interact it with an indicator for being aged 3–14 at the time of the school opening. I find that proximity to Gällivare is, at times, differentially associated with the outcomes of younger individuals. However, including this interaction in the main specification leaves the estimated effects of exposure

[25] It is important to note that I do not observe all migrants, only a sample. However, under the assumption that inclusion in the sample is orthogonal to treatment, I can still identify the causal effect, as the measurement error would be random. However, the unconditional mean is not necessarily informative, and comparing the estimated effect to the unconditional mean is therefore not meaningful.

to Swedish-only schooling qualitatively unchanged across the outcomes considered. The results are reported in Table B11.

During this period, Russia was also intensifying its assimilation policies in Finland, which may likewise have affected cohorts differently. I therefore repeat the analysis using distance to the Finnish border, implicitly assuming that areas closer to Finland were more exposed to developments associated with Russification. Again, accounting for differential exposure leaves the estimated effects of Swedish-only schooling qualitatively similar. The results are reported in Table B12 in the Appendix.

Interacting with distance to the Finnish border is also useful for addressing a related concern. The ease of crossing the border may have changed over time for reasons unrelated to the school reform, potentially affecting opportunities to marry someone from Finland differently across cohorts. As noted above, controlling for differential exposure by distance to the border leaves the estimated effect qualitatively similar, although it is statistically significant only at the 10 percent level. This attenuation may reflect either the relevance of this concern or the loss of identifying variation when allowing for differential effects by proximity to the border.

6.2.3 Additional robustness checks

With respect to the sample, Table B13 shows that the results are robust to excluding individuals aged 3–4 at the time of treatment. Assessing robustness to excluding this age group is important, as these individuals are relatively young for some outcomes and are linked to the 1890 census through their parents. However, ages 3–6 constitute the fully treated group, so excluding ages 3–4 removes half of the fully treated cohorts and therefore reduces statistical power.

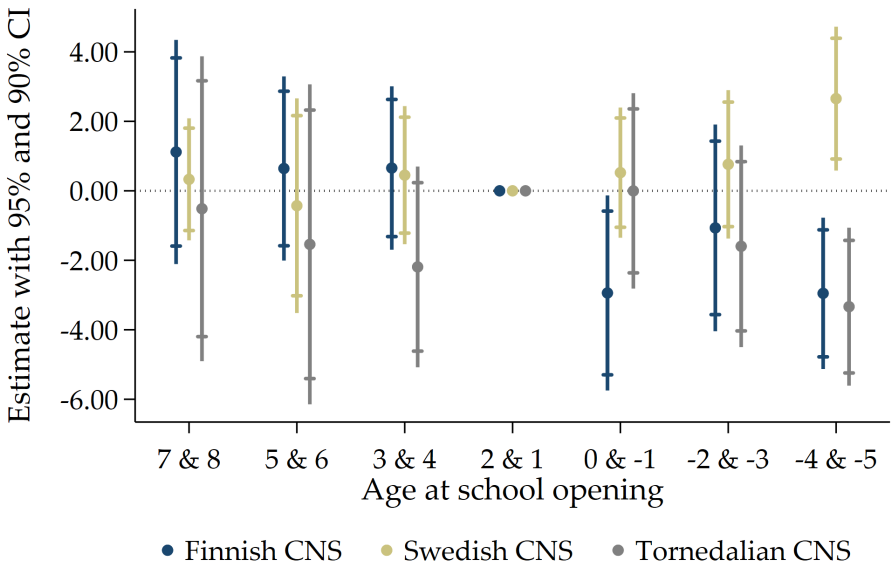
Finally, I show robustness to correcting for spatially correlated standard errors as proposed by Conley (1999) in Table B14.

6.3 Effects of Swedish-only school openings on the community at large

This section shifts the focus from individual exposure to Swedish-only schooling to the effects of school openings on the broader community. This analysis is largely motivated by the possibility of cultural resistance. As discussed in the conceptual framework in Section 3, such resistance may arise among parents or the community at large, suggesting that not only individuals directly exposed to Swedish-only schooling but also communities exposed to the mere opening of a school may respond to the reform.

First, I examine the naming response to the opening of a school to gain insight into the early cultural response of the community. I compare names given to children born just before a school opening with

FIGURE 6
CNSs around school opening



Notes: The sample consists of individuals born between eight years before and five years after the opening of a Swedish-only school, observed in the 1900 census, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from an event-study specification in which indicators for age at school opening are interacted with an indicator equal to one if an individual is born in a village that experiences a Swedish-only school opening; the outcome is the Swedish or Finnish Cultural Name Score (CNS). Age at school opening is binned in two-year intervals, with ages 1–2 as the reference category. The specification includes village, cohort, and sibling fixed effects. Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

those given to children born just after by estimating the event-study specification in Equation 3. The results are shown in Figure 6. In contrast to the children of individuals exposed to schooling, children born after school openings were given more Swedish names and less Finnish or Tornedalian names.[26] This pattern suggests that the identity response observed among individuals directly exposed to schooling is unlikely to reflect broad community-level opposition or backlash operating through increased cultural transmission. If increased cultural transmission were the underlying mechanism, we would expect the broader community to respond in the same direction as the individuals exposed to schooling. Instead, the opposing patterns are more consistent with individual-level responses arising from individuals' own experiences attending the schools. However, one possibility that cannot be completely ruled out is that parents of children attending the schools responded differently from parents in the broader community.

Changes in naming practices may partly reflect increased exposure to Swedish names rather than cultural change. Swedish-only schools may have increased exposure to Swedish names through in-

[26] I also report results from a corresponding continuous difference-in-differences specification in Table B15 in the Appendix, estimated both with and without sibling fixed effects and separately by gender.

migration, for example, if the schools brought teachers from outside the region.[27] However, such exposure would be expected to attenuate the estimated effects toward zero rather than generate effects in the opposite direction, and would therefore still weigh against a pure cultural-transmission mechanism. Importantly, my analysis includes sibling fixed effects, so the naming response is not driven by changes in the composition of families due to in-migration.

These contrasting responses motivate further examination of how communities evolved over time in terms of educational provision and language use at home. Changes in the local teaching environment provide insights into how educational provision responded to school openings, which schooling alternatives were available to children, and whether cultural resistance emerged through the opening of alternative schools. Changes in language use at home provide insights into the broader cultural consequences of school openings and exposure to schooling, which is particularly interesting given the contrasting findings.

Table 5 shows the results from estimating Equation 4, a village-level difference-in-differences analysis comparing treated and control villages before (1880 and 1890) and after (1900 and 1910) school openings. Again, we see no indication of cultural resistance. Treated villages are 29 percentage points more likely to have a Swedish teacher within 5 km but are not more likely to have a Finnish teacher within 5 km, as shown in Columns (1) and (2), respectively. I interpret this as evidence against the opening of alternative schools. Moreover, Column (3) shows that treated villages have a 10-percentage-point lower share of individuals recorded as "Finnish." To increase confidence in these results, I present event studies in Appendix Figure B6. I also show that the results are robust to using Conley standard errors instead of clustering at the village level, which may be particularly important for the teacher outcomes; see Table B16.[28]

Taken together, the analysis of school openings provides insights beyond suggestive evidence against a pure cultural-transmission mechanism underlying the individual exposure results. While individuals exposed to Swedish-only schooling shifted their expressions of identity away from Swedish and did not change their language use at home, the broader community moved in another direction: language

[27] I use names in the 1900 census, the closest available census that still allows me to evaluate naming responses relatively soon after school openings to alleviate this concern. Even so, there remains sufficient time for in-migration to have occurred.

[28] For completeness and transparency, I also include Table B17 in the Appendix, which reports corresponding effects on outcomes that are as comparable as possible to the individual-level measures of social integration and social identity: village fractionalization in terms of language use at home, marriage patterns, military employment, and name scores. Most effects are statistically insignificant, with the exception of the share employed in the military, which increases, in line with cultural assimilation at the village level. The statistically insignificant effects on naming and intermarriage at this level of analysis may reflect limited power to detect effects of the magnitude observed at the individual level for these outcomes, or the possibility that opposing responses across individuals result in little or no aggregate change in these measures.

TABLE 5
Village-level responses to Swedish-only schools

	Any Swedish teacher (1)	Any Finnish teacher (2)	Share Finnish at home (3)
Treated × Post	0.285***	-0.004	-0.102**
	(0.071)	(0.074)	(0.048)
Dep. var. mean	0.344	0.281	0.677
Observations	604	604	577
Villages	151	151	150
Village FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: The table reports estimates from a village-level difference-in-differences specification comparing villages before and after the opening of a Swedish-only school to villages that did not receive a Swedish-only school. The sample consists of villages observed in the 1880, 1890, 1900, and 1910 censuses. Treated × Post is an indicator equal to one for treated villages after the opening of a Swedish-only school. All specifications include village and census-year fixed effects. Standard errors are clustered at the village level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

use at home shifted toward Swedish, and expressions of identity, as measured by naming choices, became more Swedish. This suggests that the identity response among individuals directly exposed to Swedish-only schooling was not sufficient to overturn the broader Swedish-oriented cultural change at the community level. More generally, these contrasting responses highlight the importance of the unit of analysis when evaluating nation-building policies and the complexity of the dynamics they may generate. Both community-level analyses have important limitations, most notably that the results may partly reflect compositional change.[29] Nevertheless, they provide useful evidence on the broader consequences of school openings and help place the individual-level effects of exposure to Swedish-only schooling in context.

One possible explanation for these contrasting responses is that communities responded positively to the provision of state-funded schooling, perhaps because it reduced the local tax burden or increased perceived educational opportunities, consistent with evidence that government provision can strengthen national attachment (Caprettini and Voth, 2023). Such benefits may have encouraged greater adoption of Swedish language and culture at the community level, even as individuals directly exposed to Swedish-only instruction responded differently because of their experiences in the schools.

[29] Regarding compositional change, Table B18 reports village-level difference-in-differences estimates for the shares of the population born in Tornedalen, Finland, and Sweden outside Tornedalen. The results suggest that changes in birthplace composition do not drive the main findings. Compositional change with respect to language background could still contribute to the village-level shifts, but the results indicate that they are not simply driven by in-migration from more southern parts of Sweden outside Tornedalen.

7. MECHANISMS

This section explores the mechanisms underlying the individual-level responses to exposure to Swedish-only schooling discussed in the conceptual framework in Section 3. Before turning to these mechanisms directly, I evaluate the role of minority attachment, measured by language use at home, at both the community and family levels. This helps identify who responds to the reform and provides insight into the underlying mechanisms. Group size, in particular, plays an important role in theories of intergroup contact and cultural transmission. I then discuss alternative mechanisms in light of the results presented so far, first for social integration and then for national identity.

7.1 The role of minority attachment

I begin by analysing whether the effects on social integration and national identity depend on family-level attachment to the minority before treatment, as reflected in language use. Figure 7 reports standardized treatment effects separately for individuals from families recorded as "Finnish" before treatment and those that were not. For intergroup relations, both groups respond to the reform. For identity, the effects on the Swedish CNS and military employment are almost entirely driven by individuals from families that used Finnish at home before the reform. The Finnish CNS increases for both groups.

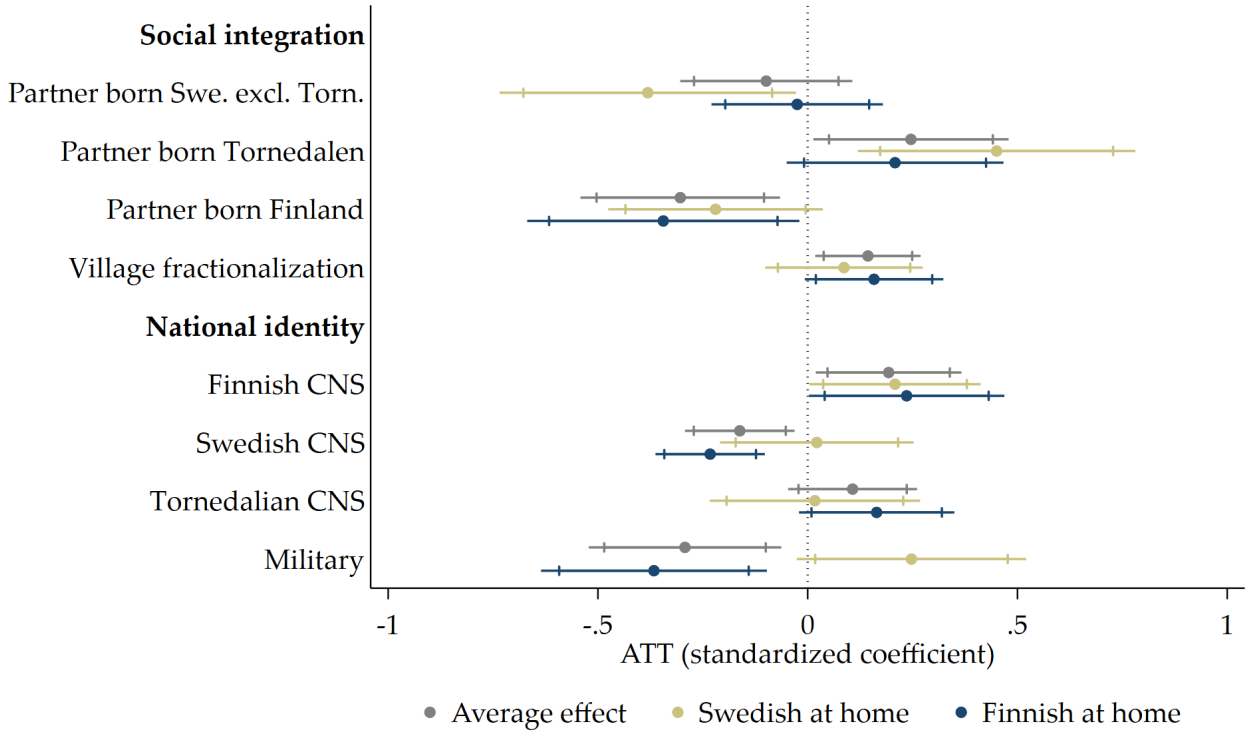
I next examine heterogeneity by community-level minority attachment. Specifically, I divide communities according to whether the pre-treatment share of individuals using Finnish at home around each individual's village of residence was above or below the median. Figure 8 reports the results using radii from 1 to 10 km.

Panel (a) reports the results for social integration. A fairly consistent pattern emerges: the effects tend to be stronger in communities with a below-median share of individuals using Finnish at home. Panel (b) reports the corresponding results for the social identity outcomes. The effects are generally larger in areas with weaker minority attachment, although for some outcomes there is little systematic difference between areas above and below the median.

These patterns help distinguish the results from two alternative interpretations of the results. First, the identity responses are unlikely to simply reflect increased signalling of ethnicity. One interpretation of the results could be that identity did not shift, but instead that signalling of ethnicity increased following a change in social integration. If one is socially integrated, one might want to signal to the other members of the ethnolinguistic group that one is still part of the group and/or the cost of signalling might have decreased after becoming visibly socially integrated. For example, Carvalho (2013)

FIGURE 7

Heterogeneity by pre-treatment language use at home

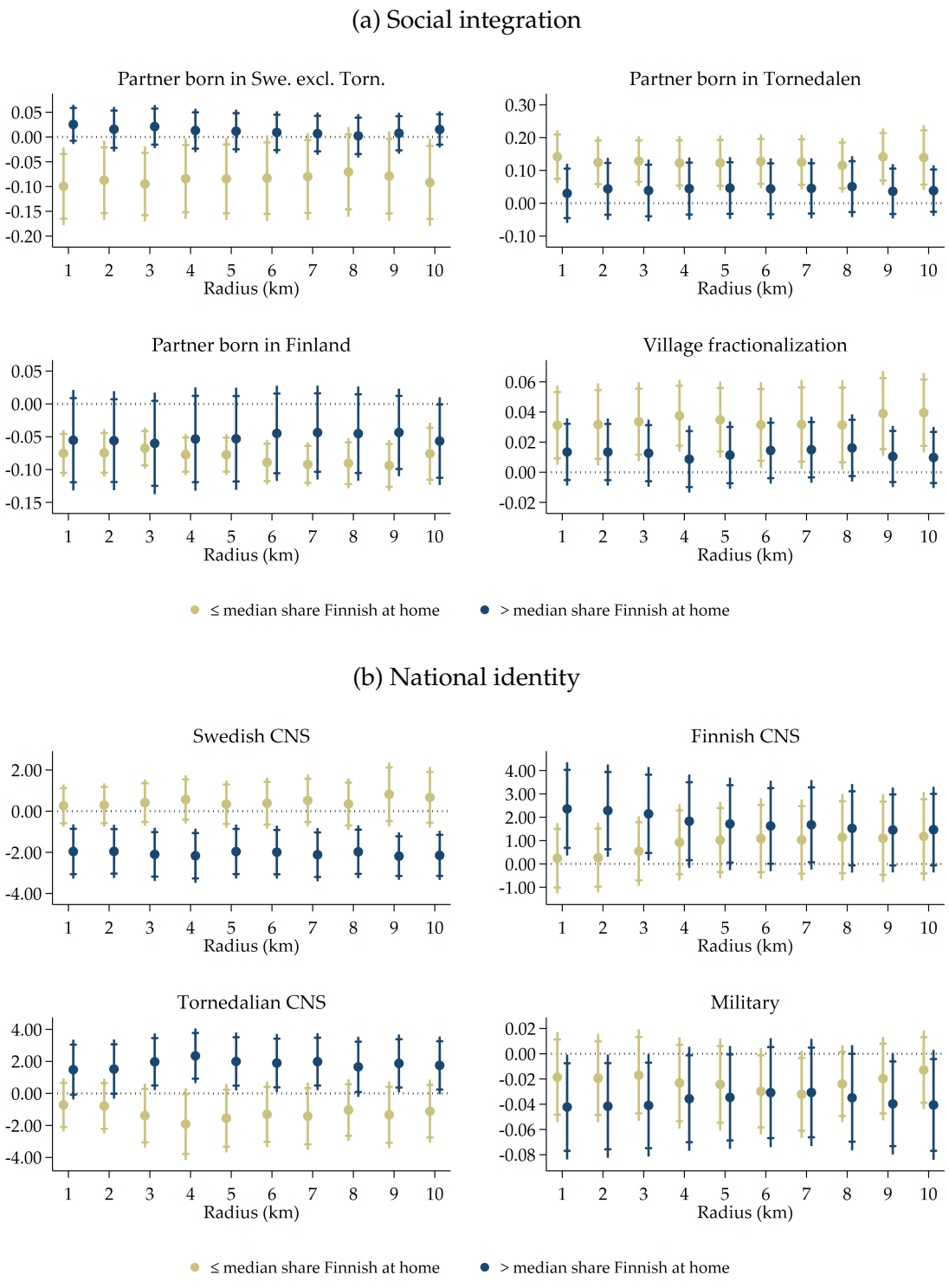


Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 censuses, and restricted to the matched village sample, with weights reflecting matching with replacement. The figure reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Treatment is defined as exposure to a Swedish-only school between ages 3 and 14. All regressions include village-stack and age-stack fixed effects. Outcomes are standardized to have mean zero and unit standard deviation. Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

discusses a response in that spirit when it comes to "Veiling". If this were the case, one would expect the strongest identity responses where increases in social integration were largest. Instead, the heterogeneity by community-level minority attachment shows that the two responses are not concentrated in the same places: increases in social integration tend to be stronger in communities with weaker minority attachment, while the identity responses do not follow the same pattern.

Second, the results are unlikely to reflect the emergence of a shared regional identity. For one, identity shifted primarily toward Finnish rather than regional attachment, although both moved in the same direction. Moreover, the heterogeneity results in Figure 8 indicate that the identity responses and increases in social integration are not concentrated among the same communities. Taken together, these patterns are difficult to reconcile with the idea that social integration was primarily driven by the emergence of a shared regional identity.

FIGURE 8
Heterogeneity by local share using Finnish at home



Notes: The figure reports treatment effects separately for individuals living in communities with above- and below-median pre-treatment shares recorded as "Finnish". Estimates are shown with 90% and 95% confidence intervals based on standard errors clustered at the village level.

7.2 Social integration

I now consider mechanisms underlying the effects on social integration. First, there is little evidence that improvements in human capital, broadly defined, or language skills explain the results. My proxy for language is, however, imperfect, so I continue discussing language as an alternative channel. As discussed above, a shared regional identity is also unlikely to provide an explanation. Instead, as discussed in the conceptual framework, the Swedish-only schools may have increased intergroup relations by bringing children from different linguistic backgrounds into common schools.

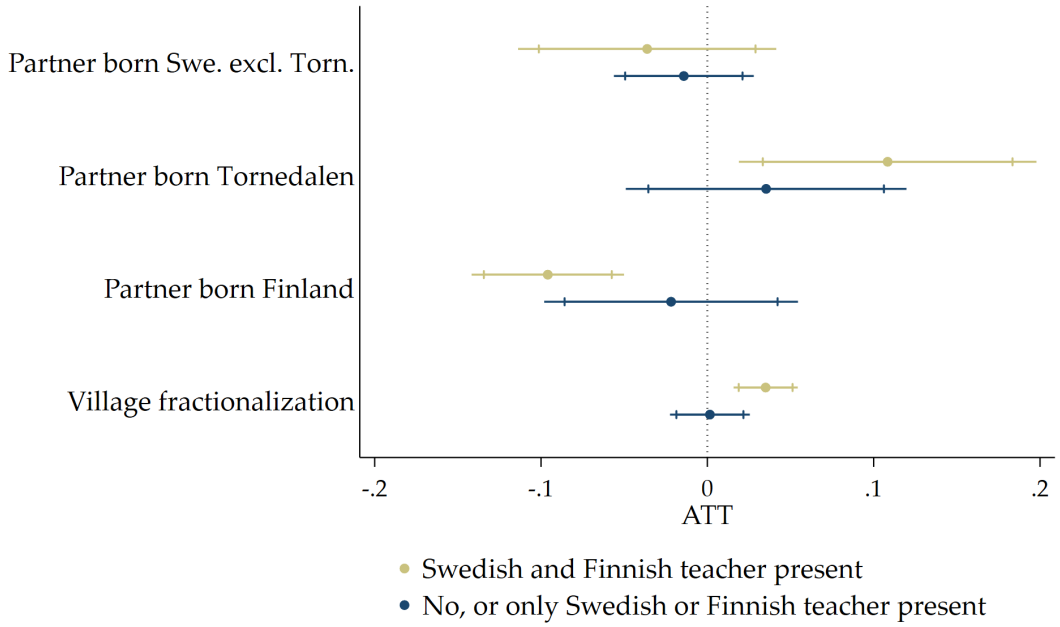
The heterogeneity by family-level language use at home in Figure 7 provides further support for increased intergroup contact in common schools rather than a pure language channel. The effects on social integration are not confined to families that were recorded as "Finnish" before treatment. This pattern is consistent with increased interaction between the groups, since children from both backgrounds would have been exposed to the change in the school environment. It is less straightforward to reconcile with improved Swedish-language skills as the primary mechanism. As discussed above, I interpret the recorded as "Finnish" measure as primarily reflecting language use at home before treatment. If improved Swedish-language skills were the primary mechanism, the largest responses should therefore be observed among individuals from families recorded as "Finnish" before treatment, who would be the ones most likely to improve their Swedish.

The heterogeneity by community-level minority ethnolinguistic attachment, presented in Figure 8, is also consistent with an important role for intergroup contact. If increased interaction in common schools is the main driver, there must be individuals from the other group with whom to interact. Given the strong minority presence throughout the region, communities with a lower share of individuals using Finnish at home provided greater opportunities for interaction between children with stronger and weaker attachments to the Finnish-speaking minority. Consistent with this, the effects on intergroup relations tend to be stronger in communities with below-median shares of individuals using Finnish at home. However, these are also places with weaker Finnish ethnolinguistic attachment more generally, which could directly affect how easily patterns of integration change.

A more direct prediction of intergroup contact being an important mechanism concerns the pre-existing educational environment. If the Swedish-only schools brought children together after they had previously been separated in schools by language, one would expect a larger response in places where both Swedish- and Finnish-language instruction were more likely to have been available before treatment. To test this, I investigate heterogeneity by whether both a Finnish and a Swedish teacher were

present within 10 km before treatment, classifying teachers based on whether they were recorded as "Finnish" by census enumerators. The results are shown in Figure 9.

FIGURE 9
Heterogeneity by types of teacher present within 10 km



Notes: The figure reports treatment effects separately by whether both a teacher recorded as "Finnish" and a teacher recorded as "Swedish" were present within 10 km of the individual's village of residence before treatment. Estimates are shown with 90% and 95% confidence intervals based on standard errors clustered at the village level.

The results show that social integration is stronger where both types of teachers were present before treatment. This is consistent with the reform having a larger effect where it represented a greater change in the extent to which children from different linguistic backgrounds attended common schools and is therefore in line with increased intergroup contact being a key mechanism. Importantly, Swedish-language skills may still have facilitated integration, even if they were not the main margin affected by the reform.

7.3 National identity

I next turn to the mechanisms underlying the national identity responses. In light of the results so far, I argue that the evidence is most consistent with the Swedish-only schools making the language component of ethnicity more salient, thereby strengthening Finnish identity and weakening Swedish identity. Before the reform, the practice was to receive schooling in one's mother tongue. With the reform, the implicit acceptance of linguistic differences was removed. Instead, by making Swedish the exclusive

language of instruction, the reform may have made language a more salient marker of ethnicity, altering the perceived distance to the Swedish and Finnish groups.

An alternative explanation is increased cultural transmission in response to the reform, as in Carvalho et al. (2024), Fouka (2020), and Bazzi et al. (2026). However, this channel is difficult to reconcile with the community-level responses, which show no signs of cultural resistance in naming patterns, the share of individuals using Finnish at home, or the presence of Finnish teachers.

The heterogeneity by family-level minority attachment provides a first piece of evidence relevant to these mechanisms. As shown in Figure 7, the responses in the Swedish CNS, the Tornedalian CNS, and military occupation are almost entirely driven by individuals from families using Finnish at home before the reform. The Finnish CNS, however, increases for both groups. This pattern is somewhat less consistent with a cultural-transmission mechanism, for which one might have expected Finnish naming to respond particularly strongly among families with greater initial minority attachment. The implications for social identity theory are less clear-cut. If the reform increased the salience of the distinction between Finnish and Swedish identity, it may have made the Finnish language and connections to Finland more salient for both groups, consistent with the increase in Finnish naming among both families using Finnish and those using Swedish. For individuals who were using Finnish at home, however, Swedish-only instruction may additionally have made their distinction from Swedish identity more salient. This could explain why responses indicating a movement away from Swedish identity are concentrated among individuals using Finnish at home.

The heterogeneity by community-level minority attachment provides a second piece of evidence. The cultural-transmission model commonly predict that identity responses should be stronger where the minority group is smaller. This is not what I find. However, this is not a perfect test because the group classified as having weaker minority attachment may also include individuals with a stronger majority identity who would not be expected to respond. Therefore, Figure B7 in the Appendix reports the same analysis conditional on the household using Finnish at home in 1890. For the name outcomes, the effects are similar below and above the median minority share, with the exception of the Swedish CNS, for which the effect is larger above the median at distances of around 5 km, which is not in line with cultural transmission. The results for military occupation are more difficult to interpret. The unconditional mean of military occupation is very low in the above-median group. Thus, although the estimates for the two groups differ at some distances in line with cultural transmission, the estimated effect for the above-median group is already of approximately the same magnitude as its unconditional

TABLE 6
Heterogeneity in Finnish naming by distance to a Swedish teacher

	Finnish CNS		
	(1)	(2)	(3)
Treated	0.084	0.097	0.940
	(0.781)	(0.830)	(1.616)
Treated × Distance to Swedish teacher	0.765***	0.777**	0.775**
	(0.256)	(0.308)	(0.309)
Treated × Distance to Finnish teacher		-0.028	-0.084
		(0.386)	(0.372)
Treated × Share Finnish at home			-1.001
			(1.778)
Village-Stack FE	✓	✓	✓
Age-Stack FE	✓	✓	✓
Dep. var. mean	20.520	20.520	20.520
N	3,925	3,925	3,925

Notes: The dependent variable is the Finnish CNS. Distance to a Swedish teacher is the log of one plus the distance to the nearest teacher recorded as "Swedish" before treatment, while distance to a Finnish teacher is the log of one plus the distance to the nearest teacher recorded as "Finnish". Share Finnish at home is the the share recorded as "Finnish" within 10 km of the individual's village of residence before treatment. All specifications include village-stack and age-stack fixed effects. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

mean, leaving limited scope for a larger negative response. Taken together, there is no consistent evidence that identity responses are stronger in areas with a smaller minority share, as predicted by the classic cultural-transmission model.

Finally, I examine whether the identity response depends on the pre-treatment presence of a Swedish teacher. If the salience mechanism is at play, we may expect that the presence of a teacher with stronger linguistic attachment to Sweden than the Finnish-speaking minority already present before treatment would attenuate the effects on Finnish naming, as the change would be less salient. I test this by interacting treatment with distance to a Swedish teacher (in logs), proxied by a teacher being recorded as "Swedish", and report the results in Table 6. In Column (1), I only include this interaction, but in Column (2), I also include an interaction with the distance (in logs) to a teacher recorded as "Finnish" to examine whether I am capturing distance to a teacher in general rather than to a Swedish teacher. In Column (3), I also interact treatment with the share of individuals using Finnish at home within 10 km to hold constant the strength of the minority attachment in the region, as this might be correlated with the presence of majority teachers.

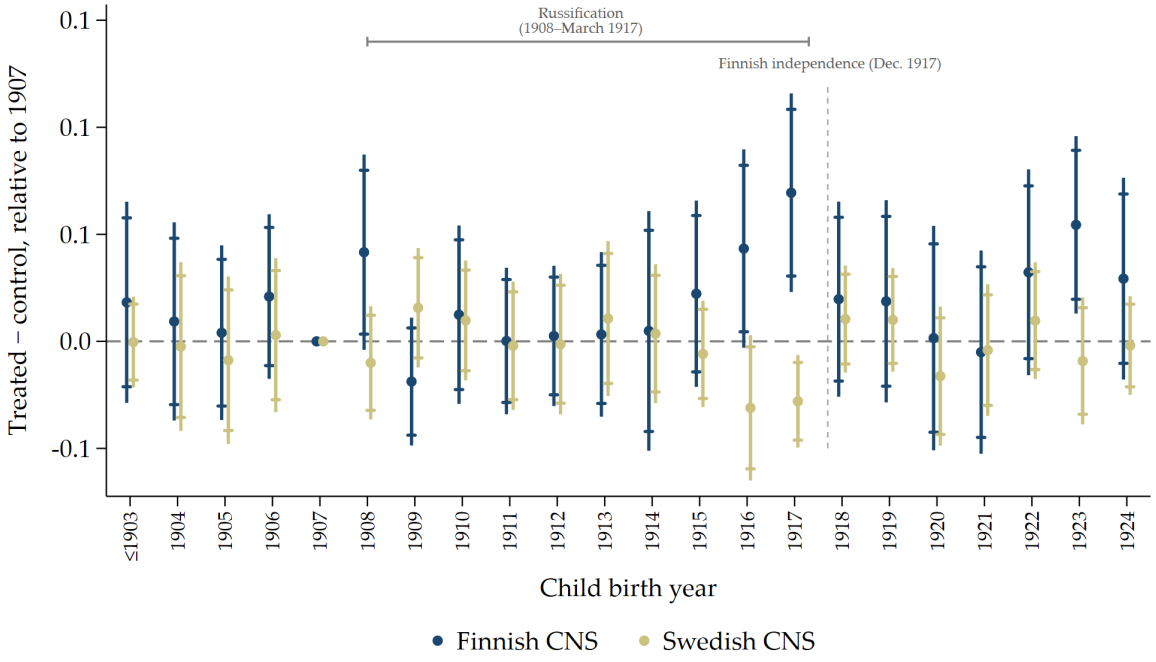
Across all specifications, I find that the effect on the Finnish CNS is larger the further away a Swedish

teacher was before treatment. I report results from Column (3) for all identity outcomes in Appendix Table B19. I do not find the same pattern across all outcomes, although the effects tend to go in similar directions for the name scores. As the Finnish CNS is my main measure of Finnish attachment, I nevertheless interpret these results as suggestive evidence in favor of the salience mechanism.

8. HETEROGENEITY

8.1 Heterogeneity by time: the potential role of Finnish independence

FIGURE 10
Children's CNSs by year of birth



Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 and the 1910 and/or 1930 censuses, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from a stacked event-study specification using the weights proposed by Wing et al. (2024), plotting the coefficients of treatment (Swedish school opened when aged 3–14) interacted with child birth year (the omitted category is 1907). The specification includes stack-specific cohort effects and family fixed effects. Standard errors are clustered at the village level.

This section explores heterogeneity in naming responses over time, examining whether the expression of identity changed during the period surrounding major historical events. As mentioned, Finland was an autonomous Grand Duchy within the Russian Empire rather than an independent nation-state when the Swedish-only schools were introduced. Between 1908 and 1917, Finland experienced a second period of Russification, during which the Russian government sought to reduce Finland's autonomy and strengthen imperial control. Following the February Revolution in Russia, the March Manifesto of

1917 restored Finland's constitutional autonomy, followed by Finland's declaration of independence on 6 December 1917 and the Finnish Civil War in 1918. World War I was also ongoing during this period, and historians emphasize the importance of both World War I and the Russian Revolution (1917) for Finland's declaration of independence, as these events weakened Russian rule and created the conditions for political separation (Alapuro, 1988). Taken together, these historical events may have affected the expression of Finnish national identity differentially among individuals exposed to the Swedish-only schools. The potential role of Finnish independence is especially interesting to explore.

To explore differential effects over time, I estimate an event-study specification by interacting treatment status with birth-year indicators of the treated individuals' children while including family fixed effects. The outcomes are the Finnish and Swedish child name scores (CNSs) of the children of treated individuals. As this analysis is exploratory, the choice of a reference cohort is necessarily somewhat arbitrary. In the figure shown, I use the 1907 birth cohort as the reference, as it is the last cohort born before the second period of Russification began. The estimates shown in Figure 10 suggest heterogeneity in the treatment effects. Specifically, the increase in the Finnish CNS and the decrease in the Swedish CNS are especially pronounced for cohorts born during the war years 1916 and 1917, suggesting that the period immediately preceding Finnish independence may have been particularly important for expressing Finnish identity.

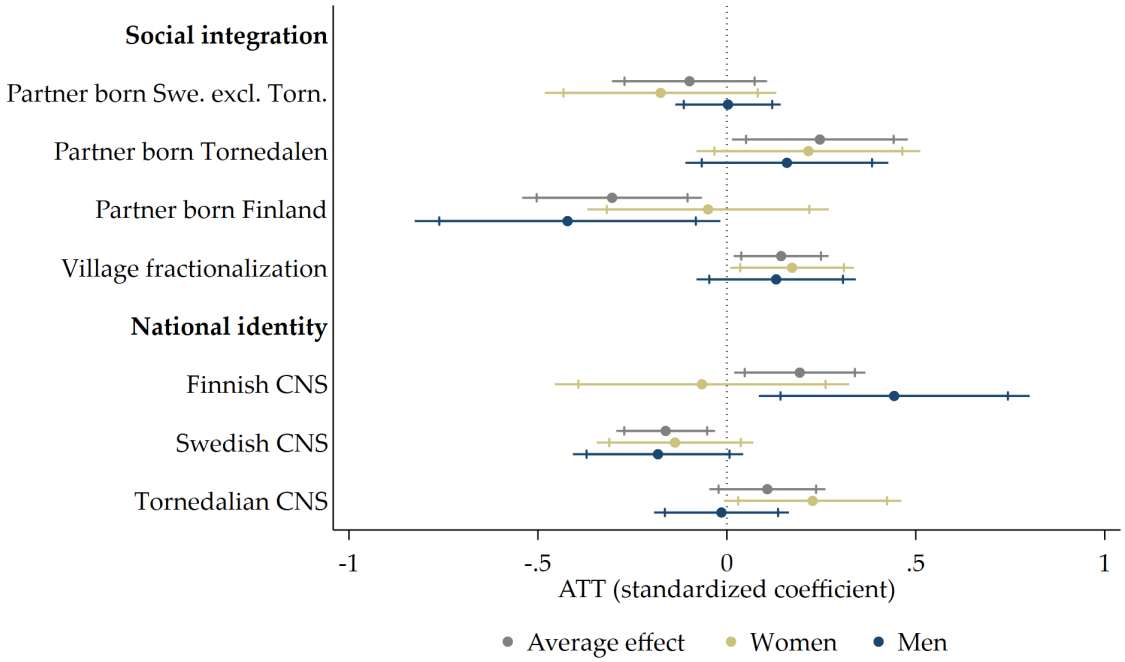
One possible interpretation is that the political events surrounding the collapse of Russian rule and Finland's transition to independence increased the salience of the ethnic divide and/or perceived status of Finnish national identity, thereby amplifying the expression of the identity effects of Swedish-only schooling through the names that treated individuals gave to their children. Related research on identity choice in the run-up to state splits along ethnolinguistic boundaries has similarly interpreted changes in consumption patterns as revealed social identity following increased ethnic salience (Atkin et al., 2021). Why this heightened expression does not persist is an interesting avenue for further research, but may suggest that salience, rather than changes in the relative status of Finnish identity, was the more important mechanism. The period leading up to independence may have been particularly important for the expression of Finnish identity. Alternatively, the Finnish Civil War may have complicated what the emerging Finnish nation-state represented and weakened its role as a focal point for Finnish national identity.

8.2 *Heterogeneity by gender*

The literature has highlighted that girls are at times seen as "keepers of the culture," a hypothesis that can also be tested in this setting (Dahl et al., 2022; Dion and Dion, 2001; Suárez-Orozco and Qin, 2006). Moreover, many historical studies have focused on men due to difficulties in linking women over time in census data, as they often change surnames. To contribute to this literature and provide additional insight into gender differences in historical contexts, I examine heterogeneous treatment effects by gender.

As discussed, interacting treatment with pre-treatment educational supply reveals that educational uptake increased in areas without prior supply and that these effects are driven by men, as the estimates are larger and only statistically significant for men. This already indicates a heterogeneous effect by gender.

FIGURE 11
Heterogeneity by gender



Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 censuses, and restricted to the matched village sample, with weights reflecting matching with replacement. The figure reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Treatment is defined as exposure to a Swedish-only school between ages 3 and 14. All regressions include village-stack and age-stack fixed effects. Outcomes are standardized to have mean zero and unit standard deviation. Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

I split the sample by gender and evaluate the effects of treatment on intergroup relations and identity. I report standardized coefficients of treatment on the various outcomes for the full sample and the

gender-specific samples in Figure 11. I find that statistically significant average effects are often driven by men when differences across gender are present. Specifically, men are less likely to have a partner born in Finland and more likely to give their children Finnish names. However, treated women are more likely to give their children names with a higher Tornedalian CNS. One potential interpretation is that the language component of identity was less salient for girls, as they attended the Swedish-only schools to a lesser extent than boys, but that lower educational uptake also reduced their exposure to Swedish culture through schooling more generally, potentially contributing to a stronger regional identity.

Taken together, these patterns are consistent with families being more willing to take advantage of the new educational opportunities for boys, while being more reluctant to send girls to the Swedish-only schools. An alternative explanation is that the gender difference in educational uptake reflects differential returns to education. While I cannot rule out this explanation, the absence of effects on later human-capital outcomes provides little evidence that differential realized human-capital gains drive the results.

9. CONCLUSION

In this paper, I provide evidence that exposure to schooling with a cultural component, such as the language of instruction, can affect national identity and social integration, and that social integration need not be coupled with cultural assimilation. Specifically, I find that individuals exposed to Swedish-only schooling in Tornedalen at the turn of the twentieth century were less likely to marry Finnish-born individuals, more likely to marry individuals born in the region, and more likely to live in ethnolinguistically diverse villages. At the same time, they gave their children names that were more Finnish and less Swedish and were less likely to work in the military, suggesting a shift in expressions of identity toward Finnish and away from Swedish.

From the perspective of a policymaker seeking to integrate a minority population, the results suggest that increasing interactions between groups in schools may help foster social integration, while imposing a particular language of instruction may generate countervailing identity responses. The evidence is consistent with an important role for increased intergroup contact in common schools in explaining the effects on social integration, while the identity response is more consistent with Swedish-only instruction making language a more salient component of ethnicity and thereby altering identity.

More broadly, even when instruction in the language children know best improves learning, such policies may also affect opportunities for intergroup contact if they lead children from different language

groups to be educated separately. My results suggest that the design of language-of-instruction policies should take into account not only their effects on learning, but also the opportunities they create for interaction across groups.

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APPENDIX

A. DATA

A.1 1930 census

The 1930 census is currently being digitized. Approximately two-thirds of the full census has been digitized and released, with some counties fully complete and others not. The county in which the Swedish-only schools opened, Norrbotten, is fully digitized, implying that I observe all individuals residing there in 1930. However, individuals who migrated out of the county may be missing from the data.

To address potential bias arising from incomplete digitization, I weight observations by the inverse probability of being included in the sample. The inclusion probability is calculated as the share of individuals from each county observed in the data relative to the estimated county population in 1930. This approach removes bias conditional on the digitization rate being orthogonal to both treatment and outcomes. The plausibility of this assumption is supported by Jayes et al. (2025), who argue that the digitization process is effectively random with respect to parish ordering. Since parishes are digitized in full before release, the order in which they are digitized—and hence released—should be orthogonal to treatment status and outcomes.

A.2 Swedish-only schools

In Table A1, I provide information on exactly when Swedish-only schools were announced, the village and parish in which each school opened, whether it was ambulatory or stationary, and the year I assign as the opening year.

TABLE A1
Swedish-only schools

Announcement date	Village	Parish	Ambulatory or stationary	Year opened (assumed)
2 November 1888	Lappträsk	Karl Gustav	Stationary	1889
2 November 1888	Teurajärvi	Korpilombolo	Stationary	1889
2 November 1888	Nikkala / Salmis	Nedertorneå	Stationary	1889
2 November 1888	Turtola / Svanstein	Övertorneå	Stationary	1889
17 March 1894	Skröfven – Storberg	Gellivare	Ambulatory	1895
17 March 1894	Mäntyvaara – Yrtivaara	Gellivare	Ambulatory	1895
17 March 1894	Palohuornas – Leipojärvi	Gellivare	Ambulatory	1895
17 March 1894	Sakavaara – Lükavaara	Gellivare	Ambulatory	1895
17 March 1894	Sammakko	Gellivare	Stationary	1895
17 March 1894	Matarengi	Övertorneå	Stationary	1895
17 March 1894	Kuivakangas	Övertorneå	Stationary	1895
17 March 1894	Jänkisjärvi	Övertorneå	Stationary	1895
17 March 1894	Narken	Korpilombolo	Stationary	1895
17 March 1894	Jerijärvi	Korpilombolo	Stationary	1895
17 March 1894	Kukkola	Karl Gustav	Stationary	1895
17 March 1894	Kengis bruk	Pajala	Stationary	1895
21 September 1894	Kuckasjärvi	Hietaniemi	Stationary	1895
21 September 1894	Niemis	Hietaniemi	Stationary	1895
21 September 1894	Säivis	Nedertorneå	Stationary	1895
21 September 1894	Mattila	Nedertorneå	Stationary	1895
28 September 1895	Muonionalusta	Pajala	Stationary	1895

Note: *Ambulatory* refers to travelling schools that rotated between villages, whereas *stationary* refers to schools that were permanently located in a single village. The opening year is assumed to correspond to the year following the announcement date, with the exception of the schools announced on 28 September 1895, for which the opening year is assumed to be 1895 for simplicity. Observations from teacher registers (Lindell, 1896; Borgh, 1889) indicate that schools did not open directly, suggesting that delays between announcement and opening were common. This justifies using the subsequent year as the assumed opening date while still remaining relatively conservative. The data are compiled from Tenerz (1960) and Tillförordnade Folkskoleinspektörer (1900).

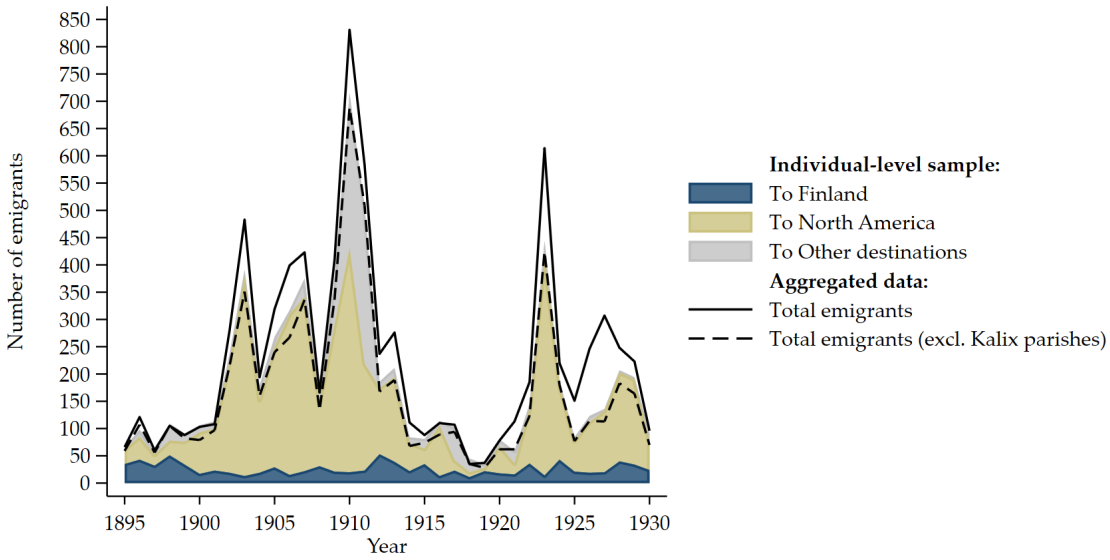
A.3 Emigration data

The individual-level emigrant data are likely incomplete. To assess how incomplete they are, I compare them to parish-level information on emigrants from (Demografiska databasen, CEDAR, Umeå universitet, 2022), which contains information reported by priests between 1895 and 1930 and was collected and summarized by Gunnar Myrdal in the 1930s. These data come from the official registers of the time and are thus the best source obtainable. However, some migrants might still be missed if they were not reported as emigrants by the priests.

Figure A1 summarizes how the register source (aggregated data) and the individual-level sample compare over time. The coloured areas summarize the number of emigrants in the individual-level data

and their destinations (North America, Finland, or other). These statistics are then compared to the aggregate register data, either for all parishes in my definition of Tornedalen (an extended definition that includes all parishes in the census containing any individuals belonging to the Finnish minority) or for a definition of Tornedalen excluding the Kalix parishes, as they contain very few minority members and are therefore often not counted as part of the Tornedalen region.

FIGURE A1
Emigration over time



Notes: The figure compares annual emigration in the individual-level EMUS data with aggregate parish-level register data from Demografiska databasen, CEDAR, Umeå universitet (2022). The coloured areas show emigrants in the individual-level data by destination: North America, Finland, or other destinations. The register totals are shown for all parishes in the extended definition of Tornedalen and for a narrower definition excluding the Kalix parishes.

Neither comparison is perfect; together, however, they suggest that the sample appears to be very complete for the Tornedalen region when compared to the registers. The individual data capture slightly more emigrants than the register totals when the Kalix parishes are excluded, but slightly fewer when they are included. Overall, the individual data appear comprehensive, and the stable and low migration trend to Finland suggests that the number of missing migrants is likely small.

A.4 Cultural Name Score

When constructing the CNS, one needs to define the different cultural groups in the data, which I do as follows. The Swedish group consists of individuals born in Sweden outside the Tornedalen region and not classified as Sámi or Romani. The Tornedalian group consists of individuals born in Tornedalen who are not classified as Sámi or Romani, while the Finnish group includes individuals born in Finland.

In addition, I calculate the prevalence of each name among the Sámi minority and among immigrants from countries other than Finland. All five group-specific name prevalences—Swedish, Tornedalian, Finnish, Sámi, and immigrant—enter the denominator of Equation 1, ensuring that the score captures the relative prevalence of a name in the cultural group of interest compared with the full set of relevant cultural groups. By including each cultural group separately, rather than pooling all individuals into a single comparison category, the measure avoids assigning disproportionate influence to larger groups and instead evaluates names relative to the full set of cultural ethnicities present in the population.

Following the literature, the score in year t is calculated using only names observed during the preceding 20 years, and I drop uncommon names. Specifically, I only consider names shared by at least five individuals within the rolling window. This rolling window allows the measure to reflect contemporary naming practices and accommodates changes in the cultural distinctiveness of names over time.

In Sweden, children are often given multiple names. When calculating the CNS, I use all recorded given names. However, when calculating an individual's CNS, I rely only on the first given name, as the first name is assumed to carry the strongest cultural signal.

To get a sense of what the Cultural Name Score (CNS) captures, Table A2 presents examples of high-scoring names.

TABLE A2
Examples of high-scoring names

Male			Female		
Name	CNS	Count	Name	CNS	Count
Panel 1: Swedish					
ARVID	100	22920	LINNEA	100	62894
EINAR	100	20661	SIGNE	100	33698
RAGNAR	100	19866	ASTRID	100	28379
GOSTA	100	18345	RUT	100	23442
BERTIL	100	16648	EDIT	100	22797
ALGOT	100	12744	GERDA	100	20437
FOLKE	100	12651	SVEA	100	17700
BIRGER	100	12100	VIOLA	100	16756
HILDING	100	11530	MARGIT	100	12095
TURE	100	10496	ALFRIDA	100	11088
Panel 2: Finnish					
MATTI	100	10	SUOMA	100	7
ARMAS	100	8	SAIDA	100	6
JUHA	100	6	LYGLI	100	5
PEKKA	100	5	SAIMI	100	4
ANTTI	100	5	DORDI	100	2
JUHO	100	14	KATRI	100	3
TOIVO	100	11	AINO	100	6
VAINO	100	29	XENIA	100	3
ARVO	100	6	IRJA	100	2
KAARLE	100	4	ADELAID	100	1
Panel 3: Tornedalian					
VALDE	100	15	MEIMI	100	8
ILMAR	100	9	SENEFIA	100	6
HEIMAR	100	3	VESTINA	100	5
FALE	99	7	NETTI	100	5
KALEB	99	16	HELLI	100	11
DONATUS	99	2	GENSINA	100	4
OLGE	99	2	TYYNI	99	3
ABNER	99	7	NETTY	99	3
AMBERT	99	2	AINI	99	4
HYGIENUS	99	2	MILIANA	99	8

Notes: The table reports the ten names with the highest Cultural Name Score (CNS) separately by gender and cultural group. CNS is calculated using 20-year birth-year windows in the 1910 census. Since a name can receive different scores across years, the table reports the maximum CNS for the name. Ties are broken using the maximum number of individuals bearing the name within a single birth year (reported under *Count* in the table).

A.5 Census linking

The objective of the census linking is to obtain pre-treatment characteristics of individuals and/or their families. In most cases, these coincide, as I typically link adults to their childhood location and observe them together with their family, allowing me to construct variables such as whether the head of the household participates in the labor market. However, in some cases I obtain information on individuals'

families before the individuals themselves are observed in the 1890 census, using their childhood home in the next census round. This is done to increase the number of observations, as some individuals are not yet born at the time of the 1890 pre-treatment census, and I want to obtain information on where their family lived just before they were born. Specifically, this applies to individuals aged 3–4 in 1895, who are not yet born in 1890, where I require pre-treatment characteristics. For these individuals, I link their 1930 and/or 1910 observations to their family in 1900, and then link their family to 1890 to obtain village of residence. This is unlikely to affect the identification strategy; however, I show robustness of my main results to excluding this group.

To avoid inducing bias from the linkage process, I link individuals only on characteristics that should be stable over time. I require exact matches on year of birth, parish of birth, and sex, while allowing for some variation in names. The Swedish census—unlike, for example, the U.S. census—reports year of birth reliably, so I require an exact match on this variable. Because parish borders change over time, I harmonize parishes using historical shapefiles available through the R package *histmaps*, which is available on GitHub and written by Johan Junkka.

I also match on first names and surnames, but I do not require exact matches. Instead, I require names to be sufficiently similar based on Jaro–Winkler distances, as is common in the literature (Abramitzky et al., 2021). In a first matching round, I require both the first name and surname(s) to be unique within a Jaro–Winkler distance of 0.15, using all reported first names. However, individuals often have multiple first names, so I conduct a second round for those unmatched in the first round. In this round, I retain only the first given name and require uniqueness within a Jaro–Winkler distance of 0.10.

The main challenge with the Swedish census is that children's surnames are most often not reported. Instead, surnames must be inferred from the parents. At the time, there were two practices for how children inherited surnames. Either a child inherited the surname of the household head (usually the father, if one existed), or the child received a patronymic surname (the head's given name plus "-dotter" for females or "-son" for males). The patronymic practice was less common and was disappearing during this period.

Because these alternative practices coexisted, each child is assigned both possible surnames: one patronymic and one inherited. If the household head has more than one given name, I use the first. However, if the household head has two reported surnames, I assume that the patronymic practice is no longer used in the family and instead assign those two surnames as alternatives. If more than two surnames are reported, I use the first and the last.

In general, if anyone in the household has a missing surname, it is assumed that they share the surname(s) of the household head, and I assign up to two alternatives if the head has more than one surname (the first and the last). For individuals with reported surnames, I separate multiple surnames into up to two alternatives rather than matching only on the combined form. However, I also allow matches on the combined surname or on missing surnames when relevant.

As a result, each individual can have up to three surnames used for matching: the reported surname (whether single, multiple, or missing) and up to two imputed alternative surnames. I allow matches on missing surnames, as an individual might remain a child or lack a reported surname for other reasons between census rounds. I only allow a match if it is unique across all surname alternatives.

I have created links between all available census waves. When I want to link an individual back to a specific census wave for an analysis, I allow every possible connection between the relevant census waves but only keep unique matches. For example, when I want to analyse outcomes in 1910 and need information from 1890, I allow a direct link between the two censuses, but I also allow the individual to link from 1910 to 1900 and then to 1890, and only keep a link if it is unique.

My outcomes are measured using information from the 1910 and 1930 censuses combined, or from either census alone. I therefore report linkage rates to both censuses in Table A3. The linkage rates are calculated conditional on being observed in the relevant outcome census, 1910 or 1930. For each census, I report linkage rates for the full relevant sample, consisting of individuals born between 1867 and 1892; for individuals born between 1867 and 1890, who can be linked directly to the 1890 census; and for individuals born in 1891–1892, whose pre-treatment information is obtained by linking them through their parents. When possible, I combine the two censuses to increase the amount of available information and enlarge the sample. Doing so allows me to include individuals linked to either the 1910 or the 1930 census, as well as those linked to both. The table shows high linkage rates by international standards for both men and women.

TABLE A3
Backward linkage rates to 1890

	Male	Female	All
1910 full sample	81.3	81.8	81.5
1910 sample directly linked	82.5	82.6	82.6
1910 sample linked through parents	71.6	75.1	73.3
1930 full sample	82.4	82.1	82.2
1930 sample directly linked	83.4	83.5	83.4
1930 sample linked through parents	73.3	70.0	71.7

Notes: The table reports backward linkage rates to the 1890 census, conditional on being observed in the 1910 or 1930 census, respectively, for individuals born in Tornedalen. The full sample includes individuals born between 1867 and 1892. The directly linked sample includes individuals born between 1867 and 1890, who can be linked to the 1890 census as individuals. The sample linked through parents consists of individuals born in 1891–1892, for whom pre-treatment information is obtained by linking them to their family in 1900 and subsequently linking the family to the 1890 census. Linkage rates are reported separately for men, women, and the full sample.

I also report measures of representativeness in Table A4, which compares pre-treatment characteristics in the directly linked samples with those of the full relevant population observed in 1890. This comparison excludes individuals linked through their parents, as these individuals were not yet born in 1890. The linked samples are similar to the full population across these characteristics. The remaining differences are small, although linked individuals are somewhat more likely to come from larger farm households and from households in which the father is present.

TABLE A4
Representativeness of the linked samples

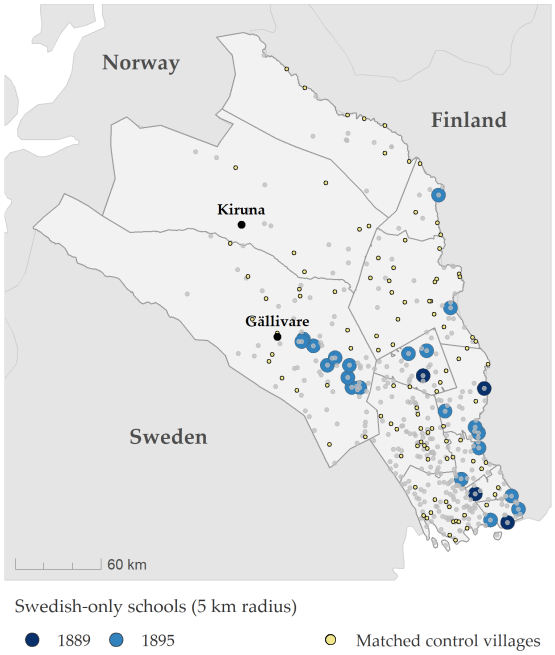
	Full 1890	Linked to 1910	Linked to 1930
Female	0.49	0.48	0.48
Finnish at home	0.49	0.48	0.47
Servants in household	0.13	0.14	0.13
Household head in labor force	0.80	0.81	0.81
Farm household	0.57	0.58	0.60
Household size	6.58	6.76	6.78
Father in household	0.91	0.93	0.93
N	22,195	13,667	10,783

Notes: The table compares characteristics measured in the 1890 census for the full relevant population with the samples successfully linked to the 1910 and 1930 censuses. The sample is restricted to individuals born in Tornedalen between 1867 and 1890. The first column includes the full 1890 population satisfying these restrictions and is not conditional on successful linkage. The second and third columns report means for individuals successfully linked to the 1910 and 1930 censuses, respectively.

A.6 Matched control villages

FIGURE A2
Matched control villages

(a) Swedish-only schools and matched villages



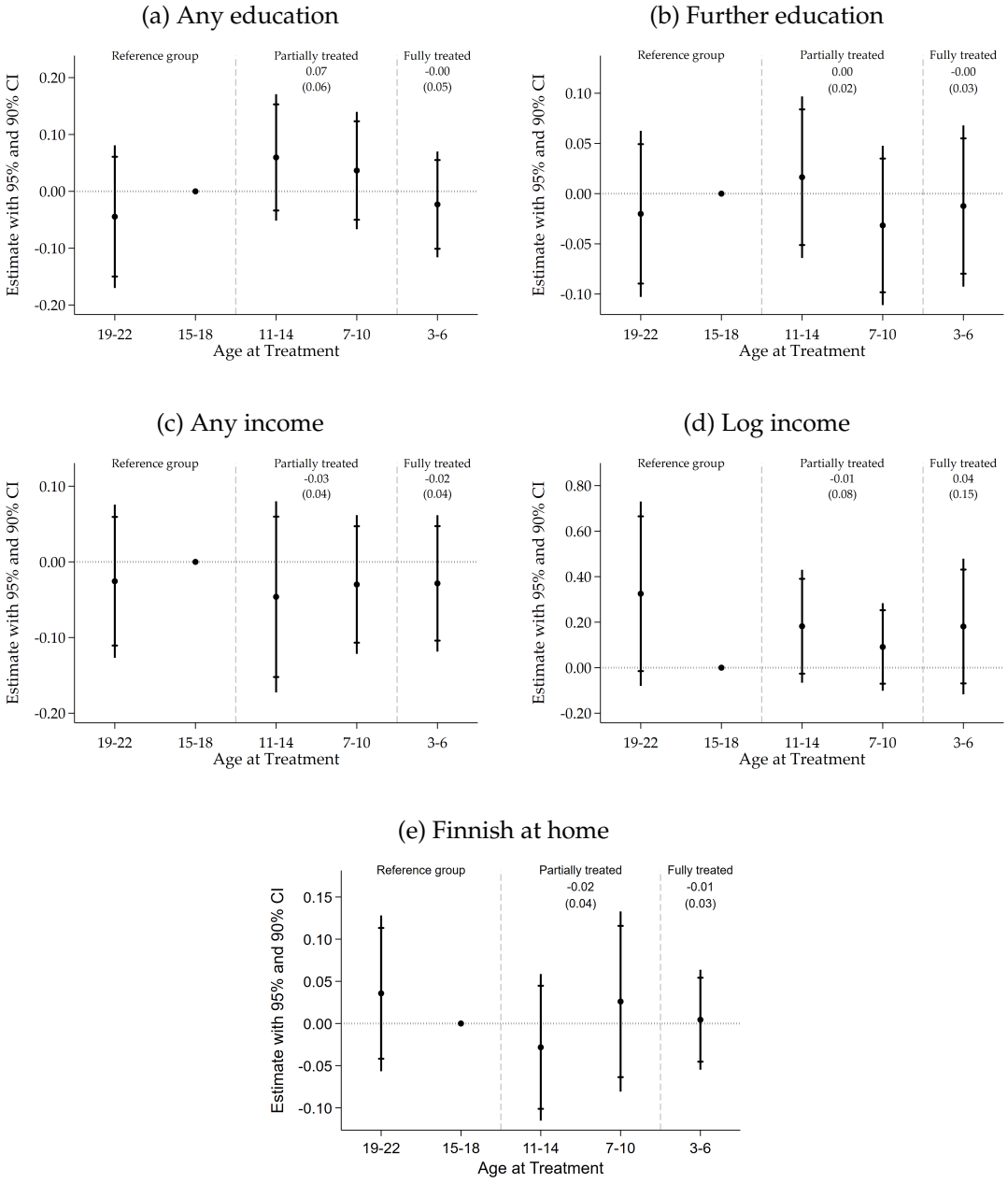
(b) Matched villages and matching weights



Notes: The left panel (a) shows the locations of Swedish-only schools and their 5-km radii together with the matched control villages. The right panel (b) shows only the matched control villages, with marker size proportional to the matching weight assigned to each village.

B. ADDITIONAL RESULTS

FIGURE B1
Event-study estimates: human capital



Notes: The education and income outcomes are measured in 1930 and the language outcome in 1910. Further education refers to above primary education. The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. As the 1930 census is incomplete, weights accounting for this are also included for relevant outcomes. The figure reports estimates from an event-study specification that interacts an indicator equal to one if an individual is born in a village experiencing a Swedish-only school opening with age-at-exposure indicators. The omitted category is ages 15–18, corresponding to individuals just above school age at the time of treatment. The specification includes village and cohort stack-specific fixed effects. Estimates are from a stacked event-study specification using the weights proposed by Wing et al. (2024). Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

TABLE B1
Swedish-only schools and human capital: Men

	Any education (1)	Further education (2)	Any income (3)	Log income (4)	Finnish at home (5)
Panel A.					
Treated	0.056	-0.007	-0.008	-0.106	0.001
	(0.055)	(0.030)	(0.047)	(0.106)	(0.040)
Panel B.					
Years treated	0.003	-0.000	0.001	-0.008	-0.001
	(0.004)	(0.003)	(0.005)	(0.015)	(0.003)
Panel C.					
Treated	0.188***	0.006	0.038	-0.049	0.008
	(0.067)	(0.027)	(0.060)	(0.155)	(0.054)
Treated × Teacher before	-0.220***	-0.022	-0.078	-0.091	-0.010
	(0.064)	(0.041)	(0.075)	(0.166)	(0.055)
Village-Stack FE	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓
Dep. var. mean	0.866	0.082	0.749	7.247	0.447
N	2,152	2,152	2,152	1,665	2,706

Notes: The education and income variables are measured in 1930 and the language variable in 1910. Further education refers to above primary education. The sample consists of men aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 and the 1910 or 1930 censuses, and restricted to the matched village sample using weights reflecting matching with replacement. As the 1930 census is incomplete, weights accounting for this are also included for relevant outcomes. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). *Teacher before* is an indicator equal to one if there was a teacher within 5 km of the village of residence in 1890. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B2

Swedish-only schools and human capital: Women

	Any education (1)	Further education (2)	Any income (3)	Log income (4)	Finnish at home (5)
Panel A.					
Treated	0.031	-0.011	0.002	-0.099	-0.017
	(0.050)	(0.040)	(0.029)	(0.295)	(0.049)
Panel B.					
Years treated	-0.005	-0.003	0.003	0.015	0.002
	(0.004)	(0.004)	(0.003)	(0.032)	(0.005)
Panel C.					
Treated	0.092	-0.018	0.012	-1.258**	-0.009
	(0.074)	(0.029)	(0.035)	(0.540)	(0.063)
Treated × Teacher before	-0.097	0.011	-0.016	1.277**	-0.013
	(0.069)	(0.052)	(0.053)	(0.526)	(0.069)
Village-Stack FE	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓
Dep. var. mean	0.873	0.080	0.108	7.242	0.439
N	1,993	1,993	1,993	221	2,413

Notes: The education and income variables are measured in 1930 and the language variable in 1910. Further education refers to above primary education. The sample consists of women aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 and the 1910 or 1930 censuses, and restricted to the matched village sample using weights reflecting matching with replacement. As the 1930 census is incomplete, weights accounting for this are also included for relevant outcomes. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). *Teacher before* is an indicator equal to one if there was a teacher within 5 km of the village of residence in 1890. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B3

Swedish-only schooling and occupational outcomes

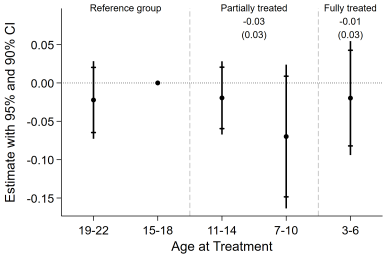
	Any job	Farmer or fisher	Skill level	Higher skilled	Medium skilled	Lower skilled	Unskilled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Pooled							
Treated	0.012	0.029	0.094	0.017	0.015	-0.044	-0.017
	(0.026)	(0.044)	(0.127)	(0.040)	(0.038)	(0.035)	(0.034)
Dep. var. mean	0.579	0.258	2.235	0.119	0.183	0.192	0.248
N	5,723	3,328	2,396	3,328	3,328	3,328	3,328
Panel B. Men							
Treated	0.003	0.035	0.007	0.013	-0.037	-0.006	-0.005
	(0.035)	(0.049)	(0.146)	(0.039)	(0.038)	(0.026)	(0.038)
Dep. var. mean	0.828	0.316	2.290	0.140	0.145	0.172	0.227
N	3,012	2,597	1,739	2,597	2,597	2,597	2,597
Panel C. Women							
Treated	0.034	0.001	0.305	0.081	0.129	-0.245**	0.034
	(0.038)	(0.058)	(0.253)	(0.084)	(0.084)	(0.094)	(0.081)
Dep. var. mean	0.307	0.083	2.117	0.060	0.295	0.252	0.309
N	2,697	710	636	710	710	710	710

Notes: The occupational outcomes are measured in the 1910 and 1930 census combined. The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 and 1910 censuses, and restricted to the matched village sample using weights reflecting matching with replacement. Panel A reports results for the pooled sample, Panel B for men, and Panel C for women. *Any occupation* is an indicator equal to one if the individual has an occupation classified according to HISCLASS meant to test if there is selection into occupational reporting. The remaining occupational outcomes are defined among individuals with a classified occupation. *Farmers and fishermen* corresponds to HISCLASS category 8. *Higher skilled* includes higher managers and higher professionals (HISCLASS 1–2). *Medium skilled* includes lower managers, lower professionals, foremen, and medium-skilled workers (HISCLASS 3, 4, 6, and 7). *Lower skilled* includes lower clerical and sales personnel, lower-skilled farm workers, and lower-skilled workers (HISCLASS 5, 9, and 10). *Unskilled* includes unskilled workers and unskilled farm workers (HISCLASS 11–12). *Skill level* is an ordinal measure ranging from 1 for unskilled occupations to 4 for higher-skilled occupations and excludes farmers and fishermen. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). The specification includes village- and age-stack fixed effects. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

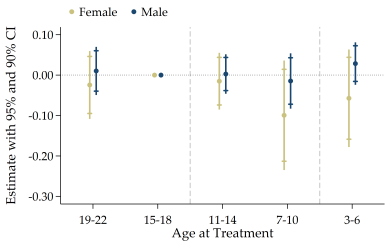
FIGURE B2

Event-study estimates: intergroup relations

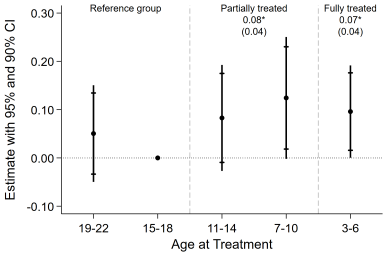
(c) Partner born Swe. excl. Torn



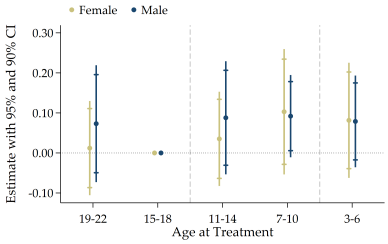
(d) Partner born Swe. excl. Torn by sex



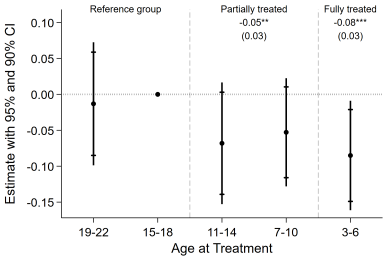
(e) Partner born Tornedalen



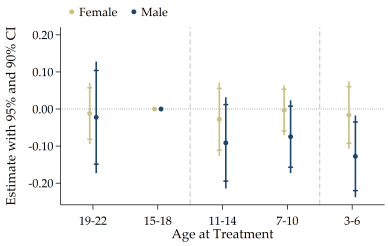
(f) Partner born Tornedalen by sex



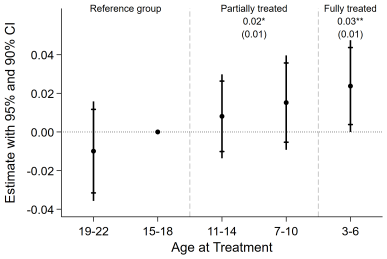
(a) Partner born Finland



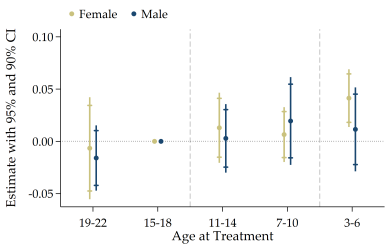
(b) Partner born Finland by sex



(g) Village fractionalization



(h) Village fractionalization by sex



Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from an event-study specification that interacts an indicator equal to one if an individual is born in a village experiencing a Swedish-only school opening with age-at-exposure indicators grouped in four-year bins. The omitted category is ages 15–18, corresponding to individuals just above school age at the time of treatment. The specification includes village and cohort stack specific fixed effects. Estimates are from a stacked event-study specification using the weights proposed by Wing et al. (2024). Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

TABLE B4

Swedish-only school exposure and having a partner

	Female & Male		Female		Male	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.029		-0.015		-0.038	
	(0.035)		(0.056)		(0.051)	
Years treated		-0.005		-0.002		-0.007
		(0.005)		(0.007)		(0.006)
Village-Stack FE	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓
Dep. var. mean	0.638	0.638	0.648	0.648	0.628	0.628
N	6,456	6,456	3,025	3,025	3,415	3,415

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B5

Swedish-only school exposure and village level share Finnish at home

	Share Finnish in village	
	(1)	(2)
Treated	0.016	
	(0.031)	
Years treated		0.002
		(0.003)
Village-Stack FE	✓	✓
Age-Stack FE	✓	✓
Dep. var. mean	0.456	0.456
N	5,700	5,700

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B6
Swedish-only school exposure and surname-based partner ethnicity

	Predicted Finnish attachment		Predicted ethnic mixedness	
	(1)	(2)	(3)	(4)
Treated	-0.035		0.035*	
	(0.040)		(0.021)	
Years treated		-0.004		0.004**
		(0.004)		(0.002)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	0.620	0.620	0.185	0.185
N	3,619	3,619	3,619	3,619

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

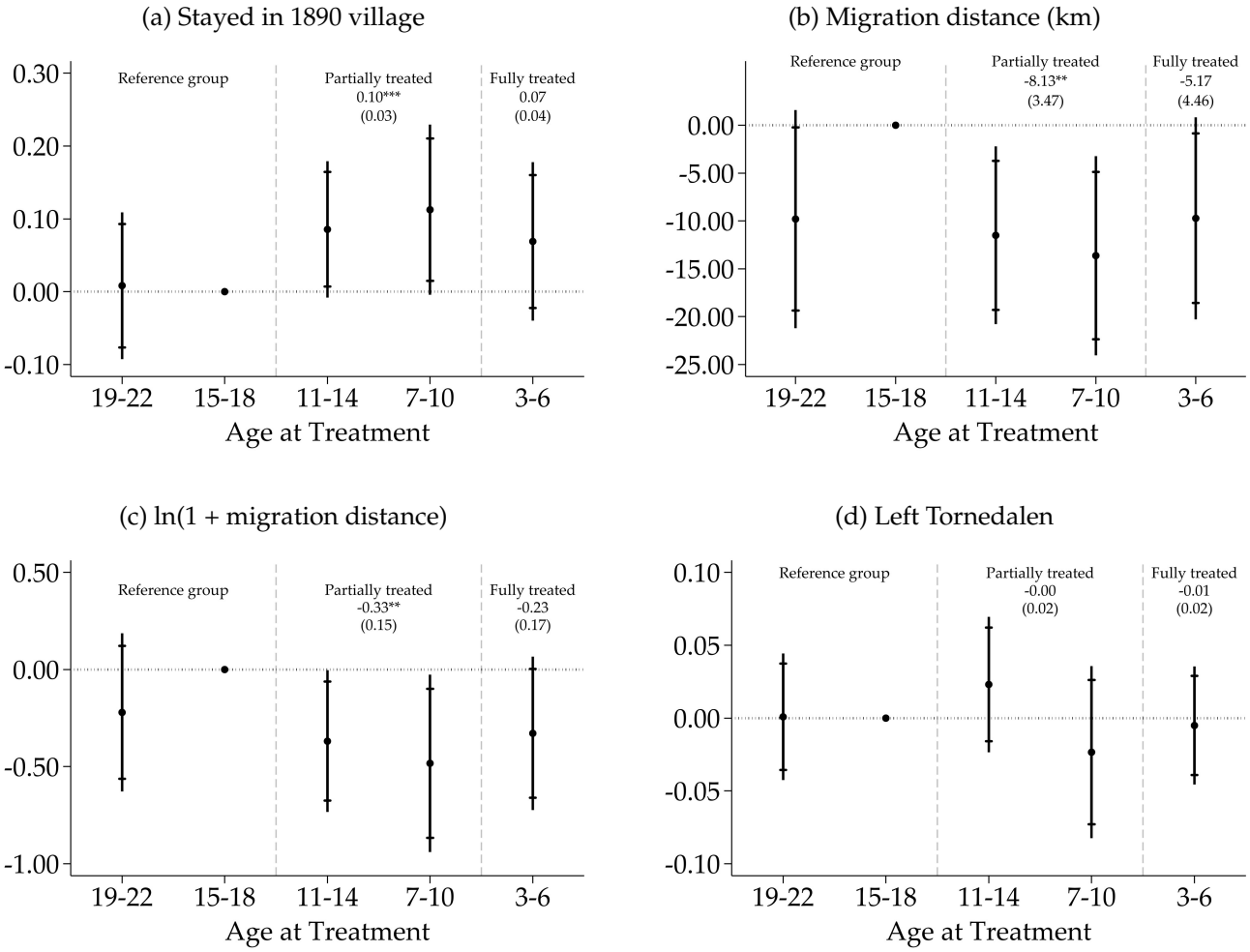
TABLE B7
Swedish-only schools and migration

	Stayed in village	Mig. dist. (km)	ln(1 + mig. dist.)	Left Tornedalen
	(1)	(2)	(3)	(4)
Panel A.				
Treated	0.084**	-6.945**	-0.288**	-0.004
	(0.037)	(3.280)	(0.145)	(0.021)
Panel B.				
Years treated	0.006	-0.480	-0.021	-0.002
	(0.004)	(0.409)	(0.016)	(0.002)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	0.468	29.413	1.849	0.113
N	6,168	6,168	6,168	6,456

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

FIGURE B3

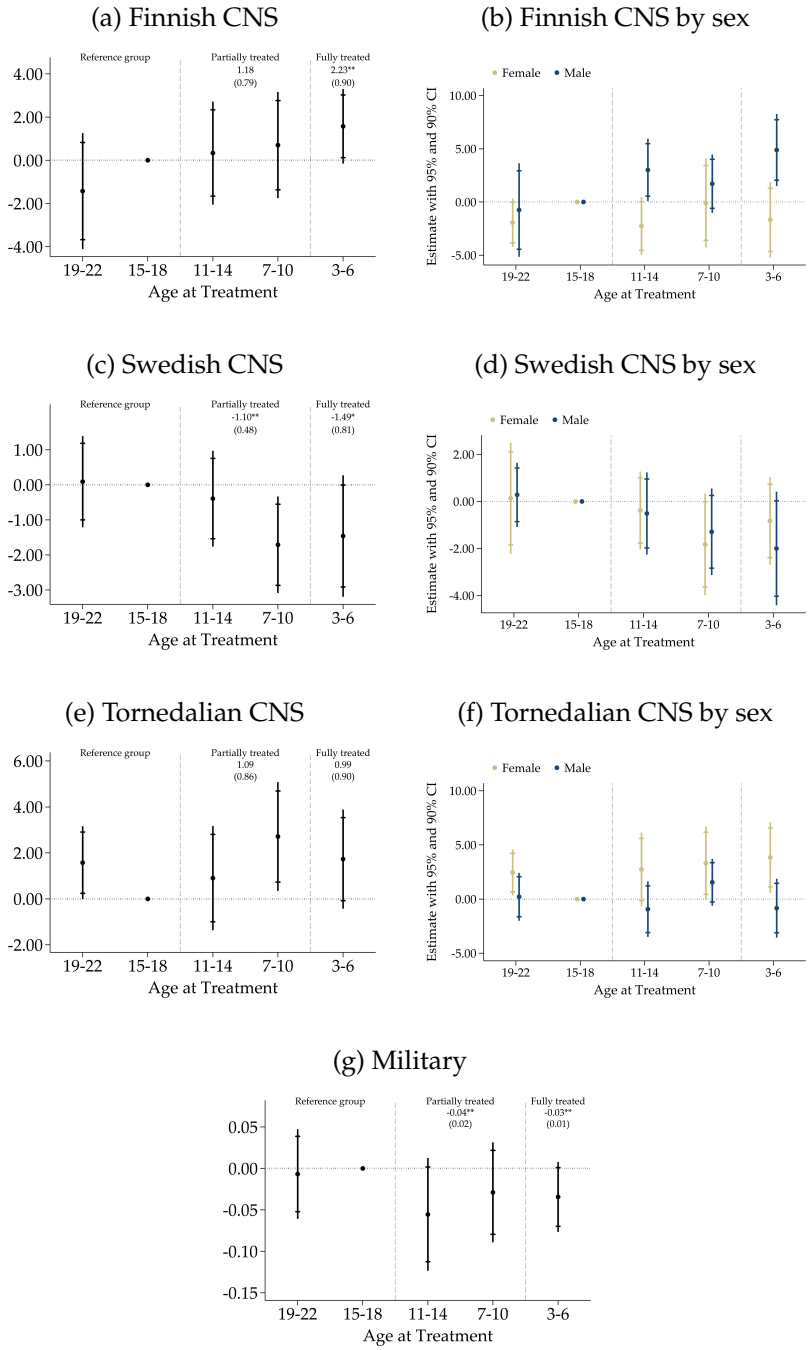
Event-study estimates: migration outcomes



Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from an event-study specification that interacts an indicator equal to one if an individual is born in a village experiencing a Swedish-only school opening with age-at-exposure indicators grouped in four-year bins. The omitted category is ages 15–18, corresponding to individuals just above school age at the time of treatment. The specification includes village and cohort stack specific fixed effects. Estimates are from a stacked event-study specification using the weights proposed by Wing et al. (2024). Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

FIGURE B4

Event-study estimates: social identity outcomes



Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from an event-study specification that interacts an indicator equal to one if an individual is born in a village experiencing a Swedish-only school opening with age-at-exposure indicators grouped in four-year bins. The omitted category is ages 15–18, corresponding to individuals just above school age at the time of treatment. The specification includes village and cohort stack specific fixed effects. Estimates are from a stacked event-study specification using the weights proposed by Wing et al. (2024). Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

TABLE B8
Swedish-only school exposure and having a child

	Female & Male		Female		Male	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.049		-0.056		-0.045	
	(0.039)		(0.053)		(0.054)	
Years treated		-0.005		-0.004		-0.007
		(0.005)		(0.007)		(0.006)
Village-Stack FE	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓
Dep. var. mean	0.623	0.623	0.664	0.664	0.586	0.586
N	6,456	6,456	3,025	3,025	3,415	3,415

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

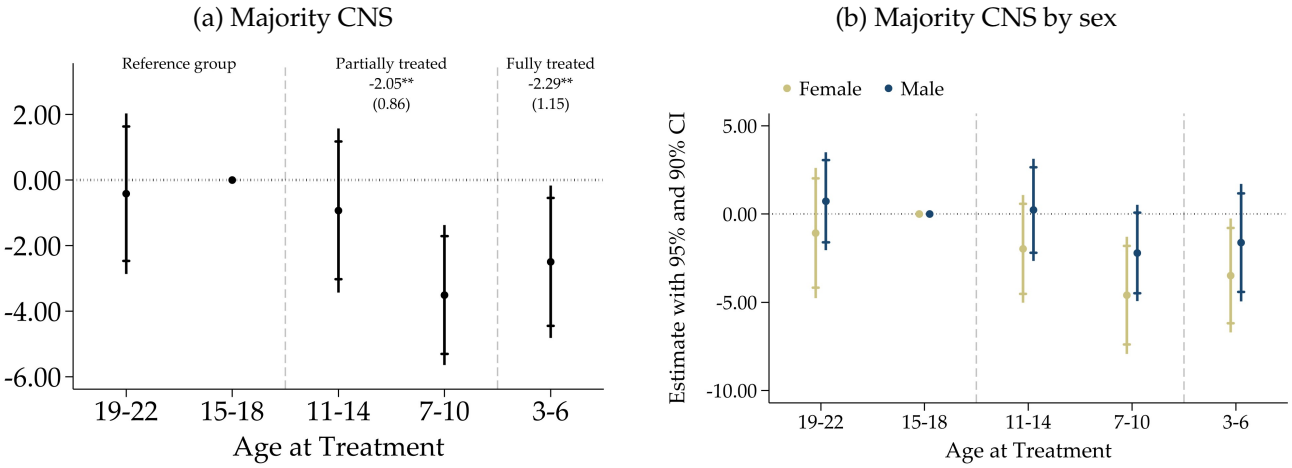
TABLE B9
Swedish-only school and majority CNS

	Majority CNS	
	(1)	(2)
Treated	-2.128**	
	(0.879)	
Years treated		-0.264**
		(0.113)
Village-Stack FE	✓	✓
Age-Stack FE	✓	✓
Dep. var. mean	46.298	46.298
N	3,925	3,925

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

FIGURE B5

Event-study estimates: majority CNS



Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. The figure reports estimates from an event-study specification that interacts an indicator equal to one if an individual is born in a village experiencing a Swedish-only school opening with age-at-exposure indicators grouped in four-year bins. The omitted category is ages 15–18, corresponding to individuals just above school age at the time of treatment. The specification includes village and cohort stack specific fixed effects. Estimates are from a stacked event-study specification using the weights proposed by Wing et al. (2024). Estimates are shown with 95% and 90% confidence intervals based on standard errors clustered at the village level.

TABLE B10
Swedish-only schools and emigration

	Any country		Finland	
	(1)	(2)	(3)	(4)
Treated	-0.019		-0.001	
	(0.041)		(0.014)	
Years treated		-0.001		0.002
		(0.004)		(0.002)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	0.126	0.126	0.020	0.020
N	5,194	5,194	5,194	5,194

Notes: The sample consists of individuals aged 3 to 22 at the opening of a Swedish-only school, linked between the 1890 census and the migrant sample and/or the 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. *Years treated* is a continuous variable ranging from 1 to 12, capturing the number of years an individual is exposed to the Swedish-only school system (equal to 1 if the school opens when the individual is 14 years old and 12 if it opens when the individual is 3 years old). The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

TABLE B11
Assessing sensitivity to concurrent events: Gällivare (mining town)

	Finnish CNS		Swedish CNS		Tornedalian CNS		Military	
Panel A: National Identity								
Treated		1.497**		-1.394***		1.292		-0.033**
		(0.743)		(0.490)		(0.818)		(0.014)
Age $\leq 14 \times$ Distance to Gällivare	0.163	0.067	0.165*	0.254***	-0.296	-0.379*	-0.005**	-0.003
	(0.200)	(0.232)	(0.098)	(0.074)	(0.196)	(0.195)	(0.002)	(0.002)
Dep. var. mean	20.520	20.520	20.458	20.458	26.704	26.704	0.029	0.029
N	3,925	3,925	3,925	3,925	3,925	3,925	3,071	3,071
	Partner born Finland		Partner born Tornedalen		Partner born Swe. excl. Torn.		Village fractionalization	
Panel B: Social integration								
Treated		-0.061**		0.069*		-0.018		0.021**
		(0.027)		(0.037)		(0.025)		(0.010)
Age $\leq 14 \times$ Distance to Gällivare	-0.006	-0.002	0.018***	0.014**	-0.014***	-0.013***	0.002	0.001
	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)
Dep. var. mean	0.090	0.090	0.846	0.846	0.077	0.077	0.128	0.128
N	4,069	4,069	4,069	4,069	4,069	4,069	5,700	5,700
Village-Stack FE	✓	✓	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B12

Assessing sensitivity to concurrent events: Border

	Finnish CNS		Swedish CNS		Tornedalian CNS		Military	
Panel A: National identity								
Treated		2.128***		-1.552***		1.469		-0.033**
		(0.715)		(0.480)		(0.929)		(0.013)
Age $\leq 14 \times$ Distance to border	0.592*	0.803***	-0.276	-0.430***	0.415	0.561	0.005	0.003
	(0.353)	(0.307)	(0.212)	(0.147)	(0.390)	(0.403)	(0.006)	(0.004)
Dep. var. mean	20.520	20.520	20.458	20.458	26.704	26.704	0.029	0.029
N	3,925	3,925	3,925	3,925	3,925	3,925	3,071	3,071
	Partner born Finland		Partner born Tornedalen		Partner born Swe. excl. Torn.		Village fractionalization	
Panel B: Social integration								
Treated		-0.063*		0.070*		-0.016		0.014*
		(0.032)		(0.039)		(0.023)		(0.008)
Age $\leq 14 \times$ Distance to border	0.006	0.000	-0.017	-0.011	0.016*	0.014*	-0.010***	-0.009***
	(0.013)	(0.015)	(0.016)	(0.016)	(0.009)	(0.007)	(0.003)	(0.003)
Dep. var. mean	0.090	0.090	0.846	0.846	0.077	0.077	0.128	0.128
N	4,069	4,069	4,069	4,069	4,069	4,069	5,700	5,700
Village-Stack FE	✓	✓	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B13

Main estimate with individuals aged 5 to 22 at the opening of a Swedish-only school

	Children's CNS		Works in the	Partner born in		Village level
	(1) Finnish	(2) Swedish	(3) military	(4) Tornedalen	(5) Finland	(6) fractionalization
Treated	1.284*	-1.052**	-0.039**	0.074**	-0.062**	0.017*
	(0.771)	(0.501)	(0.016)	(0.036)	(0.025)	(0.009)
Village-Stack FE	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓
Dep. var. mean	20.520	20.196	0.029	0.846	0.092	0.125
N	3,605	3,605	2,670	3,703	3,703	4,987

Notes: The sample consists of individuals aged 5 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 5 and 14. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B14
Main estimates with Conley SE

	Children's CNS		Works in the	Partner born in		Village level
	Finnish	Swedish	military	Tornedalen	Finland	fractionalization
Panel A: Conley bandwidth = 5 km						
Treated	1.539**	-1.237**	-0.035**	0.077**	-0.063***	0.021**
	(0.710)	(0.500)	(0.014)	(0.037)	(0.024)	(0.010)
Panel B: Conley bandwidth = 3 km						
Treated	1.539**	-1.237**	-0.035**	0.077**	-0.063**	0.021**
	(0.702)	(0.505)	(0.014)	(0.038)	(0.025)	(0.009)
Panel C: Conley bandwidth = 10 km						
Treated	1.539**	-1.237**	-0.035***	0.077**	-0.063***	0.021**
	(0.719)	(0.500)	(0.013)	(0.036)	(0.024)	(0.010)
Dep. var. mean	20.516	20.459	0.029	0.846	0.090	0.128
Village-Stack FE	✓	✓	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓	✓	✓

Notes: The sample consists of individuals aged 3 to 22 at the time of a Swedish-only school opening, linked between the 1890 census and the 1910 and/or 1930 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Treated* is an indicator equal to one if an individual is exposed to a Swedish-only school between ages 3 and 14. The table reports estimates from a stacked difference-in-differences specification using the weights proposed by Wing et al. (2024). Conley SE. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B15
CNSs around school opening

	Female & Male			Female	Male
	(1)	(2)	(3)	(4)	(5)
Panel A: Finnish CNS					
Years since opening	-0.216**	-0.298**	-0.459***	-0.196	-0.278*
	(0.109)	(0.126)	(0.156)	(0.186)	(0.141)
Dep. var. mean	20.398	20.321	20.321	20.859	19.946
N	11,378	9,438	9,438	5,554	5,803
Panel B: Swedish CNS					
Years since opening	0.270***	0.304***	0.338***	0.178	0.355***
	(0.082)	(0.079)	(0.128)	(0.152)	(0.115)
Dep. var. mean	21.511	21.464	21.464	22.025	21.018
N	11,378	9,438	9,438	5,554	5,803
Panel C: Tornedalian CNS					
Years since opening	-0.095	-0.109	-0.332*	0.052	-0.270
	(0.134)	(0.132)	(0.172)	(0.220)	(0.213)
Dep. var. mean	26.021	26.183	26.183	25.076	26.935
N	11,378	9,438	9,438	5,554	5,803
Village-Stack FE	✓	✓	✓	✓	✓
Birthyear-Stack FE	✓	✓	✓	✓	✓
Sibling sample		✓	✓		
Sibling FE			✓		

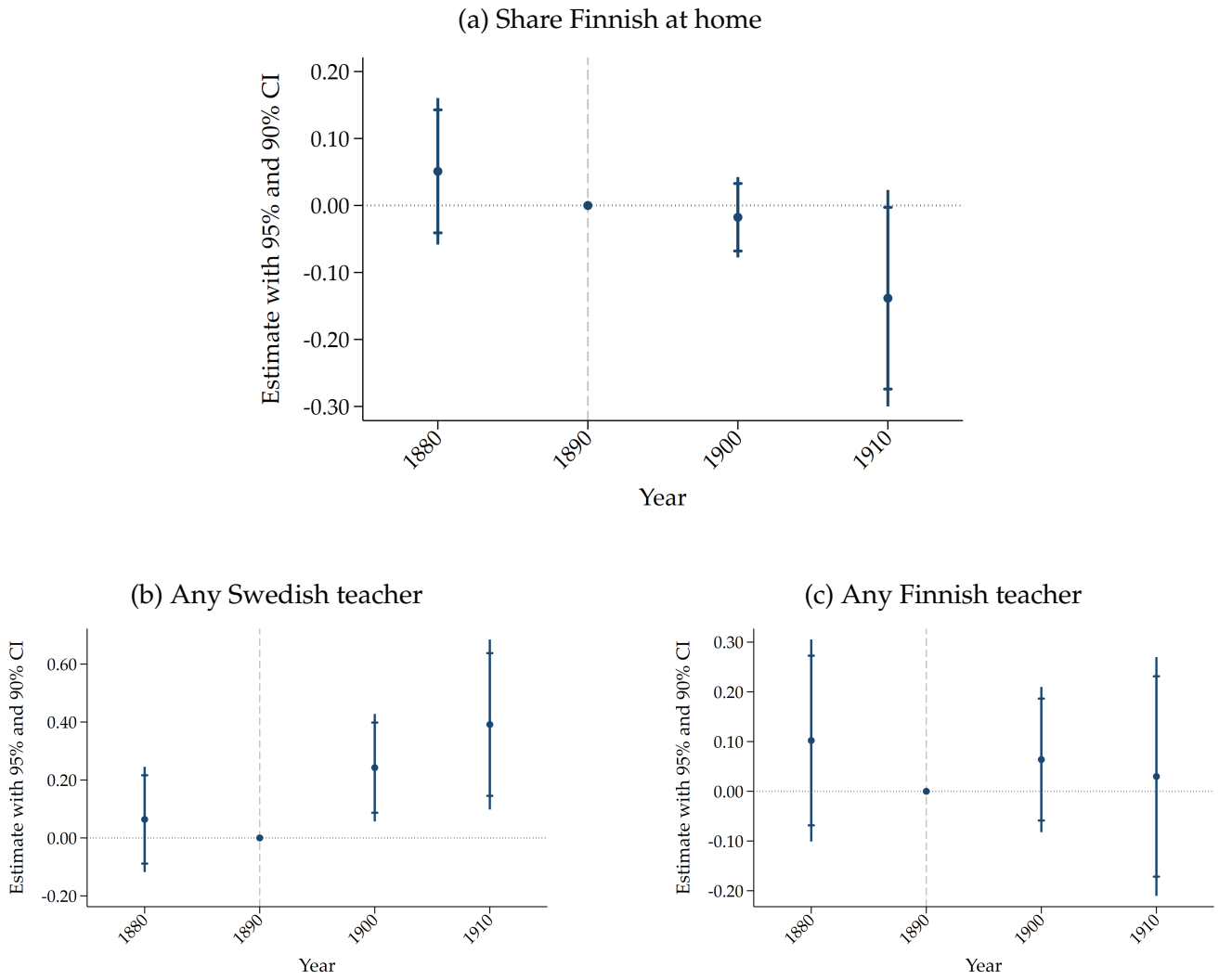
Notes: The sample consists of individuals born between eight years before and five years after the opening of a Swedish-only school, observed in the 1900 census, and restricted to the matched village sample using weights reflecting matching with replacement. *Years since opening* is a continuous variable ranging from 0 to 6, indicating the number of years since a Swedish-only school opened in the village. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B16
Local linguistic environment: Conley standard errors

	Any Swedish teacher	Any Finnish teacher	Share Finnish at home
	(1)	(2)	(3)
Treated × Post	0.285***	-0.004	-0.102*
	(0.087)	(0.085)	(0.061)
Dep. var. mean	0.344	0.281	0.675
Observations	604	604	578
Villages	151	151	151
Village FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: The table reports difference-in-differences estimates of the effect of Swedish-only schooling on the local linguistic environment. Columns (1) and (2) indicate whether there is a teacher of the indicated type within 5 km of the village. Column (3) reports the share of village residents recorded as Finnish. All specifications include village and year fixed effects. Conley standard errors allow for spatial correlation within 10 km and serial correlation within villages over the full sample period. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

FIGURE B6
Swedish-only schools and the local linguistic environment



Notes: The figure reports event-study estimates of the effect of Swedish-only schooling on the local linguistic environment. Panel (a) reports the share of residents in the village recorded as Finnish. Panels (b) and (c) measure whether there is a teacher of the indicated type within 5 km of the village. The omitted year is 1890. All specifications include village and year fixed effects. Standard errors are clustered at the village level. Vertical bars report 95 and 90 percent confidence intervals.

TABLE B17
Village-level social integration and social identity

Panel A. Social integration

	Village fractionalization	Fin. – Swe. (incl. Torn.) couple	Swe. (excl. Torn.) – Fin./Torn. couple	Couple born in Torn.
	(1)	(2)	(3)	(4)
Treated × Post	-0.010	0.021	0.016	-0.031
	(0.024)	(0.026)	(0.013)	(0.040)
Dep. var. mean	0.096	0.103	0.032	0.831
Observations	577	564	564	564
Villages	150	149	149	149

Panel B. National dentity

	Military	Tornedalian CNS	Finnish CNS	Swedish CNS
	(1)	(2)	(3)	(4)
Treated × Post	0.007***	0.741	-0.548	0.333
	(0.003)	(0.922)	(0.616)	(0.571)
Dep. var. mean	0.004	30.581	18.922	18.179
Observations	576	560	560	560
Villages	150	149	149	149
Village FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

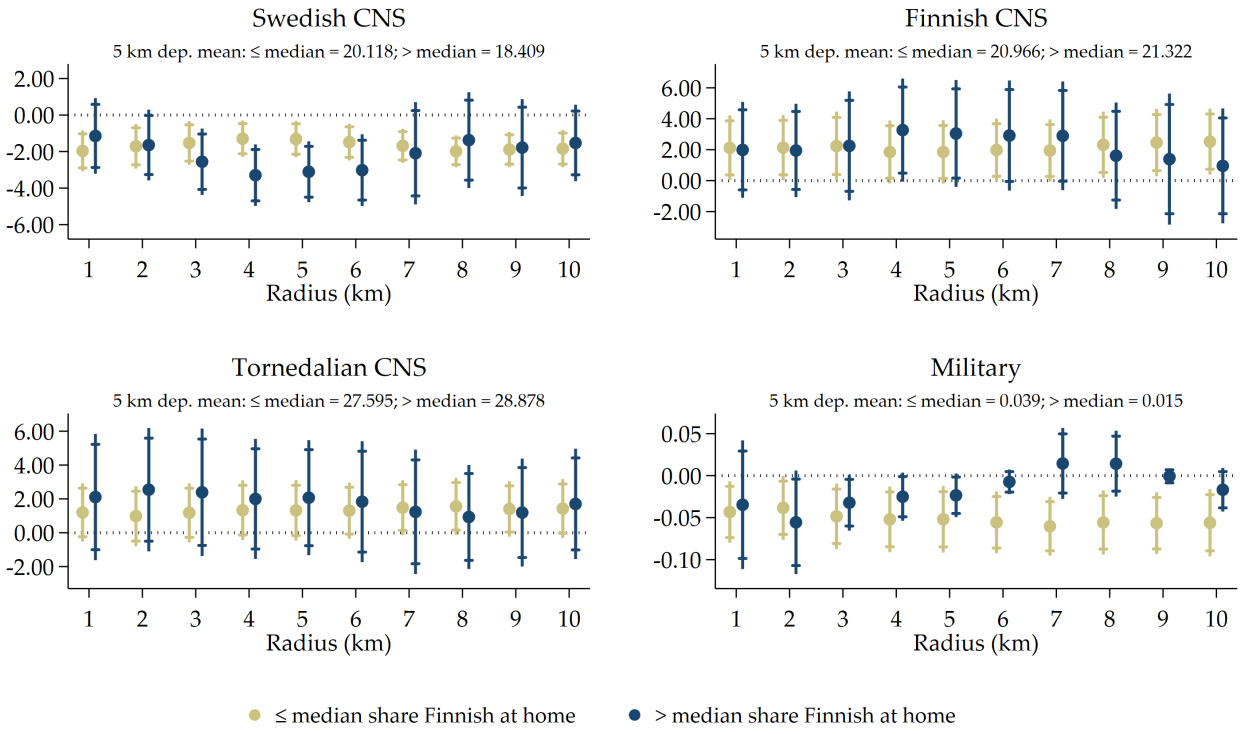
Notes: The table reports difference-in-differences estimates comparing treated and control villages before and after the introduction of Swedish-only schools. Outcomes are measured within the village. Village fractionalization is defined as $1 - [s_F^2 + (1 - s_F)^2]$, where s_F is the share of the village population recorded as Finnish. The marriage outcomes are shares among couples for whom both partners' birthplaces are observed: Finland–Sweden-born couples, where Sweden includes Tornedalen; couples in which one partner was born in Sweden outside Tornedalen and the other in Finland or Tornedalen; and couples in which both partners were born in Tornedalen. Working in the military is the share of men employed in the military. The Cultural Name Score (CNS) outcomes are village averages among children aged 10 or younger. All specifications include village and year fixed effects. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE B18
Village-level birthplace shares

	Share born in		
	Tornedalen	Finland	Swe. excl. Torn.
	(1)	(2)	(3)
Treated × Post	-0.029	0.006	0.019
	(0.022)	(0.007)	(0.022)
Dep. var. mean	0.938	0.029	0.031
Observations	577	577	577
Villages	150	150	150
Village FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: The table reports difference-in-differences estimates comparing treated and control villages before and after the introduction of Swedish-only schools. The outcomes are the shares of the village population born in Tornedalen, Finland, and Sweden outside of Tornedalen, respectively. All specifications include village and year fixed effects. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

FIGURE B7
Heterogeneity by local share using Finnish at home, among those using Finnish



Notes: The figure reports treatment effects separately for individuals living in communities with above- and below-median pre-treatment shares recorded as "Finnish", conditional on the individuals themselves being recorded as "Finnish" before treatment. Estimates are shown with 90% and 95% confidence intervals based on standard errors clustered at the village level.

TABLE B19

Heterogeneity in expressions of social identity by distance to a Swedish teacher

	Military	Swedish CNS	Finnish CNS	Tornedalian CNS
	(1)	(2)	(3)	(4)
Treated	-0.080***	0.514	0.940	1.824
	(0.024)	(1.997)	(1.616)	(2.283)
Treated × Dist. to Swedish teacher	0.014	-0.428	0.775**	0.065
	(0.011)	(0.329)	(0.309)	(0.555)
Treated × Dist. to Finnish teacher	0.014	0.045	-0.084	-0.362
	(0.010)	(0.354)	(0.372)	(0.409)
Treated × Share Finnish at home	0.002	-1.286	-1.001	-0.575
	(0.026)	(2.187)	(1.778)	(2.431)
Village-Stack FE	✓	✓	✓	✓
Age-Stack FE	✓	✓	✓	✓
Dep. var. mean	0.029	20.458	20.520	26.704
N	3,071	3,925	3,925	3,925

Notes: Distance to a Swedish teacher is the log of one plus the distance to the nearest teacher recorded as "Swedish" before treatment, while distance to a Finnish teacher is the log of one plus the distance to the nearest teacher recorded as "Finnish". Share Finnish at home is the the share recorded as "Finnish" within 10 km of the individual's village of residence before treatment. All specifications include village-stack and age-stack fixed effects. Standard errors are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.