

# Backlash: The Unintended Effects of Language Prohibition in U.S. Schools after World War I

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Do forced assimilation policies always succeed in integrating immigrant groups? This article examines how a specific assimilation policy—language restrictions in elementary school—affects integration and identification with the host country later in life. After World War I, several U.S. states barred the German language from their schools. Affected individuals were less likely to volunteer in World War II and more likely to marry within their ethnic group and to choose decidedly German names for their offspring. Rather than facilitating the assimilation of immigrant children, the policy instigated a backlash, heightening the sense of cultural identity among the minority.

*Key words:* Assimilation, Educational and language policies, Ethnicity, Cultural transmission.

*JEL Codes:* J15, Z13, N32, I28

## 1. INTRODUCTION

From France’s “burkha ban” to the politics of bilingual education in California, societies around the world grapple with the challenge of integrating ethnic minorities. Theories of nation building (Alesina and Reich, 2013) assume that policies such as imposing a national language or otherwise repressing minority culture increase homogeneity. At the same time, one strand of literature has shown theoretically that identity may be strengthened in response to policies aimed at integration (Bisin and Verdier, 2000, 2001; Bisin *et al.*, 2011; Schøyen, 2017). Whether such backlash is more than a theoretical possibility is an empirical question that has not been tested to date.

In this article, I examine the long-term effects of a particular assimilation policy: the prohibition of German in U.S. schools after World War I (WWI). When the U.S. joined the war, German speakers were increasingly treated with suspicion. Before 1917, bilingual education was common in many states that were home to German immigrants—the country’s largest group of migrants. Following the war, a number of states banned German as a language of instruction. I examine whether forced language integration affected the ethnic identity and actions of immigrant children. Did the ban on German lead to more assimilation, or did it contribute to a cultural backlash

and greater isolation from the mainstream of American culture? Using linked census records and World War II (WWII) enlistment data, I examine several outcomes for German-Americans affected by language restrictions: (i) the ethnic distinctiveness of the first names chosen for their offspring, (ii) their intermarriage rates, and (iii) their decision to volunteer for the U.S. Army during WWII.

I exploit both within-cohort variation (comparing states with and without a German ban) and within-state variation (comparing cohorts at school with older cohorts) in a difference-in-differences model. I find a strong backlash effect for the children of German immigrants and this effect is consistent across outcomes and specifications. Treated cohorts in this group were 3.6–5.7 percentage points more likely to marry endogamously (*i.e.* within their ethnic group) and about 2.5 percentage points less likely to volunteer in WWII. They also chose more distinctively German names for their children, with the estimated effect being equivalent to switching from a name like David or Daniel to a name like Adolph.

Next, I examine the mechanisms behind this reaction. I construct a simple model of intergenerational transmission, following [Bisin and Verdier \(2001\)](#) and use it to guide this part of the empirical analysis. The estimated backlash becomes weaker (or goes in the opposite direction) for Germans born to mixed couples. This establishes a link between the strength of the parents' ethnic identity and their offspring's reaction to policies affecting ethnic schooling. In line with the model, the backlash is greater in counties with a smaller share of German population. This is consistent with a cultural transmission mechanism in which parental and peer socialization are substitutes: in places where Germans constitute a smaller minority, parents try harder to shape each child's sense of ethnicity because they cannot reasonably expect that children will be socialized in their ethnic culture through peer interaction alone. The extent of the backlash was higher also in counties with a greater share of Lutherans (conditional on the share of Germans in the county), the predominantly German church that emphasized parochial schooling in the German language. The implication is that communities with a greater initial sense of ethnic identity reacted more adversely to assimilation policies.

To provide more direct evidence on the role of parental investment and rule out alternative explanations, I turn to historical information on the activities of the Lutheran Church–Missouri Synod. The number of pupils enrolled in Sunday schools increased post-war in states that experienced a German language ban. No corresponding increase was observed in other activities of the church, such as number of schools or services held in German. This suggests that the backlash was driven by increased demand of parents for German enculturation, and not by increased supply of ethnic indoctrination by the church.

My findings imply that linguistic immersion through the prohibition of German did not increase assimilation. In fact, though the effect is not always precisely estimated, the German identity of individuals with a German father was strengthened on average in response to forced monolingualism. This average effect is characterized by heterogeneity depending on the mother's ethnic background, so that, across all outcomes, a language ban led to an increase in the spread between individuals of uniform and mixed German ancestry. Furthermore, the language ban had, if anything, a positive effect on years of schooling and was thus unlikely to have reduced assimilation through its negative effect on education. There is, however, weak evidence that a strengthening of ethnic identity entailed a penalty for individuals who became more German. German-Americans affected by language laws had lower earnings. Given that schooling outcomes improved for exposed cohorts, such a drop in earnings is unlikely to have been due to lower quantity or quality of education as a result of linguistic immersion. It is, however, consistent with research emphasizing the economic payoffs of assimilation ([Biavaschi \*et al.\*, 2017](#)).

The empirical setting I examine offers a number of advantages. The timing of the legislation was plausibly exogenous, as the anti-German sentiment that motivated it was not pre-existing but

rather spurred by the war (Higham, 1998). Historical sources describe language campaigns of equal intensity and resistance on the part of German-Americans in most Midwestern states, with the final outcome often depending on the character of the local commissioners of education (Beck, 1965; Rippley, 1981). To deal with potential unobservable confounders, I focus on the state border of Indiana and Ohio, the states that banned German in schools in 1919, with their neighbouring states—Illinois, Michigan, Kentucky, West Virginia, and Pennsylvania—and create a linked data set of individuals who lived there at the time legislation was enacted in the treated states. Apart from increasing internal validity, this design allows me to observe long-run assimilation outcomes of German-Americans and to examine how the effect of the ban on those outcomes varies by the ethnic and religious composition of their home town. Finally, the case study of German-Americans yields an interesting measure of ethnic identification: volunteering for service in the U.S. Army during WWII. This is a unique historical setup in which immigrants are called upon to take sides between their host country and their country of origin.

A number of theoretical studies suggest that assimilation policies can lead to a backlash of ethnic or religious identity. Bisin *et al.* (2011) present a mechanism for the persistence of oppositional minorities.<sup>1</sup> In their model, oppositional types intensify their identification with the minority culture in response to attempts at desegregation or discrimination by mainstream society. Similarly, Carvalho (2013) predicts that bans on veiling worn by Muslim women can *increase* religiosity. Carvalho and Koyama (2016) show that, when education serves as a means of transmission of the majority culture, minorities can underinvest in education as a form of cultural resistance. This article is the first to provide empirical evidence that an identity backlash in response to assimilation policies is more than a theoretical possibility.

My research also contributes to the literature on the economics of identity (Akerlof and Kranton, 2000). Ethnic, religious, and other social identities have been shown to have a significant impact on preferences and economic behaviour (Hoff and Pandey, 2006; Benjamin *et al.*, 2010, 2016), but evidence on the determinants of identity formation is generally not causal in nature (Constant *et al.*, 2009; Battu and Zenou, 2010; Manning and Roy, 2010; Bisin *et al.*, 2016). I provide evidence on a specific mechanism through which ethnic identity can be influenced: language in school and its interaction with parental socialization. In this regard, the paper most closely related to mine is Clots-Figueras and Masella (2013).<sup>2</sup>

This study also relates to a broad literature on immigrant assimilation. Much of this research has focused on economic assimilation and the gap between native and immigrant earnings.<sup>3</sup> In addition, several papers construct measures of the speed of assimilation by looking at political (Shertzer, 2016) or cultural outcome variables (Aleksynska and Algan, 2010), such as first names (Arai *et al.*, 2012; Abramitzky *et al.*, 2018) or self-reported national identity (Manning and Roy, 2010). Dávila and Mora (2005), Neeraj *et al.* (2005), and Gould and Klor (2015) show how discrimination against Muslims in the U.S. after the 9/11 attacks reduced integration. My study contributes to this literature by identifying the effect of a specific government intervention on assimilation outcomes. Relatedly, Lleras-Muney and Shertzer (2015) investigate the effect of English-only and compulsory schooling policies in early 20th century US and find no effects on social or economic outcomes of immigrants.

1. See also Bisin and Verdier (2000), Bisin and Verdier (2001), and Bisin *et al.* (2004).

2. These authors find that instruction in Catalan, which was re-introduced in the schools of Catalonia in Spain after the Franco era, led to a stronger identification with the cause of Catalan independence and to a greater tendency to vote for Catalanist parties. My research addresses the reverse setup. Rather than focus on the effects of imposing a national language on the majority (as Catalan was for Catalonia), I examine the case of prohibiting a minority language.

3. See Borjas (1985), LaLonde and Topel (1991), Hatton (1997), Minns (2000), Card (2005), and Abramitzky *et al.* (2014).

More broadly, this article relates to a rich literature in history and the social sciences that examines the effects of education on national identity. There are many studies documenting how education, and in particular the content of the school curriculum, have been used to shape preferences, homogenize societies, and “manufacture” nations (Dewey, 1916; Freire, 1970; Weber, 1976; Colley, 1992). More recently in the economics literature, Cantoni et al. (2017) show how a new school curriculum in China had a measurable effect on the political attitudes of students. My study focuses more on the medium than on the content of education, but its results suggest that the purpose of assimilationist educational policies may not always be entirely achieved. The study of Friedman et al. (2016) in Kenya points in a similar direction. They find that more education in the context of a nationalist curriculum led to political alienation for school girls, and, if anything, heightened tribal identities instead of fostering national unity. Similarly, in the case of Zimbabwe, Croke et al. (2016) find that more education, when provided by an authoritarian regime, decreases political participation.

The article proceeds as follows. Section 2 discusses the historical background of German language schooling and the language restrictions imposed after WWI. Section 3 describes my data sources. Section 4 presents the empirical analysis. I show that the prohibition of German in school created a backlash of ethnic identity among Americans born to German parents, as measured by ethnic name choices, endogamy rates, and volunteering in WWII, and I check the sensitivity of my results along a number of dimensions. In Section 5, I provide evidence on mechanisms. I show that the backlash effect weakens among children of mixed couples, I assess how the response to legislation varies by a community’s ethnic composition and strength of ethnic identity and examine the role of parents and of churches in triggering the backlash response. I also examine whether language restrictions affected schooling and other outcomes later in life. Section 6 reviews my findings in the context of recent theory on cultural transmission and identity in economics. Finally, Section 7 concludes.

## 2. HISTORICAL BACKGROUND

This section outlines the history of the German language in U.S. schools until the early 20th century. It also discusses the reasons that led to the restriction of German as a language of instruction during and after WWI.

### 2.1. *Germans in the U.S. and the German language in schools*

Germans were the single largest foreign group that migrated to the post-colonial U.S. until at least the 1970s. German immigration started in the 17th century, increased after the failed revolutions of 1848, and peaked in the 1890s, when economic migrants replaced political refugees in the arriving immigrant cohorts. Between 1880 and 1920, Germans constituted the largest element among the foreign-born in the U.S.; in 1900, the first and second generation of Germans together accounted for more than 10% of the total U.S. population (Conzen, 1980).

As the dominant non-English speaking group, Germans established a large network of private (mainly religious) schools, in which the German language was taught and used as a medium of instruction. They also succeeded in introducing German instruction to the public schools of districts with a large German population. In cities such as Cincinnati and Indianapolis, designated German-English schools provided a form of bilingual education that included half-day instruction in German (Schlossman, 1983; Zimmerman, 2002). Such bilingual programs were favoured by German parents and supported by school officials as a way of drawing first- and second-generation German children away from private schools, which were perceived to perpetuate exclusive ethnic communities and to endanger the linguistic and cultural homogenizing function of the public

school. Some proponents of dual German-English instruction pointed out its assimilating function not just for the children of German immigrants but also for their parents. According to the Milwaukee Association of Collegiate Alumnae: “Foreign mothers, who are busy all day in their homes, have but one opportunity to acquire the language of their adopted country, and that is from their children, who bring English home from the schools” (Schlossman, 1983).

Although there is no comprehensive census of private schools and their instruction practices, individual state census records reveal the prevalence of German in parochial schools prior to WWI. According to the 1917 Minnesota Educational Census, the state counted 308 parochial schools with a total enrolment of 38,853 pupils; more than two-thirds of these schools used both German and English as a medium of instruction (Rippley, 1981).<sup>4</sup> Official statistics aside, a number of sources confirm the unofficial use of German by teachers in the classroom as a natural way of introducing first- and second-generation children of German parents to English (Schlossman, 1983). For parochial schools that employed German-born teachers and were located in predominantly German rural communities, this practice was the norm.

Despite this ethnic group’s large network of schools, the prevalence of using German and the importance placed by German-Americans on conserving their culture and a sense of *Deutschtum*, by the early 1900s Germans were fairly well assimilated—in both socioeconomic and cultural terms. In the words of Higham (1998), “public opinion had come to accept the Germans as one of the most assimilable and reputable of immigrant groups. Repeatedly, older Americans praised them as law-abiding, speedily assimilated, and strongly patriotic.”

## 2.2. WWI, anti-Germanism, and language restrictions

The outbreak of the First World War made the large German community the focus of American patriotic reaction. The growing anti-Germanism of the early war years, which was further agitated by the insistence of the German-American press on strict American neutrality, found its expression in a series of both spontaneous and organized acts of harassment and persecution once the U.S. entered the war in 1917. Numerous German-Americans were arrested as spies or forced to demonstrate their loyalty by buying liberty bonds under the threat of vandalism or tarring and feathering. The hanging of Robert Prager in Collinsville, Illinois, was the most well known in a series of lynching attacks against German-Americans (Luebke, 1974). Berlin, Michigan, was renamed to Marne in honour of the American soldiers who fought in the Second Battle of Marne. Hamburgers became “liberty steaks”<sup>5</sup> and sauerkraut consumption fell by 75% in the period 1914–1918 (*New York Times*, 25 April 1918). Moser (2012) shows that the number of German-language operas staged at the New York Metropolitan Opera fell dramatically during the war years.

The German language also came under attack. At the federal level, the 1917 Trading With The Enemy Act and also the Espionage Act required all foreign language publications to translate into English any news referring or related to the war. At the state and local level, various restrictions were placed on the use of German. The state of Iowa prohibited, among other things, the use of German over the telephone. Iowa state governor William Lloyd Harding stated in the *New York Times* in June 1918 that “English should and must be the only medium of instruction in public, private, denominational, and other similar schools. Conversation in public places, on trains, and

4. In the early 20th century, 35 out of 48 states taught some form of German in school mostly in the form of a foreign language in secondary education (Wüstenbecker, 2007).

5. An interesting parallel is the renaming of french fries to “freedom fries”, after France opposed the U.S. invasion of Iraq in 2003 (Michaels and Zhi, 2010).

over the telephone should be in the English language. Let those who cannot speak or understand the English language conduct their religious worship in their home” (Baron, 1990).

This political climate encouraged support for language restrictions in the schools. Since the war’s outbreak, nationalist organizations had propagandized against the instruction of German. A 1915 pamphlet of the American Defense League, one of the largest nationalist political groups of the time, reads as follows: “Any language which produces a people of ruthless conquistadores [*sic*] such as now exists in Germany, is not fit to teach clean and pure American boys and girls.” This propaganda merged with a pre-existing nativist movement that originated in the 19th century, but had strengthened in the early 1900s in response to the unprecedented flow of immigration to the U.S. (Kazal, 2004). During and after the war years, these attitudes were enshrined in legislation restricting foreign languages in a number of states.

Until that time, the legislative framework regulating the language of instruction in schools was heterogeneous. By 1914, 22 states had some sort of provision requiring the use of English. As documented in Edwards (1923), English had been the language of instruction in the public or common schools of some states since the end of the 19th century; in other states, such as New York and Rhode Island, English was recognized later on as the official school language to meet requirements of the compulsory schooling law. In many states, however, provisions regarding the use of foreign languages were permissive; for example, Colorado permitted German or Spanish to be taught when requested by the parents of 20 or more pupils (Luebke, 1999). The state of Ohio in 1903 allowed for German instruction in the public schools upon the demand of “75 freeholders resident in the district”, making such instruction optional “and auxiliary to the English language” in 1913 (Leibowitz, 1971).

WWI marks a clear break in the pre-existing trends of English language legislation; in the period 1917–1923, there were 23 states that prohibited the use of foreign languages as a medium of instruction or as a separate subject in elementary grades (Knowlton Flanders, 1925). Though not always explicitly targeted against German, these laws are generally viewed by legal scholars as resulting from anti-German sentiment during the war years (Van Alstyne, 1990; Bennett Woodhouse, 1992). Their main difference from previous legislation is that they applied to all schools—whether public, private, or parochial.<sup>6</sup> Since English was already the main (and most often the only) language of instruction in public schools, the laws were mainly aimed at private schools and at German-Americans, the ethnic group with the largest and oldest system of private schools in the country.

In 1923, the U.S. Supreme Court repealed the 1919 Nebraska law—and with it all legislation that restricted foreign-language education in the private schools—as a violation of the Fourteenth Amendment. Despite this ruling, most parochial schools did not re-introduce instruction in German and the number of high school students studying German, which dropped precipitously during the war years, never returned to its pre-war levels (Schlossman, 1983; Wüstenbecker, 2007).

### 3. DATA

My analysis focuses on Indiana and Ohio, the only two states that passed legislation targeted specifically against the German language. Both of these states had permissive provisions on language use in schools prior to 1919, and both provided dual language instruction programs in the public schools of their main cities, Indianapolis and Cincinnati (Schlossman, 1983). During the

6. Similar in spirit was the 1889 Bennett Law of Wisconsin, which was fiercely opposed by the state’s Lutheran and Catholic population and repealed in 1891.



period in question, two of their neighbouring states, Michigan and Kentucky, neither introduced nor had in place any language laws. Their remaining neighbours—Illinois, West Virginia, and Pennsylvania—made English the mandatory language of instruction in public schools in 1919 (Edwards, 1923). This provision did not extend to private schools, and, in Pennsylvania and West Virginia, it only applied to common English branches (which meant that foreign languages could still be taught as a separate subject in the public school). These English laws are not expected to have had a big impact, since English was already de facto the main language of instruction in these states. I therefore include all states bordering Indiana and Ohio in the control group.

I first construct a unique data set of individuals living at the border of Indiana, Ohio, and their neighbours at the time legislation was enacted and then link this data over time to later census years so as to observe choices of first names for children and intermarriage outcomes. Subsequently, to investigate whether exposure to legislation affected the national identity and patriotism of Germans later in life—as proxied by their decision to volunteer or not for service in the Second World War—I use the WWII Army Enlistment Records digitized by the National Archives. I link a subset of this data to the 1930 census in order to obtain information on the ethnic background of enlisted men.

### 3.1. *Laws*

Both Indiana and Ohio explicitly singled out German as a language to be prohibited in elementary school grades in 1919.<sup>7</sup> The law in Ohio reads as follows:

That all subjects and branches taught in the elementary schools of the state of Ohio below the eighth grade shall be taught in the English language only...Provided, that the German language shall not be taught below the eighth grade in any of the elementary schools of this state. (108 Ohio Laws, 614, 1919)

The wording was almost identical in Indiana:

All private and parochial schools...shall be taught in the English language only...provided, that the German language shall not be taught in any such schools within this state. (School Laws of Indiana, 1919)

I combine data on English-only laws with information on the age range of compulsory schooling from Goldin and Katz (2008). Because the legislation I am considering was passed in 1919, cohorts exposed to it were those that should—according to the compulsory schooling law of their respective states—be in school at the time a law was in effect. The compulsory schooling age in the period was 7–16 in Illinois, Indiana, Michigan, and Kentucky, 8–16 in Ohio and Pennsylvania, and 8–15 in West Virginia.

### 3.2. *Indiana and Ohio borders*

I use the newly digitized full count of the 1920 census to construct a unique data set of all native-born males in the 1880–1916 birth cohorts who had parents born in Germany and who

7. The only other state that explicitly prohibited German in its schools was Louisiana in 1918. This prohibition was part of a legislative package known as Act 114, which was enacted as an expedited war measure and also prohibited the use of German in public and over the phone. It was repealed by the U.S. Supreme Court in 1921.

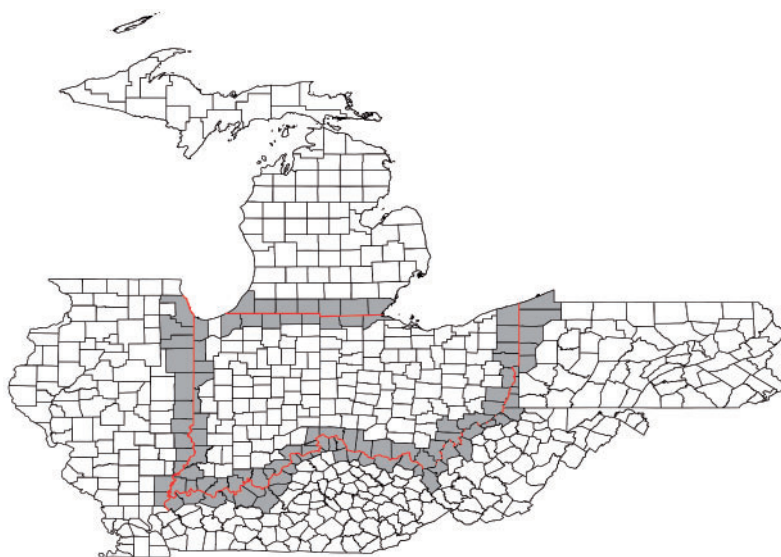


FIGURE 1  
Counties on the borders of Indiana and Ohio

TABLE 1  
*Balancedness of border counties*

	No Law	Law	Difference
Population density	144.651 (347.615)	121.4397 (177.746)	23.21 (52.49)
Share urban	0.266 (0.267)	0.330 (0.280)	-0.064 (0.054)
Share foreign-born	0.058 (0.075)	0.071 (0.080)	-0.013 (0.015)
Share German-born	0.018 (0.021)	0.022 (0.024)	-0.005 (0.004)
Share Lutheran	0.012 (0.022)	0.012 (0.019)	0.00003 (0.004)
Observations	58	47	

Notes: Data are from the 1910 county data in [ICPSR](#) and from the 1906 Census of Religious Bodies.

lived in a county on either side of the border of Indiana and Ohio with their neighbouring states (Illinois, Michigan, Kentucky, West Virginia, and Pennsylvania) in 1920—the census year closest to the introduction of these anti-German laws (see Figure 1, where the border counties are shaded).

Restricting attention to state borders is meant to increase the comparability of affected and non-affected Germans in dimensions other than language restrictions. Using 1910 county-level data from Inter-university Consortium for Political and Social Research (ICPSR) and the Census of Religious Bodies, Table 1 shows that this is largely achieved. Indiana and Ohio are slightly more urban and have a somewhat larger share of foreign-born residents, but these differences are relatively small in magnitude and not statistically significant. The share of the German-born and of Lutheran church members is largely balanced across states.



TABLE 2  
Summary statistics: border data set

	Found in 1930			Found in 1940		
	Mean	S.D.	<i>N</i>	Mean	S.D.	<i>N</i>
Married	0.695	0.460	35,886	0.790	0.407	41,875
Spouse of German ancestry	0.395	0.489	24,925	—	—	—
Number of children	2.398	1.543	20,008	2.206	1.450	25,393
Log average GNI of children	1.774	3.153	19,684	1.305	3.497	20,098
Log GNI of first son	1.358	3.662	14,309	1.095	3.775	12,816
Years of education	—	—	—	8.546	2.595	25,103
Log yearly salary earnings	—	—	—	6.920	2.144	19,305

*Notes:* The table shows summary statistics for males born 1880–1916 to German parents, who in 1920 lived in a county on either side of the border of Indiana and Ohio with Illinois, Michigan, Kentucky, West Virginia, or Pennsylvania and who were linked to the census of 1930 (left panel) or 1940 (right panel). See Section 3.2 and Section B.2 of the [Supplementary Appendix](#) for details on the construction of the GNI variables.

Using the procedure and criteria just described, I begin with a data set of 114,376 males observed in 1920. I am interested in how exposure to language restrictions affected the later assimilation outcomes of these individuals. To compile these outcomes, I use the complete-count 1930 and 1940 U.S. censuses to link records over time. Linking necessitates focusing on men—a practice followed by virtually the entire historical literature that relies on linked census data—since women change their last names after marriage and are thus much harder to locate in later census decades. Following standard census-linking procedures ([Ferrie, 1996](#); [Abramitzky \*et al.\*, 2014](#)), I start by using the phonetic equivalent of first and last name, the birthplace, and the year of birth (allowing for a two-year band around the recorded year) to locate an individual in a later census. One of the drawbacks of this procedure is that it yields a large number of records with multiple matches. Discarding these multiple matches results in loss of information. I therefore extend this process by computing the string distance between first and last names in the original data and the target census year. I use the Jaro–Winkler algorithm ([Mill, 2012](#)), which yields a measure that takes values from 0 to 1, with 1 implying that two strings are identical. I sum up the Jaro–Winkler measures for first and last name and filter multiple matches by keeping only those with the smallest value in this composite Jaro–Winkler index.

The Jaro–Winkler distance allows for further refinement of the matched data set by providing a way to discard names that are sufficiently different in origin and target census years, and thus getting rid of false positive matches. The higher the value of the Jaro–Winkler distance chosen as a threshold, the larger the share of the initially matched data that is discarded. I choose as a threshold the Jaro–Winkler value for which the change in the share of matched data dropped is maximized. Intuitively, I increase the Jaro–Winkler—and thus the precision of the match—until the point where increasing it further would imply losing too many observations. This procedure (further detailed in Section B of the [Supplementary Appendix](#)) leaves me with a total of 42,624 unique matched records in either 1930 or 1940. Table 2 provides summary statistics for the linked data set. Figure 2 shows the locations in 1920 of all individuals successfully linked in either 1930 or 1940.

**3.2.1. First names.** I use the names that individuals in my sample choose for their children as a proxy for ethnic identity. Names have an indisputable cultural component and to a great extent reflect the parents’ racial, ethnic, and social background and preferences ([Lieberson, 2000](#); [Fryer and Levitt, 2004](#); [Head and Mayer, 2008](#); [Cook \*et al.\*, 2014](#)). As such, the choice of first names for their offspring is indicative of parental tastes and, for immigrants, of assimilation into the host society ([Abramitzky \*et al.\*, 2018](#)). In particular, if cohorts affected by an anti-German

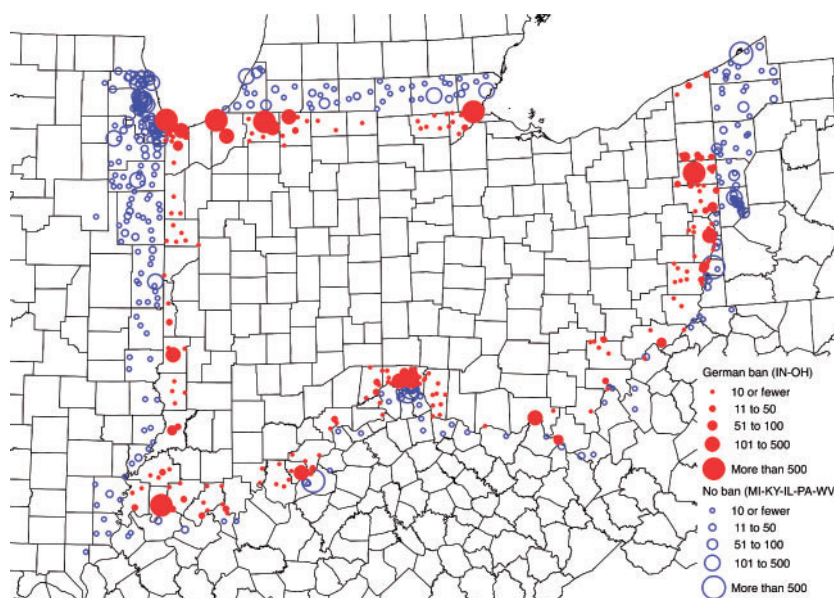


FIGURE 2

Locations of linked data set in 1920

Notes: The map shows the town-level location of all males, who were born 1880–1916 to German parents, were living in a border county in 1920 and who could be linked to the 1930 or 1940 census.

law choose to give their offspring names that are less German-sounding and more common among natives, then that would indicate an assimilation effect of language restrictions.<sup>8</sup>

In order to measure a name's ethnic content, I follow [Fryer and Levitt \(2004\)](#) in constructing an empirical index of **German name distinctiveness**, by using census data on first names and national origin. This **German name index (GNI)** captures how much more frequent a name is among the population of German origin compared with the rest of the population. A name found only among the German-born would have index value 100, whereas a name given to no individuals of German origin would have index value 0. To capture the Germanness of a name given to a child of a particular birth cohort, I calculate the relative frequency of the name among individuals born in the previous ten birth cohorts. These are the names most likely to be considered by the parents when making naming decisions for their children. Details on the construction of the index are provided in Section B of the [Supplementary Appendix](#).

Table 3 provides an overview of what this index captures in the 1930 5% Integrated Public Use Microdata Series (IPUMS) sample. The left panel shows the 10 names with the highest value of the name index that were given to more than 1,000 individuals in 1930; all are distinctively German-sounding. Not all distinctive names are common among Germans, but many of these names, including Christian and Herman, are also on the list of most popular names among German immigrants. The right panel of Table 3 lists the 10 most popular names with a zero GNI

8. [Algan et al. \(2013\)](#) show that the economic penalty associated with culturally distinctive names is an additional important determinant of parents' naming decisions. In the current setup, there is no clear reason to believe that local labour market conditions faced by children differ depending on their parents having been or not affected by language laws in elementary school. If greater discrimination in states with a language law persisted to the children's generation, this should in fact have led parents to give less and not more German names to their children.

TABLE 3  
*Most and least German-sounding names in the 1930 census*

Highest-scoring				Lowest-scoring			
Name	Total	Germans	GNI	Name	Total	Germans	GNI
Hans	1281	325	98.91	Clyde	7359	0	0
Karl	1542	267	96.21	Russell	6049	0	0
Otto	5693	962	94.78	Melvin	5687	0	0
August	5781	1264	92.30	Patrick	5381	0	0
Christian	1215	179	90.99	Leroy	5187	0	0
Adolph	3231	385	90.27	Warren	5077	0	0
Gustave	1271	231	90.21	Marvin	4588	0	0
Emil	4261	515	89.52	Jim	4231	0	0
Rudolph	3372	296	89.06	Glenn	3898	0	0
Herman	11446	1399	88.27	Leslie	3796	0	0

*Notes:* The table shows the values of the German name index for the 10 highest-scoring (left panel) and 10 lowest-scoring (right panel) names of males born in 1910 in the 1930 5% IPUMS sample. Highest-scoring names are chosen among names that appear at least 1,000 times in the 1930 sample and are ordered by their GNI value; lowest-scoring names are ordered by popularity. See Section 3.2 and Section B.2 of the [Supplementary Appendix](#) for details on the construction of the GNI.

value. Names such as Clyde, Russell, and Melvin are characteristically un-German in that they had been given to no German-born individuals in the 1930 IPUMS sample.

Because the GNI is computed based on the names of foreign-born Germans, many names in my sample of the second generation have a GNI value of 0. I take the logarithm of the GNI to deal with this skewed distribution and to allow for an intuitive interpretation of results in percentage terms.<sup>9</sup> In the main empirical analysis, I will use both the logarithm of the average GNI of all children and the logarithm of the GNI of the first son as outcome variables that proxy for ethnic identity.<sup>10</sup>

**3.2.2. Inter-marriage.** Inter-marriage has been characterized as “the final stage of assimilation” (Gordon, 1964). Unlike first names, it is not a pure choice, but a general equilibrium outcome, determined by others’ preferences and by the constraints of the marriage market. However, it is arguably a good indicator of immigrant integration in the host country, as it reflects acceptance of the host culture on the part of the immigrants and vice versa. I investigate the extent to which being exposed to restrictive legislation at school affects the probability that second-generation German-Americans end up marrying within their own ethnic group.

How can marriage decisions be affected by the language of instruction in school? The choice of a spouse involves an important preference component (Fisman et al., 2008; Banerjee et al., 2013), and U.S. society has historically been characterized by marriage segregation along racial, religious, and ethnic lines (Pagnini and Morgan, 1990; Fryer, 2007). Bisin et al. (2004) show theoretically how parents seeking to socialize their children into their culture will marry homogamously, and they demonstrate that U.S. patterns of religious endogamy are in line with this prediction. To the extent that the language of instruction in school affects the ethnic preferences of second-generation immigrants, we can expect to see changes in marriage choices later in life as one response to language restrictions. In particular, if removing German from the curriculum had the effect suggested by proponents of the policy, then English-only instruction should lead to greater assimilation as reflected in higher inter-marriage rates. That might happen because, in the first place, children would no longer be indoctrinated “with the German language, customs,

9. I use  $\log(\text{GNI}+x)$ , where  $x$  is a small positive number, to avoid loss of data where  $\text{GNI} = 0$ .

10. Male names continue to be more traditional than female ones even in modern-day Germany (Gerhards, 2005).

and prejudices of the Fatherland ... against the social and religious customs of the American communities in which they claim citizenship.”<sup>11</sup> Greater familiarization with the American language and culture, as the Americanization movement aimed to inculcate, would make these children prefer American spouses later in life. Second, to the extent that such Americanization would make these offspring more receptive to social environments other than their closed ethnic communities, the market for marriage partners would contain more non-ethnic members which would increase the likelihood of intermarriage.

The earlier U.S. censuses pose some difficulties for determining an individual’s ethnic background. In 1940, the question on parental birthplaces was posed to only 5% of the universe. This means that I can observe the ethnic background of a native-born spouse only in 1930. In this census year, treated cohorts are observed at an age when they are likely not yet married (ages 14–27). Other than leaving us with a small number of observations, comparison of these cohorts between states with and without a language law should still yield unbiased estimates, though they are not likely to be representative of the general population of German-Americans.<sup>12</sup>

### 3.3. *WWII enlistment records*

Data on men who enlisted in the U.S. Army during WWII are from the *Army Serial Number Electronic File, ca. 1938–1946*. The database is the end product of digitizing the original WWII draft computer punch cards by the National Archives and Records Administration. The complete database comprises nearly 9 million records of enlistments in the Army, the Enlisted Reserve Corps, and the Women’s Army Auxiliary Corps. Each entry provides information on enlistment details (Army serial number, enlistment date and place, enlistment term, and Army component), and also on several demographic and socioeconomic characteristics of the enlistee (nativity, race, civil status, birth year, birthplace, education, and occupation).

From this universe, I restrict my attention to individuals born in Indiana, Ohio, or their neighbouring states during the period 1880–1916, whom I match to legislation based on their state of birth. Because the enlistment database does not contain information on the birthplace of an individual’s parents, I perform a procedure, similar to the one described in Section 3.2, that links enlistees to the 1930 census and determines their ethnic origin. This is not the census year closest in time to the enlistment date range, but it is the closest one for which I can obtain information on parental nativity (since this variable is not generally recorded in the 1940 census).

**3.3.1. Volunteers.** After Japan attacked Pearl Harbor in early December 1941, Nazi Germany declared war on the U.S. Following their country’s entry to WWII, thousands of American men volunteered for service. The decision to volunteer is motivated by patriotism and, in the case of first- or second-generation immigrants, it clearly signifies a strong identification with their host country. Especially for Germans, who would be called to fight against the country of their parents, a decision to volunteer is an unmistakable indicator of assimilation.

It is not straightforward to determine whether a person volunteered for the Army or was conscripted. According to the draft classification, enlisted men are those members of the Armed Forces of the U.S. who volunteered for service. These individuals can be identified by their serial numbers, which belong to the 11 through 19 million series.<sup>13</sup> However, it was possible for a drafted

11. “The German language school question”, *The Outlook* (26 February 1919).

12. For example, [Chiswick and Houseworth \(2011\)](#) document a higher likelihood of endogamous marriages among individuals who marry young.

13. Army Regulation no. 615-30, 1942.

TABLE 4  
Summary statistics: WWII enlistments

	All			German parents		
	Mean	S.D.	N	Mean	S.D.	N
Age	19.629	4.863	160,780	22.358	5.768	2,679
Married	0.011	0.107	160,780	0.014	0.120	2,679
With dependents	0.107	0.309	160,780	0.155	0.362	2,679
Volunteer	0.109	0.312	160,780	0.079	0.270	2,679
High school graduate	0.374	0.484	160,780	0.274	0.446	2,679
College graduate	0.064	0.244	160,780	0.045	0.207	2,679

*Notes:* The table reports summary statistics for males who enlisted in the U.S. Army between 1940 and 1942 and were linked to the 1930 census. The data comprise cohorts born 1880–1916 in Indiana, Ohio, Illinois, Michigan, Kentucky, West Virginia, and Pennsylvania to German parents. The right panel restricts the sample to individuals with German parents. Volunteers are identified as having a serial number in the 11 through 19 million series.

man to enlist in the regular army as a volunteer prior to his induction; doing so gave him more say in the choice of unit and conditions of service. This possibility introduces measurement error when serial numbers are used as a method to identify volunteers, yet the estimation procedure will not be biased provided this error does not differ systematically across cohorts and states. Voluntary enlistment was ended by presidential executive order in 1942, so I restrict my attention to men enlisted between 1940 and 1942. Summary statistics for the linked sample are provided in Table 4.

#### 4. EMPIRICAL ANALYSIS

##### 4.1. Identification

My identification strategy is a difference-in-differences approach that is based on comparing cohorts of school age and cohorts too old to be at school between states with and without a language law. My main specification takes the form:

$$Y_{isc} = \alpha + \beta T_{cs} + \lambda_c + \theta_s + \delta \mathbf{Z}_{isc} + \varepsilon_{isc}, \quad (1)$$

where  $T_{cs}$  is an indicator for individuals living in a state with a law and who were within the age range for compulsory schooling (CSL) at the time that law was in place. The terms  $\lambda_c$  and  $\theta_s$  signify cohort and state of residence (in the case of the border county data set) or birth (in the case of the WWII enlistments data set) fixed effects.  $\mathbf{Z}_{isc}$  is a vector of name string properties that affect the probability of a record being matched in a later census.<sup>14</sup> The coefficient of interest is  $\beta$ : the estimated average effect of legislation on exposed cohorts.

In the above specification, state and cohort fixed effects account for average differences in the outcome variable across states and cohorts. As with every DiD approach, the identifying assumption is that there exists no omitted time-varying and state-specific factor correlated with both the passage of language laws and with the outcome variables. Because it is difficult to completely rule out this concern in an observational setting, I will report specifications that include interactions of state-level variables recorded before the enactment of the law and that are plausibly correlated with its passage (most notably the share of the German element in a state's population) with an indicator for the treated cohort ( $D_c$ ).

$$Y_{isc} = \alpha + \beta T_{cs} + \lambda_c + \theta_s + \delta \mathbf{Z}_{isc} + \gamma \times \text{German share}_s \times D_c + \varepsilon_{isc} \quad (2)$$

14. These include the length and commonness of the first and last name. Commonness is computed as the share of people in the 1920 census with the same first or last name.



I cluster standard errors at the state-border segment level, and always report  $p$ -values from the wild bootstrap procedure (Cameron *et al.*, 2008) to account for the fact that the number of clusters is small ( $N = 14$ ). In Section 4.3, I show that results are robust to alternative methods of inference.

It is worth remarking that I do not know precisely which children of German origin attended schools where German was actually used as a language of instruction. This lack of sharp variation across cohorts in terms of language used in school will likely bias all estimates toward zero, since children in non-German schools will be either unaffected by the ban, or—in the case of spillovers across schools—less affected by it than children who actually experienced a change in the language regime. In any case, the DiD coefficient should be an unbiased estimate of the intention-to-treat, that captures the effect of the law on the entire population of Germans in relevant cohorts (including non-compliers).

**4.1.1. Discrimination.** The main DiD identifying assumption will be violated if legislation is endogenous to factors that directly affect assimilation outcomes. A plausible scenario is that Indiana and Ohio introduced restrictive laws because those states were characterized by relatively more anti-German sentiment. In that case, there should be greater discrimination against Germans, which would affect some outcomes (such as intermarriage) directly and not through any mechanism related to language used in school. This scenario is unlikely for two main reasons. First, in order for differences in the intensity of discrimination to have a differential effect on the younger cohorts exposed to school laws, these differences would have to be increasing over time. Yet, we expect the opposite to be true because anti-Germanism peaked during and shortly after the war years and began to subside thereafter. In particular for endogamy, it is equally (if not more) likely that discrimination would affect marriage outcomes for the control cohorts born 1890–1900—who would be at a marriageable age exactly during the war years—than the treated cohorts born after 1903. Second, sources point to all states conducting a campaign of similar intensity against German during and after the war. Beck (1965) reports that both Ohio and Michigan had many proponents of a language ban, and language restrictions in both states faced militant opposition from Catholic and Lutheran churches. That German was banned in Ohio, but not Michigan, was due largely to idiosyncratic factors (Ripley, 1981).<sup>15</sup>

**4.1.2. Other legislative changes.** A concern similar in nature to the one above is that Indiana and Ohio introduced additional changes to the elementary school curriculum contemporaneously with the German language ban, or passed other anti-German laws that could have differentially affected younger cohorts. To rule out this possibility, I resort to legal sources and secondary bibliography listing school-related and other laws in the period of interest.

The first source is Knowlton Flanders (1925), a compilation of all state-level legislative changes affecting the elementary school curriculum between 1903 and 1923. I focus on changes in curricular prescriptions that relate to nationalism, as well as to religious education (and could have thus affected the function of parochial German schools). Table D.1 in the Supplementary Appendix presents all relevant changes enacted in the sample states between 1913 and 1923. No laws relating to elementary schools besides the German ban were passed during the period in Indiana. Ohio did introduce a number of changes in the nationalist content of the curriculum,

15. Lleras-Muney and Shertzer (2015) find that the only consistent predictors of English-only language legislation enacted in the 1910s were the share of immigrants and recent immigrants, and the length of compulsory education in a state. Consistently with this finding, border counties in Ohio and Indiana have a higher share of immigrants, though the difference is not significant (Table 1).



such as a course on citizenship, and courses on the U.S. and State constitution. We would expect these changes to affect all immigrants, and not Germans in particular—something that the data on volunteering will allow us to test. For citizenship specifically, the law required a course of study which “shall include American government and citizenship in the seventh and eighth grades”, and so did not apply to all treated cohorts in the same way as the language ban. Nonetheless, in Section 4.3, I show that results do not change when excluding Ohio from the analysis. When Ohio is included, it is possible that the estimated effect of the German ban is compounded by these additional provisions.

Second, one also notices that a number of curricular prescriptions aimed at instilling nationalism were introduced during the period in control states. Most notably, Illinois and West Virginia made English the mandatory language of instruction in public schools. To the extent that this provision had similar, albeit likely weaker, effects than the German language ban, it should bias any estimated effects downwards. One may be more worried about two other provisions: mandatory flag display (West Virginia) and the teaching of patriotic songs (Kentucky). If these practices instilled nationalism in immigrants living in these states, any differential strengthening of German identity in Indiana and Ohio could be a result of this development rather than of the language ban. Though these effects are not expected to be German-specific, I show that results are robust to excluding Kentucky and West Virginia from the analysis.<sup>16</sup>

Finally, some elements of the curriculum were removed in control states, most notably courses on history and civil government in Illinois, Kentucky, and West Virginia. These changes would have, if anything, weakened American identity among immigrants in control states, and should thus work against a backlash effect for treated cohorts.

Knowlton Flanders (1925) reports no changes in the sample states in terms of religious education or the funding of religious schools, that would be expected to affect pupils in denominational German schools. To address the possibility that other legislative changes, beyond those applying to elementary schools, were enacted during the period, I consult additional sources. The General Code of Ohio (Page and Adams, 1912; Page, 1921) reports all laws in place in the state of Ohio in the year of its publication. I look for changes between the 1910 and 1921 editions that could have differentially affected school-aged children. I undertake a similar investigation in the 1921 supplement of the Indiana Statutes (Burns, 1921), which lists all laws voted by the Indiana State Assembly between 1915 and 1921, along with their year of enactment. None of these sources indicates any other relevant legislative changes in the treatment states during the period.

#### 4.2. *Main estimates*

The main results for naming patterns are illustrated graphically in Figure 3. The figure plots the density function of the log GNI of the first son, for treatment and control cohorts. For older cohorts, the GNI distribution indicates a slightly higher frequency of distinctively German names in control states. For treated cohorts, the pattern is reversed, with the younger cohort in Indiana and Ohio experiencing a marked shift in the density to the right.<sup>17</sup>

Table 5 reports estimated coefficients derived from a regression of equation (1)’s form. The dependent variable is the log average GNI of all children in Panel A and the logarithm of the index for the first son in Panel B. The estimated effect of the law is positive in all specifications.

16. There are also changes in laws that are not expected to have an effect on national identity. For example, Kentucky and Pennsylvania introduced days of special observance between 1913 and 1923, but these are Temperance Day in Kentucky, and Bird Day in Pennsylvania.

17. A Kolmogorov–Smirnov test fails to reject the null of equality of distributions at the 90% confidence level in the left panel ( $p$ -value  $< 0.1608$ ), while it rejects the null at the 99% level in the right panel ( $p$ -value  $< 0.003$ ).

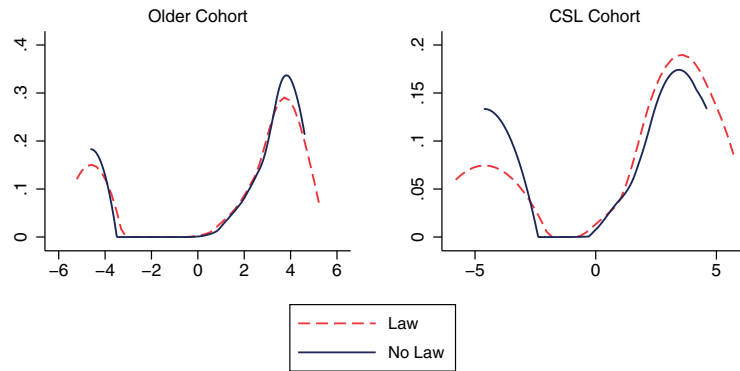


FIGURE 3

Densities of log GNI of first son by cohort

*Notes:* The figure illustrates, for the linked border data set, the kernel density of the logarithm of the GNI of the first son. The panel on the left plots this density for the cohort too old to have been in school (by compulsory law) at the time German was banned; the right panel plots the density for the treated cohort.

TABLE 5  
Baseline results: border data set

	[1]	[2]	[3]	[4]
Panel A: Dep. variable is Log average GNI of children				
Law $\times$ CSL age	0.473 (0.279) [0.1191]	0.884** (0.309) [0.0801]	0.920** (0.308) [0.0631]	0.794** (0.334) [0.1291]
Observations	26334	26334	26334	26334
R-squared	0.0494	0.0515	0.0547	0.0553
Panel B: Dep. variable is Log GNI of first son				
Law $\times$ CSL age	1.231*** (0.280) [0.00200]	1.615*** (0.300) [0.0230]	1.638*** (0.299) [0.0230]	1.310*** (0.241) [0.0240]
Observations	18459	18459	18459	18459
R-squared	0.0162	0.0194	0.0290	0.0295
Panel C: Dep. variable is Spouse German				
Law $\times$ CSL age	0.0472** (0.0164) [0.0781]	0.0573*** (0.0183) [0.0691]	0.0365** (0.0148) [0.0941]	0.0389*** (0.0105) [0.0290]
Observations	24925	24921	24921	24921
R-squared	0.0510	0.0523	0.0634	0.0636
Additional controls	N	Y	Y	Y
County FE	N	N	Y	Y
State trends	N	N	N	Y

*Notes:* The sample consists of males, born 1880–1916 in the U.S. to German parents, living in a border county in 1920 and who were linked to the 1930 census (Panel C) or the 1930 and 1940 census (Panels A and B). All regressions include residence state in 1920 and birth cohort fixed effects, and controls for the following name string properties: first and last name length and first and last name commonness. Regressions in Panels A and B include a census year indicator. Additional controls in Panels A and B include the share of Germans in the state in 1910 and border segment indicators, all interacted with an indicator for the treated cohort. In Panel C, they include the number of first and second generation German women born in the 10 years before (and including) each birth cohort. Column [4] controls for a linear state-specific trend fitted to the control cohorts. Standard errors are clustered at the state  $\times$  border segment level.  $p$ -values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

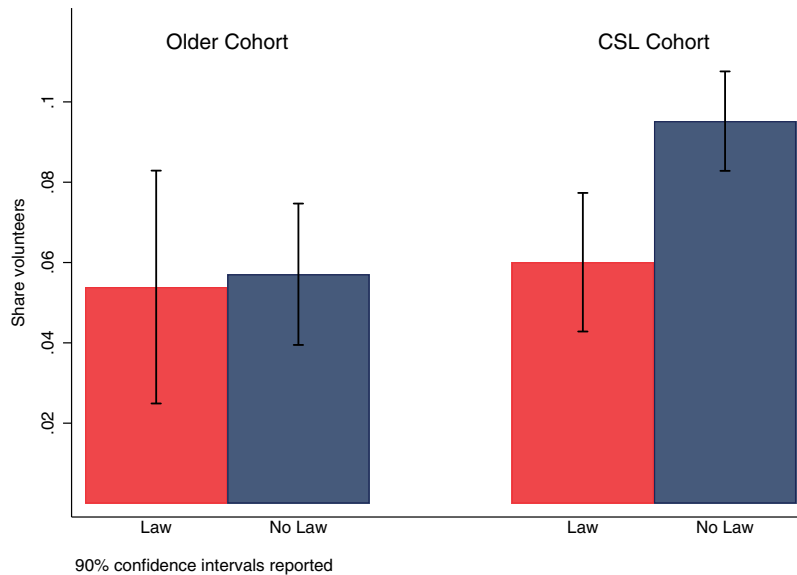


FIGURE 4

Share of volunteers by cohort and law status

Notes: The bars on the left show the share of U.S. Army volunteers by language law status for the cohort too old to have been in school (by compulsory law) at the time German was banned; the bars on the right plot the respective share for the treated cohort.

Column [1] controls only for properties of the name string. Column [2] inserts as controls the share of first- and second- generation Germans in 1910 and indicators for each border segment, interacted with an indicator for the treated cohort; hence these regressions partially account for the effects of time-varying state-specific and border segment-specific unobservables. Columns [3] and [4] include county fixed effects and linear state-specific trends, respectively. The magnitude of both estimates is only slightly reduced after inclusion of these trends. Taken together these results suggest a backlash effect resulting from exposure to a German ban. The magnitude of the interaction coefficient for log GNI is meaningful: in the specification of column [3] of Panel B, it implies that exposure to a language law leads fathers to switch from an Anglo-Saxon name like Lester to a neutral name like Robert, or from a neutral name like Daniel, to a Germanic name like Franz or Adolph.

Panel C reports estimates with endogamy as the dependent variable. The baseline effect of the language ban reported in column [1] suggests that exposure to the law increases endogamy. There are two factors that affect endogamy rates: one is preferences for ethnic mixing and the other is the size of the marriage market. These two factors do not necessarily move in the same direction. For example, while it is likely that intermarriage across ethnic lines is less desirable in smaller, and potentially more traditional communities, it is also the case that the lack of potential partners inside one's ethnic group makes intermarriage *more* likely in those places. In an imperfect attempt to better isolate the effect of ethnic preferences, I therefore control for the size of the marriage market (Banerjee et al., 2013; Voigtländer and Voth, 2013). This is done in column [2] which includes the share of first and second generation German women born in the ten years after (and including) each birth year in the data set. Inclusion of this time-variant control increases the magnitude of the coefficient. The estimate is little affected by the inclusion of county fixed effects and linear state trends in columns [3] and [4]. The magnitude implies that exposure to a

TABLE 6  
Baseline results: WWII enlistments

	[1]	[2]	[3]	[4]	[5]	[6]
Dep. variable:	Volunteer					
Law $\times$ CSL age	-0.0256** (0.00696) [0.004]	-0.0235** (0.00928) [0.004]	-0.0243** (0.00986) [0.004]	-0.0370*** (0.00785) [0.460]	0.00595 (0.0414) [0.903]	-0.00389 (0.00784) [0.768]
Law $\times$ CSL age $\times$ German parents						-0.0313* (0.0149) [0.116]
Observations	2679	2667	2667	2667	5443	160246
R-squared	0.0261	0.0641	0.0643	0.0698	0.0777	0.0746
Additional controls	N	Y	Y	Y	Y	Y
Share German in state in 1910 $\times$ Cohort FE	N	N	Y	Y	N	N
State trends	N	N	N	Y	N	N

*Notes:* The sample consists of males born 1880–1916 in Indiana, Ohio, Illinois, Michigan, Kentucky, West Virginia, or Pennsylvania, who enlisted in the U.S. Army between 1940 and 1942 and who were linked to the 1930 census. In columns [1]–[4], it is restricted to individuals with German parents and in column [5] to individuals with Italian parents. All regressions include state-of-birth and birth cohort fixed effects and control for the following name string properties: first and last name length and first and last name commonness. Column [3] includes an interaction of the share of Germans in the state in 1910 with an indicator for the treated cohort. Columns [2]–[6] control for marital status, the number of dependent family members, and enlistment year fixed effects. Column [4] controls for a linear state-specific trend fitted to the control cohorts. Standard errors are clustered at the state level.  $p$ -values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

ban of German in school increases the likelihood of endogamous marriage by 3.6–5.7 percentage points.

This analysis suggests that the removal of German from elementary schools led to a backlash, a significant strengthening of German identity for children of German couples, as measured by naming patterns and endogamy rates. I next turn to WWII volunteering; this is a novel and informative proxy of ethnic identity that has the additional benefit of capturing a clear individual decision.

The results for volunteering rates are presented graphically in Figure 4. As expected, volunteering rates are lower for older cohorts and increase for younger ones. While the difference in the share of volunteers between states with and without a law is nearly zero for cohorts unaffected by the language ban, there is a 3.5 percentage points gap for younger cohorts. This difference stems almost entirely from an increase in volunteering rates among younger cohorts in control states; volunteering in Indiana and Ohio practically remains at the same level as for older cohorts.

Table 6 reports regression results for volunteering rates. In column [1], the estimated coefficient suggests that exposure to language laws decreases the likelihood of volunteering by 2.6 percentage points. Considering that the average volunteering rate among younger cohorts is 11%, this effect is large. Column [2] incorporates an enlistment year dummy and two additional control variables: one indicator for married individuals and another one for dependent family members. Each of these factors reduces the probability of volunteering in the U.S. Army, but their inclusion has little effect on the magnitude of the estimated coefficient. Column [3] introduces an interaction of an indicator for the treated cohort with the share of Germans in the state in 1910; this leaves both magnitude and significance of the coefficient practically unchanged. Controlling for linear state trends (column [4]) increases the magnitude of the estimated coefficient, with the effect corresponding to nearly one-third of the average volunteering rate among younger cohorts.

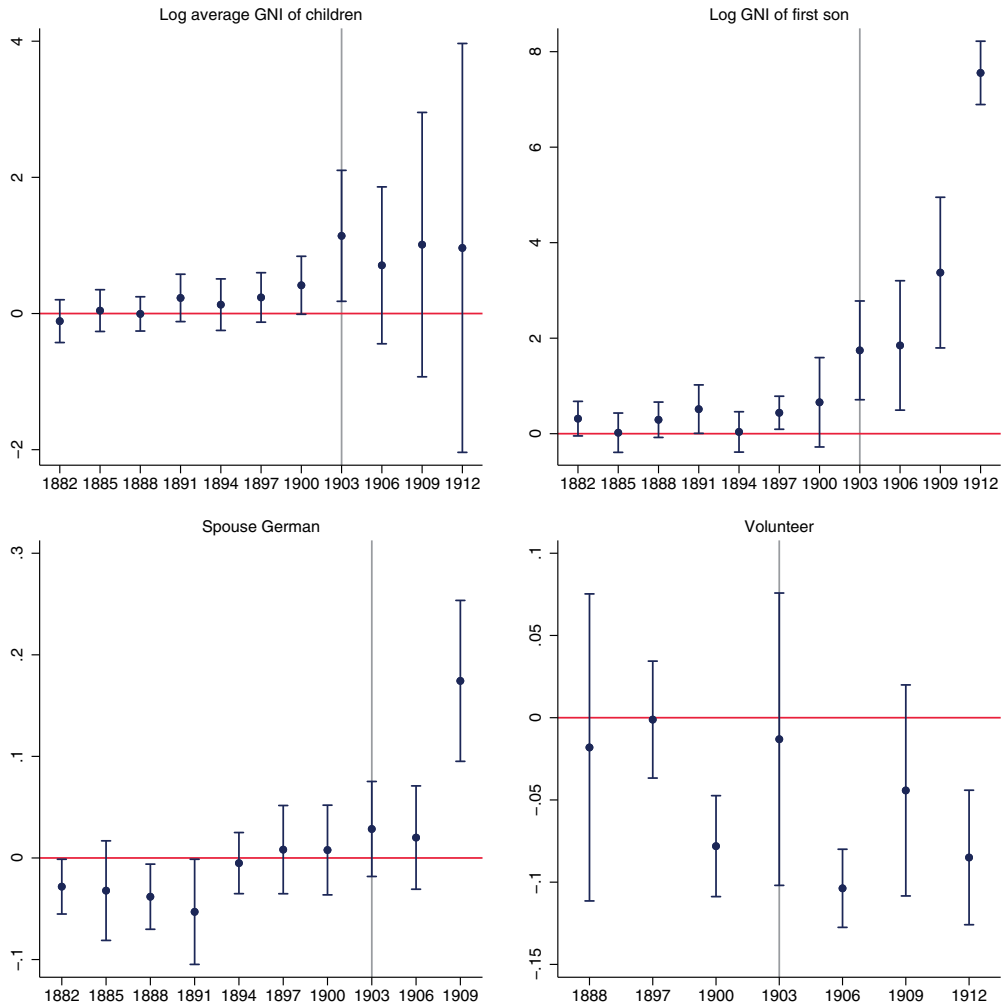


FIGURE 5

Estimated effects of language ban by birth cohort bin

*Notes:* The figure shows coefficient estimates and 95% confidence intervals from a regression of each outcome on state and birth cohort fixed effects and a set of interactions of 3-year birth cohort bins with an indicator for a language ban. The vertical line in year 1903 indicates the first cohort to be affected by the language ban.

#### 4.3. Robustness

Tables 5 and 6 already demonstrated the robustness of results to time-varying state-level differences and linear state trends. Here, I report a number of falsification tests, which show that no backlash effect is present in different periods or for groups of immigrants other than Germans. I also consider alternative computations of outcome variables and conduct robustness checks to deal with the potential confounding effect of other legislation and selective migration. Finally, I assess the sensitivity of results to alternative methods of inference.

**4.3.1. Pre-trends.** Figure 5 depicts graphically the absence of differential trends in outcomes for older cohorts. It plots the interaction coefficients of twelve birth cohort bins with

a dummy for states with a language ban in regressions which include state and birth cohort fixed effects. The upper panel shows that the Germanness of children's names was not trending upwards for individuals in Indiana and Ohio too old to have been affected by the language ban. The difference in the log GNI is not significantly different from zero for any of the cohorts born before 1903, the year marking the first affected cohort of German-American students. Furthermore, in the case of the log GNI of the first son, the magnitude of the estimated coefficient peaks for the cohorts born between 1907 and 1912, the ones with the maximum exposure to the ban before the law's repeal in 1923. Effects are similar for endogamy and volunteering rates, though more noisily estimated due to the inherent limitations of the data related to these two outcomes: treated cohorts are too young to be married in 1930, and the subset of matched enlisted men with German parents is small. Despite the noise, there is no indication of pre-trends in either case.

**4.3.2. Alternative control groups.** The enlistment data set further allows me to compare the behaviour of second generation Germans with that of other immigrant groups and of the general population. The removal of German from school curricula should affect German-American children who were formerly taught in this language but should not affect other immigrants or natives. Column [5] of Table 6 shows that the law had indeed no effect on enlisted native-born individuals of Italian ancestry. While Italians were the other large immigrant group of the time, they were much less educated and did not have an organized network of ethnic schools, like the Germans. The coefficient for Italian-Americans is indistinguishable from zero. More broadly, I can compare the difference in volunteering rates of second-generation Germans across states and cohorts with the respective difference for the rest of the sample. This approach gives rise to a triple-differences specification of the form

$$Y_{isc} = \alpha + \lambda_c + \theta_s + \beta_1 T_{cs} + \gamma_1 G_{isc} + \gamma_2 G_{isc} \times D_c + \sum_{s=1}^7 \gamma_3 G_{isc} \times \theta_s + \beta_2 T_{cs} \times G_{isc} + \delta \mathbf{Z}_{isc} + \varepsilon_{isc}, \quad (3)$$

where  $G_{isc}$  is an indicator for individuals with German parents. The coefficient  $\beta_2$  now identifies the average effect of legislation on affected cohorts of German-origin individuals. Column [6] of Table 6 reports estimates from this specification. While the effect of the law is negative and significant at the 10% level for German-Americans, it is near zero for potentially treated cohorts of non-German origin. This check is important, because the language ban was the only legislation enacted during the period that is expected to have a German-specific effect. Zero effects for other immigrant groups indicate that estimates presented so far do not capture the effect of other changes of a nationalist character introduced contemporaneously with the German ban in the elementary school curriculum.

**4.3.3. Alternative calculation of the GNI.** German distinctiveness of first names is computed using information from the entire U.S. population born in the ten years before each birth cohort. The cohort restriction follows the logic in Abramitzky *et al.* (2018), which suggests that a parent's idea of what constitutes a German name is shaped by naming patterns observed at the time of their child's birth, particularly among children relatively close in age to theirs. I assess the robustness of results to varying the range of years used for the GNI calculation. Rows 2 and 3 of Table D.2 in the Supplementary Appendix show that effects remain substantially unchanged when considering cohorts born in the 20 years before the birth of one's child, or all cohorts older than one's child.



It is also possible that parents benchmark the Germanness of names based on naming patterns they observe in the area where they live. To account for this possibility, rows 4–6 of [Table D.2](#) in the [Supplementary Appendix](#) replicate the estimation using a version of the GNI computed in the population of border counties only. This version of the GNI is noisier because its computation relies on a smaller number of observations for each first name. This fact, and the smaller set of names available for the index's calculation in the border counties reduce the precision of the estimates in some cases, but results are comparable in magnitude to the baseline. Overall, the method of calculation of the GNI does not substantially affect estimated effects.

**4.3.4. Confounding legislation.** As explained in [Section 4](#), alongside banning German, Ohio introduced a number of other changes in the school curriculum that were aimed at instilling national identity and could have plausibly compounded the effect of the language ban. Panel A of [Table D.3](#) in the [Supplementary Appendix](#) shows that dropping Ohio from the sample and considering Indiana (which did not introduce any other change to the school curriculum in the same period) as the only treated state, if anything leads to a larger estimated backlash effect along all outcomes. One interpretation for this finding is that additional legislation introduced in Ohio during and after the war years actually succeeded in instilling nationalism among immigrants, so that the estimated effect when Ohio is included in the sample is downward biased.

Panel B of [Table D.3](#) in the [Supplementary Appendix](#) drops instead Kentucky and West Virginia, two control states which introduced courses on patriotism and mandatory flag display in schools during the period. Results remain substantively unchanged, suggesting that the estimated backlash effect is not driven by increased (American) patriotism in control states.

**4.3.5. Selective migration.** One challenge to identification is endogenous sorting across the border. Given that the census nearest to the passage of language legislation is 1920, I do not observe individuals in the data set until *after* the law was enacted. It is conceivable that parents with a strong desire to send their children to a German school could have moved across the border in response to (or in anticipation of) legislation. The effect I capture would then be driven by this compositional change and would be most likely downward biased, since treated cohorts in Indiana and Ohio would be characterized by a weaker ethnic identity to begin with. Since I do not know the migration history of individuals in the years before 1919, I can assess the relevance of sorting only imperfectly: by examining the share of people who were born in a state other than the one in which they are observed in 1920. This share is plotted in the lower panel of [Figure D.1](#) in the [Supplementary Appendix](#). The two sets of states are clearly following parallel trends for cohorts born after 1890, and there is no indication that German families with school-aged children in Indiana and Ohio move out of those states and into their neighbouring states in response to the legislation.

Another way in which differential migration could bias my findings is if relatively more assimilated German-Americans (*i.e.* those with lower endogamy rates) were more mobile and thus more able to migrate out of states that banned German in the schools. Out-migration rates are not directly observable in my data. I assess the possibility that German out-migration rates were higher for younger cohorts in Indiana and Ohio using the 1920 1% IPUMS sample. [Figure D.2](#) in the [Supplementary Appendix](#) plots the share of males with German parents who were born in one of the four states in the data set, but who did not live in the same state in 1920. With the exception of a jump in the out-migration rate for cohorts born around 1890 in Ohio and Indiana, this share, though volatile, is similar (and low) for states with and without a law and does not differ for cohorts affected by the language ban.

I conduct an additional check to address concerns with selective migration. The baseline analysis is inferring treatment status based on the state of residence in 1920. In an attempt to bound the bias of the coefficient if migration between 1919 and 1920 was selective in response to the law, I assume that all individuals born in Indiana and Ohio, who were observed to live in another state in 1920, left the treatment states because of the law. I therefore assign all these individuals to the treatment group and replicate the specification in column [3] of Table 5. Panel A of Table D.4 in the [Supplementary Appendix](#) shows that this does not substantially affect the results. I also consider a less likely scenario: that all individuals born in a control state, who are observed to live in Indiana or Ohio in 1920, moved there in response to the law. Assigning these individuals to the control group again affects results little, with the exception of endogamy which loses significance (Panel B of Table D.4). Finally, I estimate my baseline specification by dropping all “movers”, *i.e.* individuals born in Indiana or Ohio who are not observed residing there in 1920. Results are shown in Panel C of Table D.4. Estimated coefficients are nearly identical to the baseline. These checks indicate that migration is not the primary driver of the observed effects of the language ban.<sup>18</sup>

**4.3.6. Inference.** All reported significance levels are based on standard errors clustered at the state-border segment level in the case of the border data set, and at the state level in the case of the enlistments data set. *P*-values from the wild bootstrap procedure suggested by [Cameron \*et al.\* \(2008\)](#) as a solution to the problem of few clusters are reported in brackets throughout the analysis. [Abadie \*et al.\* \(2017\)](#) point out that, while data may be correlated along any number of dimensions (*e.g.* within state, but also within cohort), the appropriate level of clustering is the level at which treatment assignment varies. Because treatment in this case is assigned at the state  $\times$  cohort group level (cohorts born 1903 or later), I present wild bootstrap *p*-values using this clustering level in Table D.5 in the [Supplementary Appendix](#) ( $N = 14$ ). With the exception of the log average GNI of all children, which misses statistical significance at conventional levels, effects remain precisely estimated. I also explore a more conservative clustering at the level of the state, which produces largely comparable results.<sup>19</sup>

Taken together, the results presented in this section suggest that removing a child’s home language from the school need not lead to more assimilation and can, in fact, have the exact opposite effect on ethnic preferences. The purpose of the next section is to shed more light on the channels through which language in the school affects assimilation outcomes later in life.

## 5. MECHANISMS

A reaction to forced assimilation can be accounted for by models of intergenerational identity transmission ([Bisin and Verdier, 2001](#); [Bisin \*et al.\*, 2011](#)) in which ethnic schooling and parental investment in identity are substitutes. I present a simple formalization of such a framework in

18. This exercise cannot be conducted in the volunteers data set, in which the treatment status of individuals is based on their state of birth (and not the county of residence in 1920). Given that all cohorts in the data set are born before the enactment of the language ban (the latest one in 1916), there can be no differential selection on the birthplace. Any pre-ban migration would only bias estimated coefficients towards zero.

19. This approach is recommended by [Bertrand \*et al.\* \(2004\)](#) to deal with serial correlation in the case of difference-in-differences designs where the treatment is time varying and the cross-sectional unit is stable, such as the comparison of states before and after the passage of a law. The current setup is different, in that cross-sectional variation comes from cohorts, which, though correlated, are not as highly serially correlated as the same unit observed in different points in time. According to [Abadie \*et al.\* \(2017\)](#), this level of clustering is almost certainly too conservative.

Section A of the [Supplementary Appendix](#). The main elements of the model are a distinction of minority (in this case, immigrant) members into *mainstream* and *oppositional* types, with the latter defined as those who actively try to maintain their culture and distinguish themselves from the majority. Oppositional types are responsible for the backlash effect. The main intuition of the model is that when the school's function of socializing children to their parents' preferred culture is weakened, parents respond by increasing their own investment at home. These efforts, potentially amplified through peer effect channels, can induce investment that is high enough to result in a reversal of the policy's effects.

This framework delivers three predictions that can be tested empirically. First, it predicts that language restrictions are more likely to succeed in assimilating immigrant children when their parents are mainstream types, themselves relatively more assimilated into majority society. Second, the backlash effect is increasing in the strength of identity of oppositional types. Finally, the backlash effect depends on the relative size of the minority group, with smaller communities more likely to react negatively to the elimination of ethnic schooling.

In this section, I test these predictions. The data are supportive of a model of intergenerational identity transmission driving the observed backlash effect. I also provide suggestive evidence ruling out alternative mechanisms. Specifically, I show that the backlash is more likely to be driven by parents' behaviour rather than by increased investment in enculturation on the part of German churches. Finally, I show that the response is not driven by lower educational achievement among affected cohorts.

### 5.1. *Parents' ethnic background*

Why would we expect an intervention that alters the ethnic character of education to have different effects on different groups of immigrants? According to a model of intergenerational identity transmission, when schooling is a substitute for parental investment in the ethnic preferences of children, a decrease in the ethnic content of education will increase the investment of parents with a strong ethnic identity but have the opposite effect on the investment of more assimilated parents. Common sense and the history of bilingual programs both suggest a similar dynamic. Allowing for the use of a minority language as an aide in early school years can actually help children assimilate, by allowing them to transition smoothly from the language of home and their parents to English. In the extreme case—when German language instruction is no longer an option at school—those parents with a strong preference for socializing their children to German culture will make a greater effort to instill that culture, either at home or through other means (*e.g.* more intense socialization with German-speaking peers, or higher participation in extracurricular activities in German).

Here, I investigate how the effects of language policies differ along one important dimension of heterogeneity in parents' ethnic identity: ethnic intermarriage. Toward this end, I extend my data set to include individuals born to mixed couples (German father and non-German mother).<sup>20</sup> Ethnic identity is expected to be stronger when both parents are German because within-group marriage is the endogenous decision of individuals who care relatively more about their ethnic identity and its transmission to their offspring. Such individuals choose to marry someone from their own ethnic group precisely because doing so increases the likelihood that children will inherit the parents' culture ([Bisin et al., 2004](#)).<sup>21</sup>

20. Summary statistics for this group are provided in [Tables D.6 and D.7](#) in the [Supplementary Appendix](#).

21. Several studies document lower ethnic attachment among the offspring of interethnic marriages ([Alba, 1990](#); [Waters, 1990](#); [Perlmann and Waters, 2007](#)).

TABLE 7  
*Non-German mothers*

Dep. variable:	[1]	[2]	[3]	[4]
	Log average GNI of children	Log GNI of first son	Spouse German	Volunteer
Law $\times$ CSL age	0.913** (0.310) [0.0751]	1.626*** (0.297) [0.0250]	0.0363** (0.0130) [0.0751]	-0.0243** (0.00986) [0.1421]
Law $\times$ CSL age $\times$ Non-German mother	-0.613 (0.415) [0.3744]	-2.167*** (0.366) [0.0170]	-0.0372 (0.0244) [0.2893]	0.0223 (0.0193) [0.2623]
Observations	38,654	27,015	36,953	5,226
R-squared	0.0634	0.0386	0.0773	0.0721

*Notes:* The sample consists of linked males, born 1880–1916 in the U.S. to a German father, living in a border county in 1920 (columns [1]–[3]) or born in any of the seven states in the sample (column [4]). All regressions include (1920 residence or birth) state and birth cohort fixed effects, and controls for the following name string properties: first and last name length and first and last name commonness. Regressions in columns [1]–[3] include county fixed effects. Columns [1]–[2] control for the share of Germans in the state in 1910 and border segment indicators, all interacted with an indicator for the treated cohort. Column [3] controls for the number of first and second generation German women born in the 10 years before (and including) each birth cohort. Column [4] controls for marital status, the number of dependent family members, and enlistment year fixed effects. Standard errors are clustered at the state  $\times$  border segment (Columns [1]–[3]) or state level (Column [4]). *p*-values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 7 extends the baseline analysis, by including in the sample individuals whose father is German but whose mother is not. I interact the coefficient on *Law  $\times$  CSL age* with an indicator for individuals with a non-German mother, in a triple differences specification. The effect of legislation on the GNI is either smaller or opposite in direction for those born to mixed couples compared to those with German parents. This heterogeneity is significant in the case of the log GNI of the first son.

Overall, having one non-German parent makes it less likely for language restrictions to increase the ethnic identity of German-Americans. To the extent that mixed couples have a less pronounced sense of German identity, the finding is compatible with a theoretical mechanism in which the effort of enculturating children is increasing in the initial sense of identity. Particularly in the case of the log GNI of the first son and (marginally) endogamy, estimates suggest an assimilating effect of legislation. Taken together with main estimates in Section 4.2, these results suggest that language laws increase the variance in outcomes within the German group.

What do these findings imply for the average effect of the language ban on the entire group of individuals with a German father? Table D.8 in the Supplementary Appendix estimates the specification in Column [3] of Table 5 in this pooled sample. There is no indication that the policy succeeded in suppressing German identity on average. In fact, for all outcomes the effect is one of backlash (less precisely estimated for endogamy than for names and volunteering rates). Though these averages depend on the specific composition of the group, the findings are instructive from the point of view of the policy maker. Forced monolingualism, at least in this case, fails in promoting integration, and can trigger a strengthening of ethnic identity, which is more pronounced in magnitude for minority group members of homogeneous minority background.

## 5.2. *Strength of identity*

A simple cultural transmission framework also suggests that a reaction to language restrictions should be increasing in the strength of initial ethnic identity of oppositional types. Here, I consider

two proxies of the strength of ethnic identity at the community level: the county-level share of Lutherans and the county-level share of Germans.

**5.2.1. Share of Lutheran church members.** Although most German-Americans in the U.S. at the start of the 20th century were Catholics, Germans constituted the largest ethnic group among Lutheran church members (Wüstenbecker, 2007). The Lutheran religion was also the one most strongly emphasizing conservation of the German language as a medium for transmitting the faith. Lutheran churches could follow this language policy more independently than could German Catholic churches, which were guided not by Germany but rather by the Pope in Rome (Ripley, 1985; Wüstenbecker, 2007). The Catholic Church was multiethnic but dominated by the Irish and Polish, which caused concern among prominent German-Americans that Catholic parishes were losing their German character (Viereck, 1903). German Lutherans were—among all old-church Protestants—the denomination with the highest commitment to parochial schooling (Kraushaar, 1972).

That the share of Lutherans is a good proxy for German identity can also be verified empirically. Figure D.3 in the Supplementary Appendix uses data from the 1910 census to plot the county-level correlation between the Lutheran share and two of the main outcome variables used as measures of German identity throughout the article: the log GNI of first names and endogamy rates among Germans. The data are restricted to border counties only, and variables represent residuals from a regression on the county-level share of Germans, so that the Lutheran share does not simply capture German concentration in the county. In both cases, a higher share of Lutherans is strongly and significantly correlated with more German names and higher rates of endogamy.

To examine how the backlash effect of the law for individuals with German parents depends on the share of Lutheran church members in their county, I employ a triple-differences specification in which the treatment dummy is interacted with the share  $L_{sj}$  of Lutherans in the county in 1906:

$$Y_{isjc} = \alpha + \lambda_c + \theta_s + z_{sj} + \beta_1 T_{sc} + \gamma_1 L_{sj} + \delta G_{sj} + \sum_{c=1}^{37} \gamma_2 L_{sj} \times \lambda_c + \sum_{c=1}^{37} \delta_2 G_{sj} \times \lambda_c + \beta_2 T_{cs} \times L_{sj} + \beta_3 T_{cs} \times G_{sj} + \theta \mathbf{Z}_{isjc} + \varepsilon_{isjc}, \quad (4)$$

where  $j$  denotes counties and  $z_{sj}$  is a county fixed effect. I additionally control for the interaction of the German share in a county,  $G_{sj}$ , with the treatment dummy and with birth cohort fixed effects. This ensures that any effect of the Lutheran share is not mechanically attributable to a higher share of Germans in the community.

The left panel of Figure 6 plots the triple-interaction coefficient against the share of Lutherans for the GNI of the first son for those individuals in the border data set who have two German parents. The magnitude of the reaction is indeed increasing with the share of Lutherans suggesting a stronger backlash in places with a greater sense of Germanness. Table 8 shows that this is true for all three main outcomes.

**5.2.2. German share.** The second proxy of ethnic identity at the community level I employ is the county-level share of Germans. The choice of this measure is theoretically motivated. My simple framework of cultural transmission predicts a stronger backlash among smaller minorities. Because a child is more likely to be assimilated when part of a small minority, parents who care about maintaining their culture (*i.e.* oppositional types) are more incentivized to invest

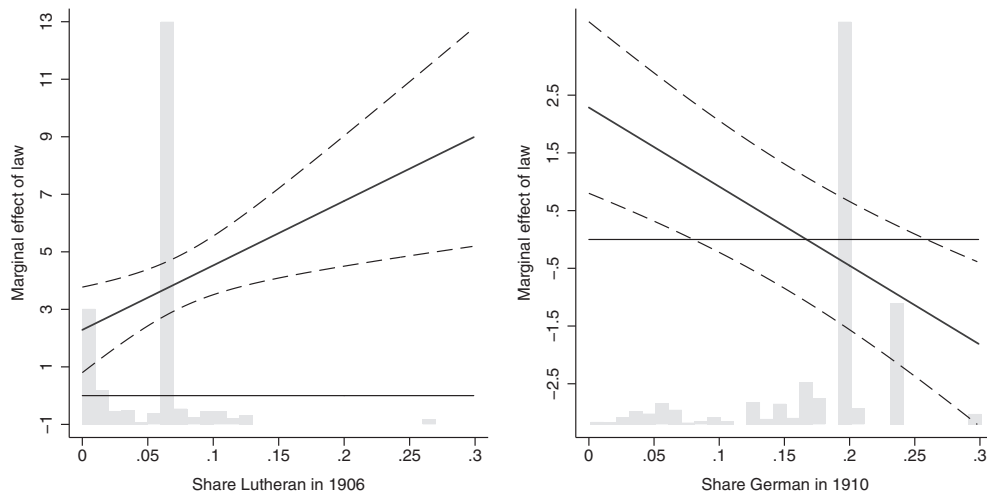


FIGURE 6

## Ethnic and religious composition and effects of the language ban

*Notes:* The left panel of the figure plots coefficient  $\beta_2$  from a regression specified in equation (4) against the county-level share of Lutheran church members in 1906. The right panel plots coefficient  $\beta_3$  of the same equation against the share of first- and second-generation Germans in a county in 1910. The dependent variable is the logarithm of the GNI of the first son. Dashed lines represent 95% confidence intervals. The underlying histograms show how the data are distributed across counties with different shares of Lutheran church members (left panel) and first- and second-generation Germans (right panel). In all cases, the data are restricted to native-born individuals with two German parents. Data on county shares of German ethnic stock are from ICPSR. Data on county shares of Lutheran church members are from the 1906 Census of Religious Bodies.

heavily in that child's identity. Thus smaller minorities have a stronger sense of ethnic identity, particularly among oppositional types.<sup>22,23</sup>

From cross-sectional data, it is possible to verify that the identity of oppositional types is indeed stronger in minority communities of smaller relative size. Table D.9 in the [Supplementary Appendix](#) demonstrates this with data from the 1910 census in the sample of border counties. Among the German population as a whole, ethnic identity, as measured by the log GNI, correlates positively with community size (column [1]). To gauge the correlation among oppositional types only (predicted to be negative by the theory), I restrict the sample to individuals whose first name belongs to the top 10% (column [2]) and top 1% (column [4]) of most distinctive German names. For this subset, identity—as proxied by the log GNI—is stronger where Germans constitute a smaller share of the community. When further restricting the sample to those endogamously married (columns [3] and [5]), this negative correlation becomes even stronger.<sup>24</sup>

To examine the average effect of the German share on the magnitude of the backlash, I once again turn to the specification in equation (4), but now focus on the coefficient  $\beta_3$ , the differential

22. Proposition 3 in Section A of the [Supplementary Appendix](#) demonstrates this formally.

23. This is a somewhat counterintuitive prediction of models of cultural transmission. One could expect that places with a larger share of Germans are more likely to react to forced assimilation, e.g. because they are better organized and hence better able to defend themselves against efforts to suppress their culture. In a model of language assimilation emphasizing a trade-related mechanism, Lazear (1999) finds that smaller minorities are more likely to assimilate.

24. The model additionally predicts that the intergenerational correlation of identity for oppositional types should depend on the ethnic share in a U-shaped fashion (Proposition 5). Figure D.4 in the [Supplementary Appendix](#) shows that this pattern holds when examining the correlation of father's log GNI and the average log GNI of his children (left panel) or the log GNI of his first son (right panel) in 1910 data for the border counties. This lends additional support to the validity of the model.



TABLE 8  
*Effects by share of Lutheran church members and share of Germans in the county*

Dep. variable:	[1]	[2]	[3]
	Log average GNI of children	Log GNI of first son	Spouse German
Law $\times$ CSL age	1.991*** (0.629) [0.000]	1.481** (0.575) [0.0840]	0.0220 (0.0497) [0.664]
Law $\times$ CSL age $\times$ Share German	-13.84*** [0.004]	-8.931** [0.0480]	-0.0801 [0.684]
Law $\times$ CSL age $\times$ Share Lutheran	27.85** [0.0480]	13.73* [0.144]	0.772* [0.108]
Observations	18,459	26,334	24,921
R-squared	0.0248	0.0552	0.0609

*Notes:* The sample consists of linked males, born 1880–1916 in the U.S. to a German father, living in a border county in 1920 (columns [1]–[3]) or born in any of the seven states in the sample (column [4]). All regressions include (1920 residence or birth) state and birth cohort fixed effects, county fixed effects, and controls for the following name string properties: first and last name length and first and last name commonness. *Share German* and *Share Lutheran* are the share of first- and second-generation Germans and of Lutheran church members in the county in 1910 and 1906, respectively. The latter variable is from the 1906 Census of Religious Bodies. Standard errors are clustered at the state  $\times$  border segment level.  $p$ -values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

effect of the treatment by the county-level share of Germans. The right panel of Figure 6 plots the coefficient and 95% confidence intervals against the share of Germans. The dependent variable is the logarithm of the GNI of the first son. The magnitude of the coefficient is decreasing in the German share, indicating a greater reaction in counties where Germans constitute a smaller minority. Results for all outcomes from the border data set are shown in Table 8 (as before, conditional on the county-level share of Lutherans). They suggest that resistance to cultural assimilation is stronger for communities of smaller (relative) size.

### 5.3. Parents and churches

Sections 5.1 and 5.2 provide evidence consistent with the predictions of a model of intergenerational transmission in which the backlash is driven by parents investing in their children's ethnic identity. This investment can be high enough to counteract and reverse the effect of the German ban. Here, I complement this indirect evidence on the role of parental socialization in two ways. First, I examine whether increased investment in German identity in response to the ban came not from parents, but from other institutions of ethnic socialization, such as the church. Second, I look for direct evidence that parental investment in children's enculturation increased after the language ban.

The substitution of a German language curriculum with increased investment in other forms of German enculturation need not only take place at home. Lutheran schools could be responding to language restrictions by modifying their curriculum along other dimensions emphasizing German education, and churches could be increasing their efforts to inculcate German culture through sermons or Sunday schools even in the absence of linguistic means.

While it is hard to find information on parental socialization activities in a period of general repression of the German element in the U.S., the activities of German churches are better documented. I turn to the Statistical Yearbooks of the Lutheran Church-Missouri Synod, one of the largest Lutheran synods in the U.S., with a strong presence in the Midwest and a German

character.<sup>25</sup> These publications include a list of all parochial schools operated by the Synod, along with information on the number of teachers and pupils, as well as other synodical activities. I focus on changes in school- and language-related activities of the Synod before and after the introduction of the German ban. For this purpose, I digitize all relevant information from the 1916 and 1921 Statistical Yearbooks for the seven states in my sample.

The upper panel of Figure 7 plots the percentage change between 1916 and 1921 in a number of school-related operations of the Synod, for treated and control states. The most dramatic change is the increase in the number of pupils enrolled in Sunday school in Indiana and Ohio, relative to control states. One interpretation of this result is that parents in Indiana and Ohio compensate for the ban of German in day schools, by enrolling their children to Sunday schools. It is of course hard to disentangle demand for Sunday schooling among parents from increased supply of Sunday activities by the church. However, no other change is observed in church activities between 1916 and 1921 that would support the supply side interpretation. In fact, the number of Synod-operated schools increased relatively less in states with a German ban, and while the number of teachers showed a less pronounced decrease in those states, this could be reflective of the increase in the number of Sunday teachers (who are counted in the total), and is much smaller than the increase in the number of enrolled Sunday pupils. Table 9 presents difference-in-differences regressions for these outcomes aggregated at the county level, for the border counties in the sample. For comparability, outcomes are standardized using the pre-war standard deviation. Effects on all outcomes are positive, but this is not surprising given that supply (*e.g.* number of schools) and demand (*e.g.* number of pupils) elements of parochial education move together. Once again, the largest increase in treated states in 1921 is observed for the number of Sunday pupils. This is also the only outcome for which the increase is statistically significant. These results are more consistent with a scenario in which the language ban led to an increase in parental demand for Sunday school, with the Lutheran church adjusting its activities to meet it.

Another possibility is that, in response to the language ban, the church invested more in non-educational activities. Unfortunately, most of the information on other activities of the Synod is aggregated at a level higher than the state (that of the district, comprised of multiple states) and also not consistently recorded in the yearbooks before and after WWI. The 1921 yearbook does, however, permit a cross-sectional comparison in the state-level share of monthly church services conducted in German. This share is shown in the lower panel of Figure 7 and is somewhat lower for Indiana and Ohio. This is another indication that the church did not increase its emphasis on German language and identity in response to the language ban.

Instead, the increase in the number of pupils enrolled in Sunday school is more reflective of increased demand for German education on the part of the parents. Anecdotal evidence on the reactions of the parents, albeit scarce, is consistent with this interpretation. For example, there was a documented increase in the number of *Ergänzungsschulen* (*Supplementary Schools*) in the Midwest post-WWI, with the largest of them found in Cincinnati and Toledo, Ohio (Kloss, 1962). These were primarily language schools, both of secular and of religious character, and they were not subject to the language prohibition that applied to elementary education. That their numbers should increase after the war can be interpreted as increased demand for education in German on the part of the parents. Parents also likely increased their efforts to teach German to their children in private. A poem published in 16 October 1925 in the *Deutsch-Amerikanische Buchdrucker Zeitung*, the official newspaper of German-American printers in Indianapolis, illustrates this. In response to criticism levied against Germans for not fighting harder to uphold the teaching of

25. Until WWI, all its publications were in German. Vocal advocates of the conservation of *Deutschtum* considered it a protector of the German language (Viereck, 1903).

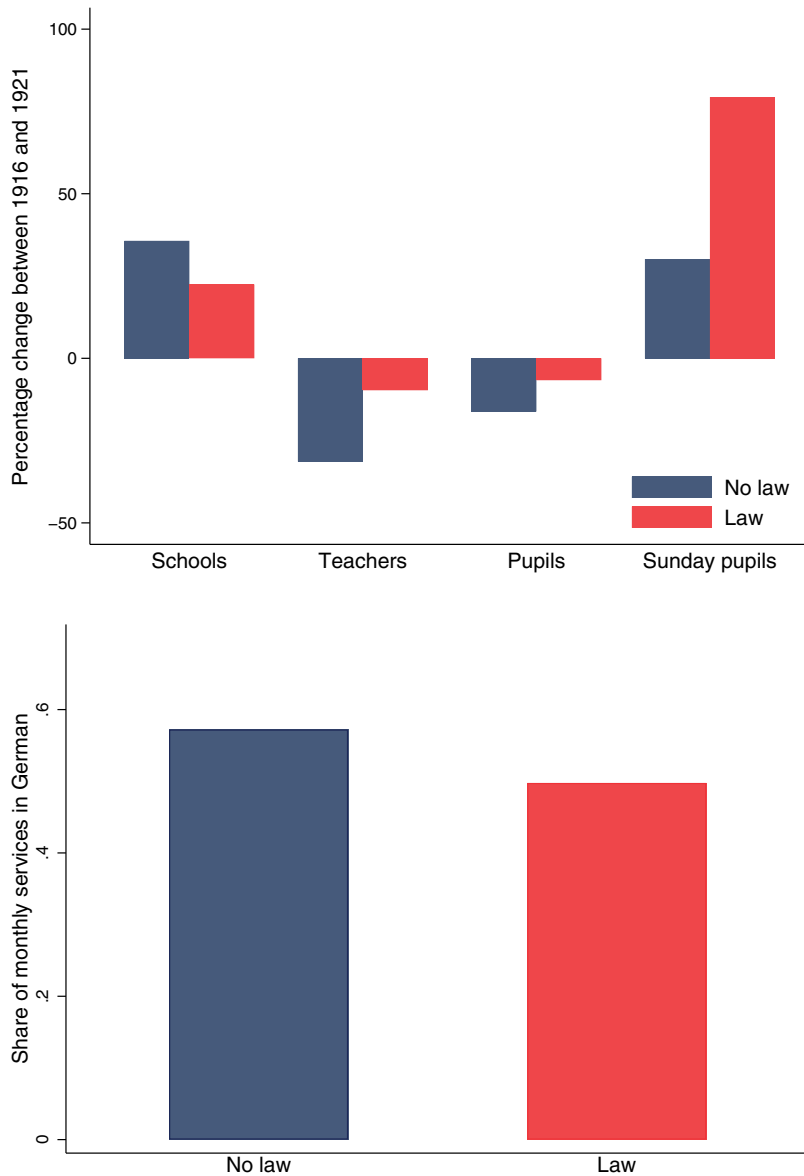


FIGURE 7

Change in the activities of the Lutheran Church–Missouri Synod, 1916–1921

Notes: Data are from the 1916 and 1921 Statistical Yearbooks of the Lutheran Church–Missouri Synod, for the seven states in the sample (Indiana, Ohio, Illinois, Michigan, Kentucky, West Virginia, and Pennsylvania).

their language in public school, a German father responds: “*German ei, you bet, /This he[the son] hears from my mouth,/For that we don’t need any teachers.*”<sup>26</sup>

26. “Dschörmen, ei, you bet/Hört er daheim aus meinem Munde,/Da braache mer kaa Teachers net.”

TABLE 9  
*Change in activities of Lutheran Church–Missouri Synod, 1916–1921*

	[1]	[2]	[3]	[4]
Dep. variable	Schools	Teachers	Pupils	Sunday pupils
Law $\times$ 1921	0.0955 (0.125) [0.3624]	0.155 (0.145) [0.1892]	0.131 (0.123) [0.3674]	0.307 (0.209) [0.0971]
Observations	78	78	78	78
R-squared	0.861	0.873	0.912	0.783

*Notes:* Data are from the 1916 and 1921 Statistical Yearbooks of the Lutheran Church–Missouri Synod, aggregated at the county level for the sample of border counties. Outcomes are standardized using the standard deviation in 1916. Standard errors are clustered at the state  $\times$  border segment level. *p*-values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\**p* < 0.01, \*\**p* < 0.05, \**p* < 0.1.

#### 5.4. *Effects on educational achievement and earnings*

Banning the German language from elementary schools could have had direct effects on the content and quality of education of German-American children.<sup>27</sup> Such effects could in turn impact language proficiency, mobility rates, or cultural adaptability and thereby intermarriage (Bleakley and Chin, 2010; Furtado and Theodoropoulos, 2010; Wozniak, 2010). For many children, especially those born to homogamous German couples, instruction in German at school may contribute to their smooth transition from the language spoken at home to the language of society. In the absence of this auxiliary language, the schooling outcomes of these children might be worse. Conversely, for children of already assimilated families, German instruction might constitute an impediment to their progress in English language courses (Chin *et al.*, 2013).

Column [1] of Table 10 tests these notions, by examining how the German language ban affected the years of schooling completed by individuals in the border data set. The language ban increases schooling for affected cohorts, though the estimated effect is relatively small. This suggests that the observed backlash effect is not due to lower quantity of education and lends support to the claim that observed effects resulted mainly from ethnic preferences and the parents' socialization efforts.<sup>28</sup>

**5.4.1. Backlash costs.** Is the strengthening of ethnic identity in response to language laws, costly for exposed cohorts later in life? Studies on intermarriage (for a review, see Furtado and Trejo 2013)—and on other assimilation decisions of immigrants, such as Americanizing surnames (Biavaschi *et al.*, 2017)—indicate that, notwithstanding the possibility of immigrants' self-selection, assimilation entails a premium in the labour market.<sup>29</sup> Conversely, it is conceivable that strongly adhering to one's own ethnicity implies a cost (Battu and Zenou, 2010). Individuals who marry endogamously lose access to valuable networks outside their ethnic group and thus may be sacrificing mobility by retaining strong ties with their communities.

27. Eriksson (2014) and Ramachandran (2017) demonstrate that mother tongue instruction in primary school has positive effects on years of schooling, literacy, and wages in South Africa and Ethiopia, respectively. In a related study, Dee and Penner (2017) show that "culturally relevant" curricula which include ethnic studies courses increase both school attendance and educational attainment.

28. That the German language ban increases schooling, but decreases integration later in life accords with the findings of Friedman *et al.* (2016) and Croke *et al.* (2016) that more education in an authoritarian context reduces political participation.

29. A related body of work on the adoption of a white racial identity by African Americans in 19th and 20th century America (Mill and Stein, 2016; Nix and Qian, 2015) uncovers a substantial positive effect of "passing" on economic outcomes.

TABLE 10  
Educational and labour market outcomes

Dep. variable	[1]	[2]
	Years of schooling	Log yearly wage income
Law $\times$ CSL age	0.311** (0.129) [0.2372]	-0.106 (0.132) [0.6086]
Observations	25,819	19,727
R-squared	0.0678	0.0197

*Notes:* The sample consists of males, born 1880–1916 in the U.S. to German parents, living in a border county in 1920 and who were linked to the 1940 census. All regressions include residence state in 1920 and birth cohort fixed effects, county fixed effects, indicators for the share of Germans in the state in 1910, and border segment indicators interacted with an indicator for the treated cohort, and controls for the following name string properties: first and last name length and first and last name commonness. When the dependent variable is log yearly wage income, the data set is restricted to salaried workers. Standard errors are clustered at the state  $\times$  border segment level.  $p$ -values from the wild bootstrap (Cameron *et al.*, 2008) are reported in brackets. Significance levels: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Column 2 in Table 10 is only a weak indication that such costs apply in the case of German-Americans. The estimated effect of the German language ban on the log of yearly wage earnings of affected cohorts is large, but not statistically significant: exposure to that law implies an imprecisely estimated 10% reduction in yearly wage income. Given that schooling does not decrease for these cohorts, this negative effect can be plausibly attributed to reduced cultural assimilation.

## 6. DISCUSSION

The findings reported here suggest that restrictions on immigrants' native language can increase ethnic identity. They are consistent with a model of cultural transmission of ethnic identity in which ethnic schooling and parental investment in identity are substitutes. Such a framework can account for a stronger backlash in communities with a greater sense of Germanness, where parental investment in ethnic identity is initially higher. This channel may be complementary to others: localities with a stronger German identity may facilitate parental investment outside the school through German clubs and associations, or may achieve a less strict enforcement of the language ban in the classrooms. The stronger reaction in places where Germans are a smaller share of the total population can also be accounted for in terms of strength of identity. In places where the German minority is smaller, parents put more effort in shaping each child's sense of ethnicity because reliance on peer interaction is not guaranteed to transmit their culture. Such places have a stronger initial sense of ethnic identification and consequently react more to any attempted assimilation. Other theories can produce similar results. Jia and Persson (2017) also find evidence of a negative correlation between the response to external incentives for assimilation and the size of the group. In the context of policies favouring minorities in China, they show that material benefits for changing the identity of one's children have a smaller impact on parents' decisions when the size of the group sharing that identity is small. Building on Bénabou and Tirole (2011b), they suggest that social considerations and intrinsic motivations are more likely to crowd out any external incentives in smaller communities.

A reaction to attempted assimilation is broadly compatible with a number of theories. Applying their seminal framework of identity on education, Akerlof and Kranton (2002) show how schools which promote a single social category or educational ideal can alienate students whose background is too distant from the behaviours that this ideal prescribes. Their model can explain the clash between immigrant students and Americanizing schools of the early 20th

century—interestingly, those less assimilated would be more likely to distance themselves from the behaviours prescribed by the school. In the framework of [Bisin \*et al.\* \(2011\)](#), children are allowed to choose their own identity. When the share of oppositional types in society is reduced because of an assimilation attempt by the majority, the remaining oppositional individuals have an incentive to strengthen their identity and thus to reduce their costs of interacting with people who are different from them. In this model, a language ban would lead to fewer but more intensely oppositional types. In [Bénabou and Tirole \(2011a\)](#), identity is an asset built with investment over time. Increases in the salience of identity or in the uncertainty of one's type, such as might well be sustained by second-generation immigrants discriminated by the majority population, can lead to costly investments in identity if the initial ethnic identification (here the sense of Germanness among the parents' generation) is strong.

## 7. CONCLUSION

Can government policies of forced assimilation backfire? I examine the prohibition of German in U.S. elementary schools and its effects on the assimilation of German children. Using both linked census records and information on WWII volunteers, I show that the policy had a negative effect on assimilation outcomes, particularly for individuals of more homogeneous German background. This effect is larger in areas where there were fewer Germans. This strongly suggests that parents overcompensate, investing in their child's identity all the more as horizontal socialization declines. Effects are larger in areas with more Lutherans suggesting that an ethnic community's initial degree of identity determines the magnitude of its reaction to assimilation efforts.

Can the historical case study of U.S. Germans inform modern-day language and integration policies? The debate about language restrictions is very much alive in immigrant receiving states and countries, such as California and Germany.<sup>30</sup> This suggests that modern day societies face many of the same questions. Furthermore, the finding of a backlash in a well-integrated prosperous immigrant group such as the Germans in the U.S.<sup>31</sup> implies that negative consequences of assimilation policies may be even more likely amongst poor marginalized groups—such as Muslims in Europe.

One of the implications of this article is that policies favouring linguistic and cultural autonomy may actually increase social cohesion—both by facilitating assimilation for the least integrated minority members and by decreasing the variance within the minority group.<sup>32</sup> My findings thus highlight a dimension that is complementary to educational achievement and that should be considered when debating bilingual education and linguistic immersion policies.

## Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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30. In 2006, the Herbert Hoover School (a low-track secondary school in Berlin) implemented a ban on Turkish and other foreign languages on its premises, a policy that earned it the German National Prize and \$94,000 from the National German Foundation. The school's director, Jutta Steinkamp, explained that "this ban [has been introduced] to enable our students to take part in German society through speaking and understanding the language properly" and that "knowing the language is a precondition for successful integration" ([Crutchfield, 2007](#)).

31. Germans had the highest rates of naturalization among the foreign-born ([Ripley, 1985](#)).

32. This evidence from history accords with studies reporting positive effects of contemporary multiculturalist policies on immigrant integration ([Wright and Bloemraad, 2012](#)).



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