# Essays on the Economics of Culture and Identity

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To my father

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# Abstract

This thesis examines how culture — in the sense of identity and preferences — relates to economic decision-making. The first chapter investigates how a prohibition of their native language in school affects the assimilation of German immigrants in the US in the early 20th century. I find that restrictive language policies produce a backlash of ethnic identity, which is particularly pronounced among less assimilated immigrants. In the second chapter I examine the role of history and memory in shaping consumer decisions. I show that the share of German car sales in Greece drops during periods of conflict between the Greek and German governments, and that this drop is larger in areas of the country which suffered from German reprisals during World War II. The third chapter provides a theory on the origin of the preference for work. Combining agroclimatic and attitudinal survey data from Europe, I show that regions in which returns to labor in agriculture have historically been high place more value on work versus leisure today.



# Resumen

En esta tesis se analiza el modo en qué la cultura — en el sentido de identidad y preferencias — está relacionada con la toma de decisiones económicas. El primer capítulo investiga cómo la prohibición de su lengua materna en la escuela afecta la asimilación de los inmigrantes alemanes en los EE.UU. a principios del siglo XX. Los resultados demuestran que las políticas lingüísticas restrictivas producen una reacción contraria al propósito de la misma en cuanto a la identidad étnica, que es particularmente pronunciada entre los inmigrantes menos asimilados. En el segundo capítulo examino como la historia y la memoria histórica afectan las decisiones de consumo. Muestro que la proporción de las ventas de automóviles alemanes en Grecia cae durante los períodos de conflicto entre los gobiernos griego y alemán, y que esta caída es mayor en las zonas del país que sufrieron represalias alemanas durante la Segunda Guerra Mundial. En el tercer capítulo desarrollo una teoría sobre el origen de la preferencia sobre el trabajo. Combinando los datos de encuestas agroclimáticas y de actitud de Europa, muestro que las regiones en las que los retornos del trabajo en agricultura han sido altos históricamente, hoy en día ponen más valor al trabajo con respeto al ocio.

# PREFACE

Until recently, culture — in the sense of preferences, collective beliefs and social norms formed through parental socialization and history — featured very little in the topics of interest of economists. The vagueness of its definition, and the reluctance of the discipline to touch fundamentals of economic models such as preferences, left the study of culture to other social sciences, better equiped to deal with it, such as psychology or anthropology. Owing to Greif (1994), and after him Akerlof and Kranton (2000) and Bisin and Verdier (2000), economists not only started developing ways of thinking about culture, but also managed to show that economic models are a good way of understanding how culture is both shaped by the economic environment and is itself a driver of economic behavior.

Despite the burgeoning literature that ensued on the study of culture, we still have a long way to go in understanding where culture comes from and how it matters for economic outcomes. Empirically identifying the roots of attitudes and norms, or isolating their effects on behavior from other proximate factors is challenging. A number of studies document convincingly the persistence of culture over very long periods of time. But what makes this persistence weaken, and when the effect of history on behavior becomes more or less pronounced is relatively less understood.

The assimilation of immigrants in the host country is one of the prime examples of cultural change, and I exploit such a context in the first chapter of this thesis in order to investigate the movements of one of the defining characteristics of culture: ethnic identity. My precise question is when and to which direction ethnic identity responds to state integration efforts. I focus on the case study of German immigrants to the US and their experiences during the First World War. The anti-German sentiment of the period led many US states to place restrictions on the use of the German language in elementary schools. I compare assimilation outcomes of German-American children exposed to language-banning legislation at school with those of their peers in older cohorts and in states that did not ban the German language, in a difference-in-differences framework. To do this, I construct a new data set of census records linked over consecutive census years and combine this with data on World War II enlistments. I find that restrictive language laws

produce a backlash of German ethnic identity, particularly for individuals coming from homogeneous German families. These German-Americans were more likely to marry within their ethnic group and choose decidedly German names for their offspring. They were also less likely to volunteer in the US Army during World War II. This backlash effect is more pronounced in places with a small German minority and places where the initial sense of German identity, as proxied by the strength of the Lutheran church and the presence of Lutheran parochial schools, was stronger. These results are compatible with a model of cultural transmission of identity, in which parental investment overcompensates for the direct effects of assimilation policies.

In the second chapter, coauthored with Hans-Joachim Voth, I investigate how historical memory affects consumer choices in the context of the Greek debt crisis. Combining an index of political sentiment constructed from over 60,000 newspaper articles with historical maps on German war crimes in Greece during World War II, we find that German car sales drop in response to German-Greek political acrimony, especially in areas with a memory of past German atrocities. We conclude that cultural aversion was a key determinant of purchasing behavior, and that memories of past conflict can affect economic choices in a time-varying fashion. These findings are compatible with behavioral models emphasizing the importance of salience for individual decision-making.

In the third chapter, coauthored with Alain Schlaepfer, I shift my focus from the effects to the origins of culture. We develop a theoretical model of endogenous preference formation to explain how labor intensive production structures lead societies to place more value on work versus leisure. Our hypothesis is that a society's work ethic depends on the role that labor has played in it historically, as an input in agricultural production: societies that have for centuries depended on the cultivation of crops with a high equilibrium labor to land ratio will work longer hours and develop a preference for working hard. To test this theory, we combine agroclimatic and attitudinal survey data from the regions of Europe; we find that individuals in areas suitable for the cultivation of labor-intensive crops report longer desired work hours and a greater importance of work in their lives. We find support for the hypothesis of cultural transmission, by examining the correlation between potential labor intensity in the parents' country of origin and

hours worked by children of European immigrants in the US.

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# Chapter 1

# BACKLASH: THE UNINTENDED EFFECTS OF LANGUAGE PROHIBITION IN US SCHOOLS AFTER WORLD WAR I

## 1.1 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, 2014) assume that policies such as imposing a national language or otherwise repressing minority culture will lead to more homogeneity. At the same time, one strand of literature has shown theoretically that identity may be strengthened in the face of policies aimed at integration (Bisin and Verdier, 2000, 2001; Bisin et al., 2011). What is unclear is when assimilation can work in practice, and what drives the risk of a backlash.

In this paper I examine the long-term effects of a particular assimilation policy: the prohibition of German in US schools after World War I. When the United States 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? To guide the empirical analysis, I construct a simple model of intergenerational transmission of ethnic identity. I derive conditions under which a cultural backlash is more likely. Using a variety of data sources — including linked census records and World War II enlistments — I examine several outcomes for German-Americans affected by language restrictions: (i) their intermarriage rates, (ii) the ethnic distinctiveness of the first names chosen for their offspring, and (iii) their decision to volunteer for the US Army during World War II.

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 (DiD) 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 are 1–8 percentage points more likely to marry endogamously (i.e. within their ethnic group) and 10–12 percentage points less likely to volunteer in 1942. They also choose more distinctively German names for their children, with the estimated effect being equivalent to switching from a name like Chris to a name like Adalbert.

Next, I examine the mechanisms behind this reaction. The estimated backlash becomes weaker (or goes to the opposite direction) for Germans born to mixed couples. This establishes a link between the strength of the parents' ethnic identity and their offsprings' reaction to policies affecting ethnic schooling. This link suggests that the law acts on ethnic preferences through adjustments made by parents to investing in their children's identity. In line with my theoretical framework and models of cultural transmission (e.g., Bisin and Verdier, 2001), the backlash is greater in counties with a smaller share of German population. This result implies 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 is higher also in counties with a greater share of Lutherans, 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 react more strongly to assimilation policies.

In accordance with this dynamic, I also find a greater backlash among those living closer to Lutheran parochial schools; such individuals were likely to sort there owing to their preference for ethnic schooling.

My findings imply that linguistic immersion through the prohibition of German has no clear assimilation effect on average. Instead, and across all outcomes, a language ban leads to a robust increase in the spread between individuals of uniform and mixed German ancestry. Furthermore, I find that the language ban has no significant effect on years of schooling and is thus unlikely to influence assimilation outcomes through its effect on education. I do, however, find evidence that a strengthening of ethnic identity entails a penalty for individuals who become more German. German-Americans with two German parents affected by language laws experienced an earnings penalty in the labor market. Given that schooling outcomes are unaffected, such a drop in earnings is unlikely to be 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., 2013).

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). Furthermore, there is no clear pattern of differences in terms of observable outcomes across states that did and did not introduce language bans *other* than the share of their German population (Lleras-Muney and Shertzer, forthcoming). To deal with potential unobservable confounders, I focus on the state border of four comparable states — Indiana, Ohio, Michigan, and Kentucky — and create a unique linked data set of individuals who lived there at the time legislation was enacted in Ohio and Indiana. Apart from increasing internal validity, this design allows me to observe long-run assimilation outcomes of German-Americans and to examine how those outcomes vary by the ethnic composition of their home town. I can thus identify precise conditions, such as the size and character of the minority group, that lead to a more pronounced identity backlash. Finally, the case

study of German-Americans yields an interesting measure of ethnic identification: volunteering for service in the US Army during World War II. This is a unique historical setup in which immigrants are called upon to take sides between their host country and their country of origin; as a proxy of assimilation, that decision is very little contaminated by economic or other confounding factors.

The paper proceeds as follows. Section 1.2 relates this work to the existing literature. Section 1.3 discusses the historical background of German language schooling and the language restrictions imposed after WWI. Section 1.4 presents a simple model of cultural transmission used to examine the implications of a shock on ethnic schooling for the ethnic identity of children belonging to a minority. Section 1.5 describes my data sources. Section 1.6 is devoted to 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 endogamy rates, ethnic name choices and volunteering in World War II. Section 1.7 shows how this backlash effect weakens among children of mixed couples and assesses how the response to legislation varies by a community's ethnic composition and strength of ethnic identity. Section 1.8 examines whether language restrictions affected schooling outcomes and verifies that language laws had a similar impact in the US as a whole as they did in the restricted set of states I examine. Section 1.9 reviews my findings in the context of recent theory on cultural transmission and identity in economics. Finally, Section 1.10 concludes.

## 1.2 Relation to existing literature

A number of theoretical studies suggest that assimilation policies can lead to a backlash of ethnic or religious identity — but until now, there is no compelling empirical evidence that this is more than a theoretical possibility. 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 behaviors 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 behaviors prescribed by

the school. Bisin et al. (2011) present a mechanism for the persistence of oppositional minorities.[1] 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.

My paper 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.[2] In addition, several papers construct measures of the speed of assimilation by looking at political (Shertzer, 2014) or cultural outcome variables (Aleksynska and Algan, 2010), such as first names (Arai et al., forthcoming) or self-reported national identity (Manning and Roy, 2010). Dávila and Mora (2005), Neeraj et al. (2005), and Gould and Klor (forthcoming) show how discrimination against Muslims in the United States 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.

My research also contributes to the literature on the economics of identity (Akerlof and Kranton, 2000). Identity has been shown to explain a variety of economic outcomes, including risk and intertemporal choice patterns (Benjamin et al., 2010), contributions to public goods (Benjamin et al., 2013), as well as educational decisions (Austen-Smith and Fryer, 2005), but evidence on the determinants of identity formation is generally not causal in nature (Constant et al., 2009; Nekby and Rödin, 2009; Battu and Zenou, 2010; Manning and Roy, 2010; Bisin et al., 2013).

More broadly, this paper relates to a rich literature in history, economics, and sociology that examines the effects of education on national identity. There are many studies documenting how education and 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. (2014) show how a new school curriculum in China had a measurable effect on the political attitudes of students. My study

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

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

focuses more on the medium than on the content of education, but its results suggest that the purpose of integrative educational policies is seldom entirely achieved. The study of Friedman et al. (2011) 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, especially those not belonging to the country's leading tribe.

Relative to the existing literature, I make the following contributions. My paper is the first to provide empirical evidence on the existence of a backlash of identity in response to assimilation policies. Second, I test the relevant mechanism driving this reaction: cultural distinction, or the tendency of parents to invest more in enculturating their children when there are few role models among their children's peers. Finally, I contribute to the literature on identity formation by providing 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).[3]

## 1.3 Historical background

This section outlines the history of the German language in US 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 World War I.

### 1.3.1 Germans in the United States and the German language in schools

Germans were the single largest foreign group that migrated to the post-colonial United States until at least the 1970s. Starting in the 17th century, German immigration increased as a result of the mid–19th-century political upheavals in Europe and peaked in 1890, when economic migrants replaced political refugees

[3]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.

in the arriving immigrant cohorts. Between 1880 and 1920, Germans were always the largest element among the foreign-born in the United States; in 1900, the first and second generation of Germans together constituted little more than 10% of the total US 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 daily half-day instruction in German (Schlossman, 1983; Zimmerman, 2002). Such bilingual programs were favored by German parents and they were also 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 World War I. According to the 1917 Minnesota Educational Census, the state counted 308 parochial schools with a total enrollment of 38,853 pupils; more than two thirds of these schools used both German and English as a medium of instruction (Rippley, 1981).[4] 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 prac-

[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).

tice 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 — into US society. 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."

### 1.3.2 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 harrasment and persecution once the United States 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 honor of the American soldiers who fought in the Second Battle of Marne. Hamburgers became "liberty steaks"[5] 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 featuring in 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

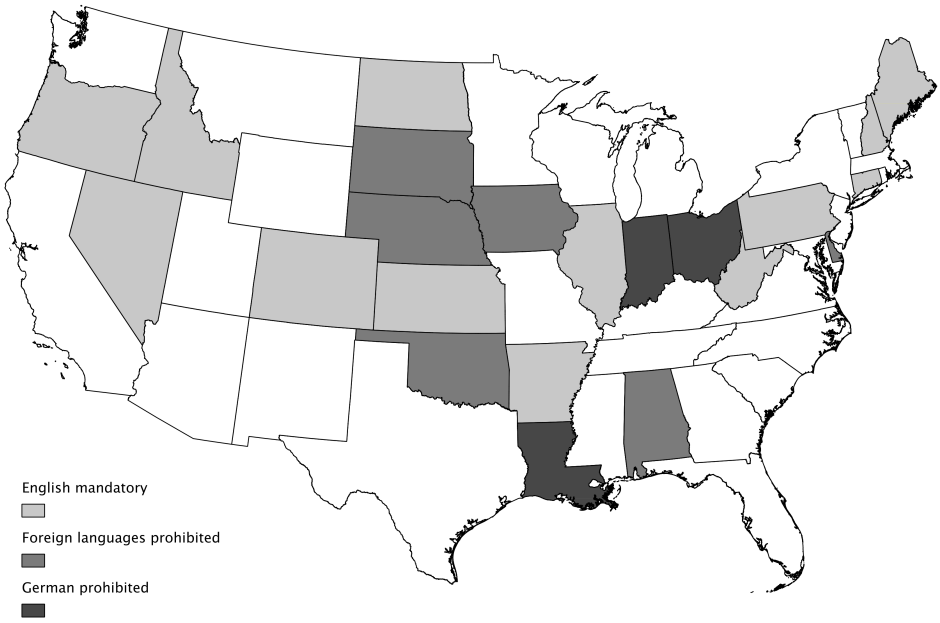
[5] An interesting parallel is the renaming of french fries to "freedom fries", after France opposed the US invasion of Iraq in 2003 (Michaels and Zhi, 2010).

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 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 United States (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).

Figure 1.1: Language laws, 1917–1923




**Notes:** Data from Edwards (1923).

World War I marks a clear break in the pre-existing trends of English language legislation; in the period 1917–1923, there were 21 states that prohibited the use of foreign languages as a medium of instruction or as a separate subject in elementary grades (Knowlton Flanders, 1925). Figure 1.1 depicts these states in terms of the type of law they introduced. Though not always explicitly targeted against German, these laws are generally viewed by historians 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.[6] 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 US Supreme Court repealed the 1919 Nebraska law — and with it

[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.

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).

## 1.4 Conceptual framework

To guide the empirical analysis, this section provides a basic conceptual framework for understanding how language restrictions at school affect the formation of ethnic identity among immigrant children. I construct a simple model of intergenerational transmission of ethnic identity; the model borrows from Bisin and Verdier (2001) and Bisin et al. (2011), and it features both vertical and horizontal socialization. A child's sense of ethnicity is the end product of parental investment and socialization in the school. Using this model, I derive predictions for the effect of a language ban on the younger generation's sense of ethnic identity and for how this effect varies according to the initial strength of parents' ethnic identification.

### 1.4.1 Modeling the intergenerational transmission of ethnic identity

Consider a population of fixed size N that consists of a majority and a minority group. The two groups are differentiated by some external attribute, which is exogenous to the individual. In the context of our specific case study, this attribute is ethnic — in particular, German — origin. Hence we use subscript $G$ to denote the minority group, whose size is $N_G$; we use subscript $NG$ to denote the majority, whose size is $N_{NG}$ ($N = N_{NG}+N_G$). Within the minority group of German ethnics, there are two types of individuals: "mainstream" ($i = m$) and "oppositional" ($i = o$). Mainstream types are assimilated into the majority Anglo-Saxon culture and follow its norms, whereas oppositional types actively try to maintain their German culture and resist assimilation by the mainstream. Although members of the German minority can be either mainstream or oppositional, the majority group is assumed to consist only of mainstream types.

Families are composed of a parent and a child of unspecified gender. Children (marked by superscript $c$) initially inherit the type or trait, $i = m, o$, of their parents (marked by superscript $p$); however, they can switch to a different trait after exposure to interactions with peers, role models, or other cultural partners in society. I assume that such "horizontal" socialization occurs in school, which every child attends. In school, the child interacts with teachers and peers and is paired to a role model, who with probability $q_i$ is of a different type than the focal child's inherited type. If this is the case, then the child switches to the role model's type with probability $1 - \lambda_i^c$. Here $\lambda_i^c$ denotes the intensity of the child's identification with his initial type and is the result of parental investment. Since oppositional parents are more likely to feel strongly about the identity of their children, we assume that parameter values are such that $\lambda_o^c > \lambda_m^c$.

We next explain in more detail the function of the school in our model. After the family, the school is assumed to be the main (and, for simplicity, the only) socialization pool entered by the child. The school's ethnic character — in other words, the importance it places on German education — thus determines how likely it is that the child will become oppositional later in life. Recall that $q_i$ denotes the probability of the child meeting a role model of type different than her parents. For oppositional children, the probability $q_o$ of meeting a mainstream role model is lower in a school that emphasizes the transmission of German ethnicity (as in, e.g., a school that teaches the German language).

Given the socialization mechanism just described, the probability with which a parent of trait $i$ will end up with a child who shares that same trait is given by

$$P_{ii} = 1 - q_i(1 - \lambda_i^c) \tag{1.1}$$

Similarly, the probability that a parent of trait $i$ will end up with a child who instead exhibits trait $j \neq i$ is

$$P_{ij} = q_i(1 - \lambda_i^c) \tag{1.2}$$

Later in life, each child takes an action $a \in \{G, NG\}$. Action $G$ ("German") is accepted as appropriate by the German minority, whereas action $NG$ ("Non-

German") is compatible with the mainstream values and is taken by an assimilated individual. For instance, one could have $G = \{\text{marrying a German ethnic}\}$ versus $NG = \{\text{marrying a native}\}$ or $G = \{\text{giving one's child a German name}\}$ versus $NG = \{\text{giving one's child an Anglo-Saxon name}\}$.

I define preferences such that a mainstream minority type always prefers action $NG$ to action $G$ while an oppositional type always prefers $G$ to $NG$. In particular, utility is given by:

$$U(G) = \begin{cases} a\lambda_i^c + b\overline{\lambda_o} + d & \text{if } i = o \\ 0 & \text{if } i = m \end{cases}$$

$$U(NG) = \begin{cases} 0 & \text{if } i = o \\ F & \text{if } i = m \end{cases}$$

with $a, b, d, F > 0$. For minority oppositional types, utility is increasing in the child's intensity of identity. It is also increasing in a social interaction component, denoted by $\overline{\lambda_o}$, which captures the strength of German identity among the child's peers. The intuition for this latter component is straightforward. The utility that the oppositional child derives from a German action is greater if his immediate environment supports that action. I normalize the utility that an oppositional child would derive from taking action $NG$ to zero. A mainstream child derives utility $U_m^o = F > 0$ from action $G$ and 0 otherwise.[7]

Parents are characterized by what Bisin and Verdier (2001) call *imperfect empathy*, a form of paternalistic altruism whereby parents care about their children's action but evaluate it using their own preference parameters. So in our setup oppositional parents whose intensity of identity is $\lambda_o^p$ will derive utility $U_o^p = a\lambda_o^p + b\overline{\lambda_o} + d$ if their child is oppositional (takes action $G$) or zero utility if their child is main-

[7]Mainstream minority children may also enjoy a psychological benefit when interacting with mainstream peers whose intensity of identity is similar to their own. The converse of this mechanism is the "acting white" phenomenon (Austen-Smith and Fryer, 2005), or the psychological cost sustained by minority individuals who do not conform to the norms of their group. Here, I assume that the psychological benefit to mainstream children is negligible when compared with the direct gain $F$ of assimilation. In other words, the benefit of undertaking the action prescribed by one's type is more psychological in nature for oppositional individuals but more tangible in nature for mainstream individuals.

stream (takes action $NG$). Conversely, mainstream parents gain utility $U_m^p = F$ if their child is mainstream or zero utility if their child is oppositional. Parents can influence their child's choice of action by undertaking a costly socialization investment in her identity. A stronger ethnic identity will reduce the likelihood of the child abandoning the parental trait for a random role model in school and will also increase the child's incentives to take parentally desired actions later in life.

Assuming investment costs are quadratic, the problem of an oppositional parent can be written as follows:

$$\max_{\lambda_o^c} \quad (1 - q_o(1 - \lambda_o^c))(a\lambda_o^p + b\overline{\lambda_o} + d) - \frac{\lambda_o^{c2}}{2}$$

This expression gives the optimal intensity of identity chosen by the parent as

$$\lambda_o^{c*} = q_o(a\lambda_o^p + b\overline{\lambda_o} + d) \tag{1.3}$$

Since $\lambda_o^{c*} = \overline{\lambda_o}$ in equilibrium, we can write

$$\lambda_o^{c*} = \frac{q_o(a\lambda_o^p + d)}{1 - bq_o}$$

Note that $\lambda_o^{c*}$ is increasing in $q_o$ (i.e., in the likelihood of the child meeting a mainstream role model). This relation makes intuitive sense. Parents will invest more in their child's identity when it is threatened — that is, when the child interacts more frequently with role models belonging to a different type.

The model exhibits a unique and stable steady state, in which $\lambda^{ss} = \frac{dq_o}{1-q_o(a+b)}$. To ensure that the problem has an interior solution, I further assume that $1 \geq q_o(a + b + d)$.

An analogous maximization problem for the mainstream parent yields

$$\lambda_m^{c\ *} = q_m F$$

Just as with their oppositional counterparts, the investment of mainstream parents is increasing in the payoff of ending up with a mainstream child and in the probability that the child will meet an oppositional role model at school. For both types of parents, the vertical and horizontal transmission of culture are substitutes

(Bisin and Verdier, 2001).

### 1.4.2 Implications of a language ban in elementary school

A school's ethnic character is determined by the probability $q_i$ that a child meets a role model of different type. In this context, a German language ban at school corresponds to an increase in $q_o$ and, equivalently, a reduction in $q_m$ (i.e., a reduced likelihood that the child meets an oppositional role model). If we denote the share of oppositional types among the minority by $p$ and the share of the minority in society by $s$, then we can think of $q_o$, or the probability of an oppositional child meeting a mainstream role model, as $q_o = (1 - \kappa s) + \kappa s(1 - p)$. Similarly, the probability of a mainstream child meeting an oppositional role model can be written as $q_m = \kappa sp$. Here $\kappa$ represents how likely it is to meet a minority role model at school, so a language law can be thought of as a reduction in $\kappa$.

The law sets in motion two opposing forces. Its immediate effect, acting through horizontal socialization, is a reduction in the share of oppositional children; this effect is mediated by a decrease in the relative importance of oppositional role models at school. The second, indirect effect acts through vertical transmission. Given that socialization in the school and in the family are substitutes, oppositional parents will react to the weakening of the school's ethnic socialization function by increasing their own investment in the child's identity. This increase in parental investment can be high enough to counteract the language law's direct effect. Hence we can state the following proposition

**Proposition 1** (Backlash)**.** *Starting from the steady state, children of oppositional parents are more likely to become oppositional in response to a language-banning policy if the steady state intensity of oppositional identity is high enough.*

*Proof.* For a backlash to occur, we need

$$\frac{dP_{oo}}{dq} = -(1 - \lambda_o^c) + q_o \frac{a\lambda_o^p + d}{(1 - bq_o)^2} > 0 \tag{1.4}$$

Evaluating the above derivative at the steady state, we can rewrite the condition

for a backlash as

$$\lambda^{ss} > \frac{1 - bq_o}{2 - bq_o}$$

□

To gain some intuition on the content of Proposition 1, it is useful to revisit equation (1.3), which describes the optimal investment for oppositional parents. That expression makes clear that the law increases parental investment through two channels. The first one is the direct reaction of oppositional parents to the now diminished role of the school, which formerly served as a substitute for their own investment. Hence parents now invest more, which leads to higher $\lambda_o^c$ (i.e., to a stronger sense of identity) for those children who remain oppositional. In turn, that behavior increases $\overline{\lambda_o}$, the average ethnic identity among oppositional children. Equation (1.3) shows that this dynamic feeds directly into the parental decision inducing parents to make additional investment in cultural identity. If this amplification effect is strong enough, then the share of oppositional children will actually increase as a result of the school language law. Propositions 3 and 4 posit that such a "backlash" result is more likely if the minority community is small and/or if oppositional parents strongly identify with their type to begin with.

Although the *sign* of the average reaction is indeterminate, the next proposition predicts how each type of minority parent reacts.

**Proposition 2.** *The difference between oppositional and mainstream parents — with regard to their respective shares of oppositional children — increases in response to a language-banning policy.*

*Proof.* Using the transition probabilities and the fact that $q_m = 1 - q_o$, we can write this difference as

$$D \equiv P_{oo} - P_{mo} = \lambda_o^c + q_o(\lambda_m^c - \lambda_o^c)$$

Taking the derivative with respect to $q_o$ yields

$$\frac{\partial D}{\partial q_o} = (1 + \frac{1}{1 - bq_o})\lambda_o^c - 2\lambda_o^m > 0$$

where the inequality follows from $\lambda_o^c > \lambda_m^c$ and $1 - bq_o \in (0, 1)$. □

Proposition 2 shows that a language ban always leads to an increase in heterogeneity within the minority group. Both types of minority parents adjust their identity investment in response to the law, with mainstream parents now investing less and oppositional parents more. However, the combination of a stronger identity and the effect of social interactions for oppositional parents ensures that their reaction will always be more pronounced than that of their mainstream counterparts. As a result, the spread in the shares of oppositional children born to the two types of parents will increase.

The following two statements identify conditions under which the language law's backlash effect will be more pronounced.

**Proposition 3.** *A backlash from oppositional types is more likely when the share $s$ of the minority community is small.*

**Proposition 4.** *A backlash from oppositional types is more likely when the initial identity of parents is strong (i.e., when $\lambda_o^p$ is large).*

*Proof.* Recall that the condition under which a backlash occurs is $\lambda^{ss} > \frac{1-bq_o}{2-bq_o}$. Then Proposition 3 follows from the fact that $q_o$ is increasing in $s$, the left-hand side of the previous expression is increasing in $q_o$ and the right-hand side is decreasing in $q_o$. For Proposition 4, recall that $\lambda_o^c$ is increasing in $\lambda_o^p$. A backlash is more likely if the share of the minority is small or if oppositional types have a stronger identity. □

The mechanism of cultural distinction elicits higher investment levels from oppositional parents who belong to a small minority. When the share of the minority group is small, the child is unlikely to meet an oppositional role model at school; in this case, the parents must replace horizontal socialization with their personal effort. The result is both high initial $\lambda_o^c$ and high average identity among oppositional types after the introduction of a language policy. When an oppositional

child's utility from social interactions is high, the initial increase in parental investment spurred by the language ban is amplified considerably. This increases the likelihood of parental compensation outweighing the law's first-order assimilation effect and producing a backlash.

The intuition behind Proposition 4 is similar to that behind Proposition 3. In both cases, initial parental investment is high enough to ensure a high average identity — and thus a high utility benefit from social interactions — for oppositional children. These conditions make for a strong amplification mechanism and a more pronounced reaction of oppositional parents to assimilation policies.

## 1.5 Data

This section describes the data sources I use to evaluate the effects of a language ban on the long-term assimilation outcomes of German-Americans. My analysis is focused 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, respectively Indianapolis and Cincinnati (Schlossman, 1983). During the period in question, their neighboring states (Michigan and Kentucky) neither introduced nor had in place any language laws. I first construct a unique data set of individuals living at the border of these states (and of their neighbors) at the time legislation was enacted and then link this data over time to later census years so as to observe intermarriage outcomes and choices of first names for children. 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 World War II 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.

### 1.5.1 Laws

Data on English-only laws are from Edwards (1923) and Knowlton Flanders (1925). Edwards offers a complete list of state laws passed in the late 19th and early 20th centuries that regulated the language of instruction in elementary schools. Knowlton Flanders classifies every state in each of the years 1903, 1913, and 1923 in terms of (a) whether or not English is the mandatory language of instruction and (b) whether permissive, restrictive or no legislation had been passed concerning foreign languages in elementary schools. In cases of disagreement between the two sources, I consult the original state and school laws while using as manuals Hood (1920) and Ruppenthal (1919), who provide references to all language-related legislation enacted in the United States until 1919.

Both Indiana and Ohio explicitly singled out German as a language to be prohibited in elementary school grades in 1919.[8] 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 the years 1918–1923, 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 average compulsory

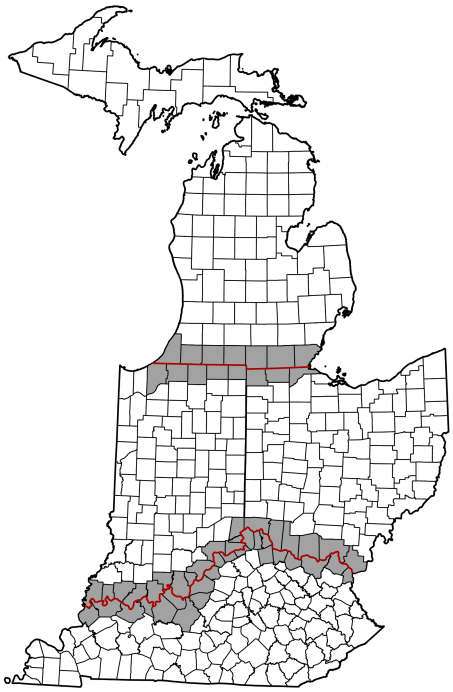
[8]The only other state that explicitly prohibited German in its schools was Louisiana in 1918. This prohibition was part of a legislative package known as Act 114, which was enacted as an expedited war measure and also prohibited the use of German in public and over the phone. It was repealed by the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

schooling age range in the period was 7–14, with 6 the minimum age (in New Mexico) and 18 the maximum age (Idaho, Nevada, Oklahoma, and Utah).

### 1.5.2 Indiana and Ohio borders

I construct a unique data set of all males born 1880–1916 who had parents born in Germany and who lived in a county on either side of the border of Indiana and Ohio with Michigan and Kentucky in 1920 — the census year closest to the introduction of these anti-German laws (see Figure 1.2, where the border counties are shaded). This approach is an improvement over relying on census samples in that it is no longer necessary to use state of birth as a proxy for exposure to the laws. I use the genealogical website Ancestry.com to extract records of individuals fitting the criteria just stipulated from the digitized 1920 census index, and I record the following variables: birthplace, birth year and individuals' exact location in 1920.

Figure 1.2: Counties on the northern and southern borders of Indiana and Ohio



Focusing attention on state borders is meant to increase the comparability of

affected and non affected Germans in dimensions *other* than language restrictions. Table 1.1 shows that counties on either side of the border were comparable in 1910, although this is true mostly for the Michigan border segment. Kentucky counties have a lower concentration both of German-born individuals and of Lutheran Church members. A DiD strategy can deal with these pre-existing differences in the absence of differential trends. Yet the results are not qualitatively affected when Kentucky's border is excluded from the analysis.

Table 1.1: Balancedness of border counties

| | MI Border | | | KY border | | |
|---|---|---|---|---|---|---|
| | Law | No Law | Diff. | Law | No Law | Diff. |
| Population density | 142.02 | 58.56 | −83.46 | 136.16 | 116.01 | −20.15 |
| | (176.45) | (17.22) | (67.28) | (251.38) | (167.50) | (65.90) |
| Share urban | 0.426 | 0.257 | −0.169 | 0.274 | 0.190 | −0.084 |
| | (0.330) | (0.075) | (0.128) | (0.296) | (0.297) | (0.093) |
| Share foreign-born | 0.093 | 0.074 | −0.019 | 0.034 | 0.019 | −0.014 |
| | (0.082) | (0.038) | (0.034) | (0.031) | (0.026) | (0.009)* |
| Share German-born | 0.043 | 0.036 | −0.010 | 0.022 | 0.011 | −0.011 |
| | (0.043) | (0.023) | (0.018) | (0.017) | (0.015) | (0.005)** |
| Share Lutheran | 0.022 | 0.035 | 0.013 | 0.012 | 0.003 | −0.009 |
| | (0.025) | (0.039) | (0.016) | (0.012) | (0.010) | (0.005)** |
| Observations | 8 | 7 | | 19 | 22 | |

**Notes:** Data are from the 1910 county data in Haines and Inter-university Consortium for Political and Social Research (2010) and from the 1906 Census of Religious Bodies.

Using the procedure and criteria just described, I begin with a data set of 34,830 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 Ancestry.com to search for each record in the 1930 and 1940 census. Following the standard census-linking procedures used in the literature (Ferrie, 1996; Abramitzky et al., 2014), I use the phonetic equivalent of first and last name, the birthplace, and the year of birth to locate an individual in a later

census. I discard observations with multiple matched records. Exploiting the features of Ancestry.com's search formula, I increase the band around the year of birth to as many as 2 years for those observations that return zero matches when the exact year of birth is used. When searching for individuals in the 1930 census, I use the father's birthplace as an additional matching characteristic.[9]

The match rate is approximately 50% for the 1930 and 48% for the 1940 census.[10] As is common in record-linking procedures, both demographic characteristics and properties of the name strings can be correlated with the probability of locating an individual in a later census. Table 1.13 (in the Appendix) demonstrates that this is true for my data. US-born individuals, as well as those who are observed to have remained in the state of their birth, are more likely than others to be found. A similar association with the probability of a successful match exists for a number of properties of the first and last names of individuals in the data set. Shorter and less common last names are more likely to be found in a later census. I control for these name string characteristics in all subsequent regressions.

It is noteworthy that the characteristics affecting the probability that an observation is linked do not vary systematically across cohorts or between the two sides of a border; see Figure 1.10 (in the Appendix), which compares match rates for states with and without a law. Despite the match rate's substantial volatility across cohorts, there is no systematic difference in the probability of a successful match that could bias the difference-in-differences analysis.

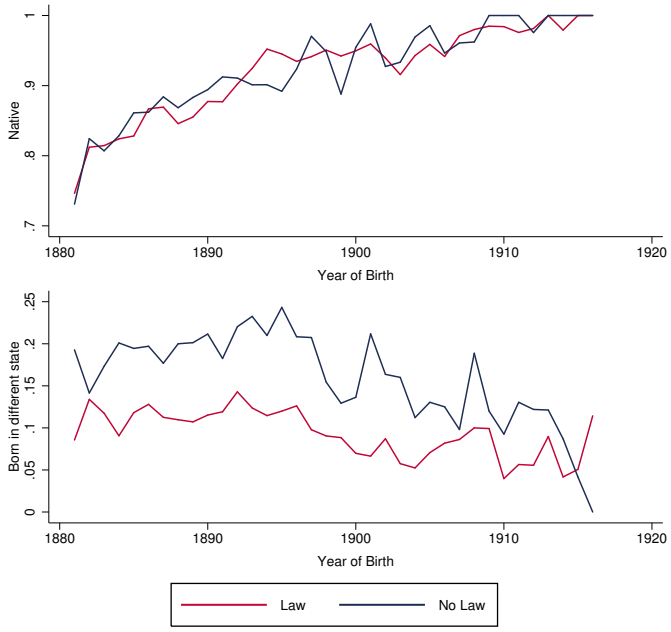
Figure 1.3 examines the balancedness of the linked data set in terms of the few available pre-treatment characteristics. The cross-border share of natives is comparable and does not vary systematically by cohort. The lower panel plots the share of "movers", or individuals born in a state other than the state in which they are observed in 1920. Figure 1.3 shows that Michigan and Kentucky have a higher share of in-migration than do Indiana and Ohio. Although this phenomenon could be a worrisome indication of sorting across the border, in fact the trends move in

[9]For the 1940 census, Ancestry.com does not provide a digitized entry for parents' birthplace. Recall that, for the 1940 IPUMS, only a random 5% of the sample were asked about parental birthplace.

[10]This is roughly comparable to the match rates of previous work linking individuals between censuses. Parman (2011) reports a match rate close to 50%. Long and Ferrie (2013) and Collins and Wanamaker (2014) report lower match rates of about 20%, but they link records across samples and not across the universe of censuses.

parallel for the two groups of states over time; hence out-migration from states with a language law is not higher for the younger cohorts.

Figure 1.3: Balancedness of border data set




**Notes:** The figure compares characteristics of the linked border data set across birth cohorts and across states with and without a language ban. The data consist of males, born 1880–1916 to German parents, who lived in a border county in 1920 and could be linked to the 1930 or 1940 census. The upper panel plots the share of native-born and the lower panel plots the share of "movers" (i.e., people who were born in a different state than the one in which they lived in 1920).

Once an observation is matched across to a later census, I record all variables digitized by Ancestry.com in the respective census year. These variables are listed in Table 1.2, which provides summary statistics. For individuals who are married in 1930, I use the spouse's birthplace and the birthplace of her parents to examine intermarriage. I can examine this outcome only for 1930 because in the 1940 census the parents' birthplace was recorded for only 5% of the universe and never for more than one member in a given household. Figure 1.4 shows the locations in 1920 of all individuals successfully linked in either 1930 or 1940.

Table 1.2: Summary statistics: Border data set

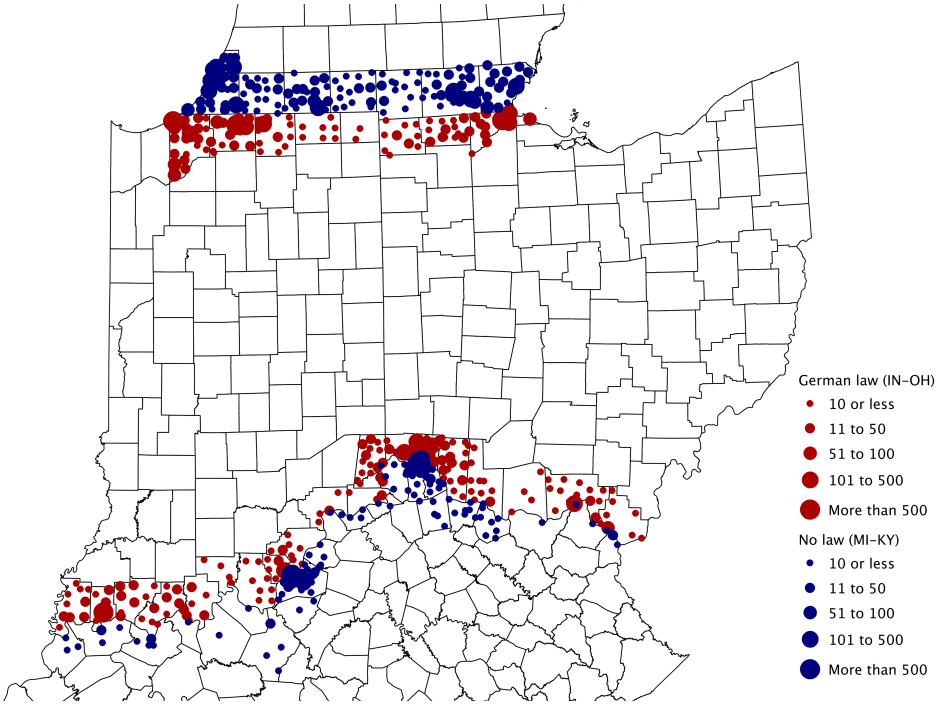
| | Found in 1930 | | | Found in 1940 | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | *N* | Mean | S.D. | *N* |
| Married | 0.727 | 0.445 | 17208 | 0.817 | 0.387 | 16557 |
| Spouse of German ancestry | 0.399 | 0.490 | 11842 | – | – | – |
| Number of children | 2.242 | 1.559 | 4801 | 2.629 | 1.711 | 9398 |
| Log average GNI of children | 2.467 | 2.264 | 4665 | 2.764 | 1.902 | 9303 |
| Log GNI of first son | 2.433 | 2.528 | 3239 | 2.482 | 2.603 | 6926 |
| Lives in same state as 1920 | 0.842 | 0.364 | 17208 | 0.783 | 0.412 | 16557 |
| Lives in same county as 1920 | 0.743 | 0.437 | 17208 | 0.598 | 0.490 | 16557 |
| Years of education | – | – | – | 8.449 | 2.871 | 16338 |
| Yearly salary earnings | – | – | – | 6.567 | 2.537 | 11443 |

**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 (IN) and Ohio (OH) with Michigan (MI) or Kentucky (KY) and who were linked to the census of 1930 (left panel) or 1940 (right panel). See Section 1.5.2 for details on construction of the GNI variables.

**Intermarriage.** One key outcome of interest is intermarriage. It has been characterized as "the final stage of assimilation" (Gordon, 1964) and 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 immigrans 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 US 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 US patterns of religious endogamy are in line with this predic-

Figure 1.4: 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, living in a border county in 1920 and who could be linked to the 1930 or 1940 census.

tion. 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 intermarriage rates. That might happen because, in the first place, children would no longer be indoctrinated "with the German language, customs, and prejudices of the Fatherland ... against the social and religious customs of the American communities in which they claim citizenship."[11] 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,

[11] "The German language school question", *The Outlook* (26 February 1919).

the market for marriage partners would contain more non-ethnic members and thus would increase the likelihood of intermarriage.

The earlier US 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 the spouse of a native person only in 1930. In this census year, younger cohorts are observed at an age when they have likely not yet completed their marriage spells (ages 18–27). Comparison of these cohorts between states with and without a language law should still yield unbiased estimates, but they are not likely to be representative of the general population of German-Americans.[12]

**First names.** I use the names that individuals in my sample choose for their children as an additional 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., 2013). As such, the choice of first names for their offspring is indicative of parental tastes and, for immigrants, of assimilation into the host society. In particular, if cohorts affected by an anti-German 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.[13]

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 ethnic origin. This *German name index* (GNI), which captures how much more frequent a name is among the population of German origin compared with the rest of the population, is constructed as follows

$$\text{GNI}_{\text{name},s} = \frac{\Pr(\text{name}|\text{German}_s)}{\Pr(\text{name}|\text{German}_s) + \Pr(\text{name}|\text{non-German}_s)} \times 100$$

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

[13]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 labor 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.

A name found only among German ethnics would have index value 100, whereas a name given to no individuals of German origin would have index value 0. To compute this index, I use information from the 1930 5% IPUMS sample (Ruggles et al., 2010) and define Pr(German) as the share of foreign-born individuals in the census that were born in Germany. I compute this index separately for men and women and drop from the analysis all names that appear fewer than 10 times in the data; this procedure ensures that index values are not driven by rare names.

Table 1.3: Most and least German-sounding names in the 1930 census

| Highest-scoring | | | | Lowest-scoring | | | |
|---|---|---|---|---|---|---|---|
| Name | Total | Germans | GNI | Name | Total | Germans | GNI |
| Hans | 1272 | 324 | 25.47 | Clyde | 7350 | 0 | 0 |
| August | 5772 | 1260 | 21.83 | Russell | 6045 | 0 | 0 |
| Gustav | 1270 | 231 | 18.19 | Melvin | 5682 | 0 | 0 |
| Karl | 1538 | 268 | 17.42 | Leroy | 5183 | 0 | 0 |
| Otto | 5685 | 959 | 16.87 | Warren | 5071 | 0 | 0 |
| Christian | 1214 | 179 | 14.74 | Marvin | 4585 | 0 | 0 |
| Hermann | 11423 | 1398 | 12.24 | Jim | 4226 | 0 | 0 |
| Emil | 4256 | 515 | 12.10 | Glenn | 3893 | 0 | 0 |
| Adolph | 3225 | 385 | 11.94 | Leslie | 3795 | 0 | 0 |
| Conrad | 1341 | 150 | 11.18 | Wayne | 3648 | 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 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 1.5.2 for details on construction of the GNI.

Table 1.3 provides an overview of what this index captures in the 1930 5% 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 Hermann and Christian, are also on the list of most popular names among German immigrants. The right panel of Table 1.3 lists the 10 most popular names with a zero 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.[14]

In my data, the GNI is always highly correlated with gender and birth order. Males have a higher GNI than females, and the first-born son almost always has the highest value in the GNI among his siblings.[15] 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.

### 1.5.3 World War II enlistment records

Data on men who enlisted in the US Army during World War II 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 of 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 a random sample of individuals born in Indiana, Ohio, Michigan, or Kentucky during the period 1880–1916. As with the IPUMS samples, I match the individuals to legislation based on their state of birth and thus limit the analysis to second-generation Germans. 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

[14]The GNI performs well in predicting an individual's German ancestry among the second generation of Germans in the 1930 5% IPUMS sample; see Figure 1.11.

[15]Figure 1.12 shows that the GNI's density for girls is to the left of that for boys. Figure 1.13 reveals that birth order is highly predictive of the index value, with older children having a more distinctive German name. Male names continue to be more traditional than female ones in modern-day Germany (Gerhards, 2005).

Section 1.5.2, that links enlistees to the 1930 census and determines their ethnic origin, via Ancestry.com's search engine. 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 usually recorded in the 1940 census).

**Volunteers.** Japan attacked Pearl Harbor in early December 1941. Only days later, Nazi Germany declared war to the United States. Following their country's entry to World War II, 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 their country of origin, 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 United States who volunteered for service. These individuals can be identified by their serial numbers, which belong to the 11 through 19 million series.[16] However, it was possible for a drafted 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. To reduce measurement error, I restrict my attention to men enlisted in 1942 — the year immediately following US entry into the war and during which enlisted men were most likely to be volunteers and not draftees.

Figure 1.14 plots the match rate between enlistment records and the 1930 census for the entire sample of men enlisted in 1942 and for volunteers.[17] Volatility in the match rate is high, especially for older cohorts, but there are no systematic differences in the linking probabilities across states and cohorts. Table 1.4 gives summary statistics for the linked sample.

[16] Army Regulation no. 615-30, 1942.

[17] The match rate is approximately 14%. Parman (2015) uses identical criteria to match a sample of WWII enlistees to the 1930 census and reports a match rate of about 8%, after an additional manual inspection of the matched sample.

Table 1.4: Summary statistics: WWII enlistments

| | All | | | German parents | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | $N$ | Mean | S.D. | $N$ |
| Age | 31.07 | 5.25 | 41519 | 34.344 | 5.786 | 485 |
| Married | 0.268 | 0.443 | 41502 | 0.204 | 0.403 | 485 |
| With dependents | 0.144 | 0.351 | 41502 | 0.153 | 0.360 | 485 |
| Volunteer | 0.135 | 0.341 | 41519 | 0.062 | 0.241 | 485 |
| High school graduate | 0.414 | 0.493 | 41519 | 0.309 | 0.463 | 485 |
| College graduate | 0.060 | 0.237 | 41519 | 0.037 | 0.189 | 485 |

**Notes:** The table reports summary statistics for a random sample of males who enlisted in the US Army in 1942 and were linked to the 1930 census. The sample comprises of cohorts born 1880–1916 in Indiana, Ohio, Michigan, and Kentucky. 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.

## 1.6 Main results

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:

$$Y_{iscj} = \alpha + \beta T_{csj} + \lambda_c + \theta_s + \delta \mathbf{Z}_{iscj} + d_{sj} + \varepsilon_{isc} \tag{1.5}$$

where $T_{csj}$ is an indicator for individuals living in a state with a law and who were within the age range for compulsory schooling at the time that law was in place. The terms $\lambda_c$ and $\theta_s$ signify cohort and state of birth/residence fixed effects. $d_{sj}$ is a fixed effect for the border segment (Michigan or Kentucky), and $\mathbf{Z}_{iscj}$ is a vector of name string properties that affect the probability of a record being matched in a later census. The coefficient of interest is $\beta$: the estimated average effect of legislation on exposed cohorts. I cluster standard errors at the level of the state of residence in 1920. As explained in Bertrand et al. (2004), this clustering method

allows for arbitrary patterns of autocorrelation within states across time but yields consistent estimates of the standard errors only with a sufficiently large number of clusters. To correct for the small number of clusters ($n = 4$) in my data, I always report $p$-values computed using the wild-$t$ cluster bootstrap method of Cameron et al. (2008).[18]

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 the passage of language laws. The decision of states to adopt restrictive legislation was not entirely random; it was driven mainly by the share of the German element in the state's population.[19] To control for the potentially time-varying effect of this factor on outcomes such as endogamy rates, I always report specifications that interact the share of first- and second-generation Germans in a state in 1910 with cohort fixed effects.

$$\begin{aligned} Y_{iscj} = \alpha + \beta T_{csj} + \lambda_c + \theta_s + \delta \mathbf{Z}_{iscj} \\ + d_{sj} + \sum_{c=1}^{37} \gamma_c \times \text{German share}_s \times \lambda_c + \varepsilon_{iscj} \end{aligned} \tag{1.6}$$

It is worth remarking that there should be significant measurement error in my estimates because it is not known 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 the language of instruction will bias all estimates toward zero; yet it should not generate a systematic bias in the estimated coefficient of the difference in differences, which will be an average treatment effect on the entire population of Germans in relevant cohorts (including non compliers).

**Threats to identification.** The main DiD identifying assumption will be

[18] The significance of estimated coefficients in the entire analysis that follows is affected little when clustering at the state-cohort level or when adjusting for two-way clustering within state and within cohort (Cameron et al., 2011). Results available upon request.

[19] This can be verified by a mean comparison of the German share across states and also by a visual inspection of the pattern of legislation across the United States. Most of these laws were passed in the midwest and Great Plains states, the areas of highest German concentration.

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 three 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 1904. In any case, 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.

A second 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. I would then be identifying the effect of legislation on the selected group of non movers, and it could be biased in any direction — most likely in one of higher assimilation, since these individuals would be characterized by a weaker ethnic identity to begin with. Since I have no way of knowing where individuals lived 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 1.3. Although the share of these "movers" is indeed higher in Michigan and Kentucky, the trends across states with and without a law are parallel and

indicate no break around the treated cohorts. For my research design, this is an encouraging indication that parents did not relocate in response to language legislation.

A third and related concern is that of spillovers across the border. In particular, individuals living close to a state border — and who want to attend a German school — could always do so on the other side of the border, provided a school is close enough to their town. Therefore, restricting attention to border counties makes it easier to compare the treatment and control groups but also increases the possibility of measurement error caused by non compliers. The effect of spillovers would be to bias any estimated coefficients downward. In a robustness check, I will assess empirically the relevance of this concern by excluding from the data set all individuals who live very close to the border.

### 1.6.1 Intermarriage and first names

Panel A of Table 1.5 examines the average effect of legislation on the linked data set of males with German parents. Column [1] reports the estimated coefficient derived from a regression of equation (1.5)'s form. The estimate is negative, which suggests a decline in endogamy for treated cohorts. Column [2] inserts state controls measured pre-legislation in 1910 interacted with birth cohort dummies; hence this regression accounts for the effects of time-varying factors correlated with the law. These controls are, for year 1910, the share of first- and second- generation Germans and the sex ratio (computed as the ratio of males to females) among Germans in the state.[20] The sex ratio is an important determinant of intermarriage decisions (Angrist, 2002). During the period under study, the United States placed restrictions on immigration that could have distorted the ethnic composition and sex ratios across states.[21] Because immigration quotas were set on the basis of pre-existing immigrant shares, the included interactions partly control for this confounder by addressing the time-varying effect of population shares and sex

[20] I compute this using the 1% IPUMS sample for 1910.

[21] The United States imposed a literacy test in 1917, seeking thereby to reduce immigrant flows from lower-income countries — in particular, from Southern Europe. In 1924, immigration quotas severely restricted immigrant inflows and distorted pre-existing immigrant ratios by favoring Northern European countries over Southern European and Asian ones (Goldin, 1993).

ratios on intermarriage rates. When these controls are included, the estimated coefficient decreases nearly by half and becomes insignificant. Columns [3] and [4] introduce, respectively, county fixed effects and linear state-specific trends. This inclusion reverses the sign of the coefficient; in particular, exposure to a ban of German in school increases the likelihood of endogamous marriage by 1.1 to 1.2 percentage points.

So far, regressions with endogamy on the left-hand side suggest a weakening of ethnic identity and choices of first names point in the same direction. Panel B of the table reports results from using the logarithm of the average German name index of all children as a dependent variable. The average effect is positive and precisely estimated in almost all specifications, with the magnitude of the coefficient remaining fairly stable across specifications. Panel C examines the log of GNI of the first son; the results mirror those for the average GNI but with higher precision and larger estimated magnitudes. These results suggest a backlash effect resulting from exposure to a German ban for individuals with German parents. The magnitude of the coefficient for log(GNI) is meaningful: it implies that exposure to a language law leads fathers to switch from an Anglo-Saxon name like Garrett to a neutral name like Walter.[22] It also implies switching from an ambivalent name like Chris to a clearly German name like Adalbert.

To address spillover effects, column [5] excludes from the sample all individuals living within 5 km of the border. If there are non-compliers, then their exclusion should increase the magnitude of the estimated coefficients. This is indeed the case for all outcomes. For this subset of the data, endogamy increases by almost 8 p.p. for treated cohorts. Column [6] includes an additional robustness check. As revealed by Table 1.1, Kentucky differs significantly from Indiana and Ohio along dimensions such as the share of the German population. When restricting the data to the border of Indiana and Ohio with Michigan — which are comparable across all dimensions, including the share of Germans — the estimated effects remain strongly positive and become larger for most outcomes.

[22]Depending on the pronunciation, Walter can be an Anglo-Saxon or German name. In 1920, it was among the 10 most popular names for boys in Germany.

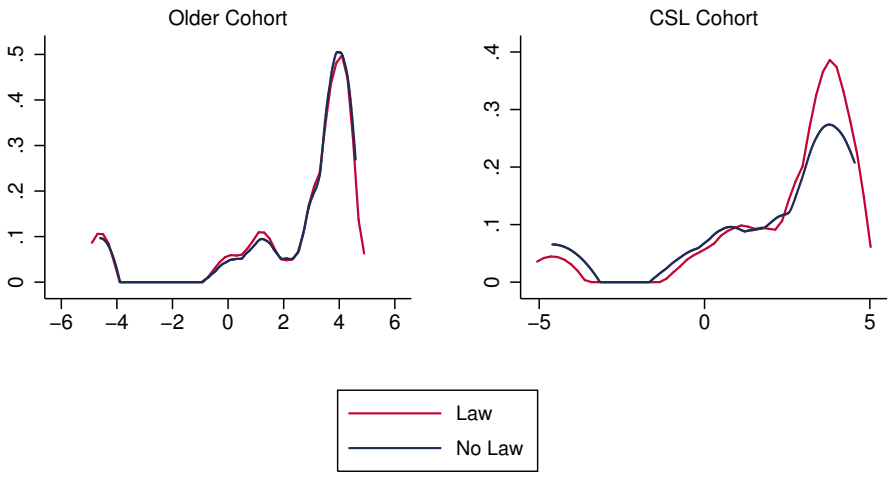
Table 1.5: Baseline results: Border data set

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Panel A | Dep. Variable: Spouse German | | | | | |
| Law × CSL age | −0.0285* | −0.0135 | 0.0108** | 0.0119** | 0.0781*** | 0.0165** |
| | (0.0117) | (0.00607) | (0.00321) | (0.00326) | (0.0101) | (0.00374) |
| *p*-value | 0.508 | 0.152 | 0.009 | 0.000 | 0.000 | 0.000 |
| Observations | 11835 | 11835 | 11835 | 11835 | 5601 | 5007 |
| R-squared | 0.0355 | 0.0399 | 0.0565 | 0.0565 | 0.0717 | 0.0676 |
| Panel B | Dep. Variable: Log average GNI of children | | | | | |
| Law × CSL age | 0.291 | 0.403* | 0.465* | 0.313** | 0.695** | 1.058*** |
| | (0.133) | (0.160) | (0.162) | (0.0901) | (0.151) | (0.00382) |
| *p*-value | 0.560 | 0.444 | 0.089 | 0.004 | 0.009 | 0.000 |
| Observations | 13968 | 13968 | 13968 | 13968 | 6815 | 6085 |
| R-squared | 0.0298 | 0.0353 | 0.0408 | 0.0412 | 0.0435 | 0.0341 |
| Panel C | Dep. Variable: Log GNI of first son | | | | | |
| Law × CSL age | 0.629*** | 0.709** | 0.751** | 0.384*** | 1.072** | 1.248*** |
| | (0.0433) | (0.170) | (0.171) | (0.0623) | (0.213) | (0.0661) |
| *p*-value | 0.000 | 0.009 | 0.009 | 0.000 | 0.000 | 0.000 |
| Observations | 10165 | 10165 | 10165 | 10165 | 4964 | 4406 |
| R-squared | 0.0249 | 0.0301 | 0.0398 | 0.0403 | 0.0365 | 0.0200 |
| Controls | Y | Y | Y | Y | Y | Y |
| State Controls × Cohort FE | N | Y | Y | Y | Y | Y |
| County FE | N | N | Y | Y | Y | Y |
| State trends | N | N | N | Y | N | N |
| Far from border | N | N | N | N | Y | N |
| Michigan border | N | N | N | N | N | Y |

**Notes:** In Panel A, the sample consists of males, born 1880–1916 to German parents and living in a border county in 1920 and who were linked to the 1930 census. Regressions in Panels B and C are estimated in the pooled dataset of individuals linked in 1930 and 1940. All regressions include residence state in 1920 and birth cohort fixed effects, a border segment indicator and controls for the following name string properties: first and last name length and first and last name commonness. Regressions in Panels B and C include a census year indicator. State controls interacted with birth cohort dummies include the share of the German stock and the sex ratio among Germans in the state in 1910 in Panel A, and a border segment indicator in Panels B and C. Column [5] excludes individuals living within 5 km of the state border in 1920. Column [6] restricts the dataset to the northern border of Indiana and Ohio with Michigan. Standard errors are clustered at the state level. *P*-values reported are calculated using wild-cluster bootstrap at the state level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

Figure 1.5 illustrates these results graphically, by plotting the density function of the log GNI of the first son, for treatment and control cohorts. While, for older cohorts, the GNI distribution is practically identical between states with and without a law, the younger cohort experiences a marked shift in the density to the right.

Figure 1.5: Densities of log GNI of first son by cohort




**Notes:** The figure illustrates, for the linked border dataset, 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.

This analysis suggests that the removal of German from elementary schools led to a backlash, a significantly *lower* assimilation along all dimensions for children of German couples. However, both intermarriage and the choice of ethnic names — an outcome determined, to some extent, by homogamy — are general equilibrium outcomes and thus subject to identification concerns, such as differential discrimination against Germans. To address these concerns, I turn my attention to WWII volunteering; this is an informative proxy of ethnic identity that captures a clear

individual decision.

### 1.6.2 WWII Volunteers

I examine volunteering decisions in the context of the four states of Indiana, Ohio, Michigan, and Kentucky by using a sample of the digitized WWII Army Enlistment Records that is linked to the 1930 census (where I can identify the ethnic background of enlisted men). Columns [1]–[3] of Table 1.6 report interaction coefficients from a regression of (1.5)'s form, using state of birth (rather than state of residence) to match individuals up with legislation. The sample is restricted to individuals with German-born parents. The estimated coefficient suggests that exposure to language laws decreases the likelihood of volunteering by 10 percentage points, a large effect that corresponds to nearly half of a standard deviation of the share of volunteers in the entire sample. Column [2] incorporates two additional control variables: one indicator for married individuals and another for dependent family members. Each of these factors reduces the probability of volunteering in the US Army, but their inclusion has little effect on the magnitude of my estimated coefficient. Finally, column [3] introduces an interaction term for the share of Germans in the state in 1910 and birth cohort fixed effects; the result is an increase in the negative effect of laws on volunteering rates to 12 p.p. but the coefficient remains significant at the 5% level.

**Triple differences.** The WWII enlistment data set allows me to compare the behavior of German men with that of the general population in the four states examined here. The removal of German from school curricula should affect German children who were formerly taught in this language but should not affect other immigrants or the native population. I can explicitly test this hypothesis in my sample of men who volunteered in 1942, by comparing the difference in volunteering rates of 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

$$Y_{isc} = \alpha + \lambda_c + \theta_s + \beta_1 T_{cs} + \gamma_1 G_{isc} + \sum_{c=1}^{37} \gamma_{2c} G_{isc} \times \lambda_c + \sum_{s=1}^{4} \gamma_{3c} G_{isc} \times \theta_s + \beta_2 T_{cs} \times G_{isc} + \delta \mathbf{Z}_{isc} + \varepsilon_{isc} \quad (1.7)$$

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. Furthermore, for the laws to be relevant to Germans only, we now expect that the coefficient $\beta_1$ (which captures the average effect of laws on school-age cohorts of non-Germans) will be zero.

Columns [4]–[6] in Table 1.6 report estimates from this specification. Having German parents is associated with a lower average probability of volunteering in 1942, an intuitive result given that the war was against Germany. It is important to note that the effect of legislation is close to zero for potentially treated cohorts of non-German origin, yet for Germans it remains both precisely estimated and negative.

Taken together, the results presented here verify Proposition 1. 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.

Table 1.6: Baseline results: WWII enlistments

| Dep. variable: | Volunteered in 1942 | | | | | |
|---|---|---|---|---|---|---|
| | Difference-in-differences | | | Triple differences | | |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Law × CSL age | −0.104** | −0.104** | −0.125** | −0.00118 | −0.00582 | −0.00370 |
| | (0.0286) | (0.0293) | (0.0219) | (0.0276) | (0.0262) | (0.0289) |
| German parents | | | | −0.0242 | −0.142* | −0.143 |
| | | | | (0.0456) | (0.0600) | (0.0610) |
| Law × CSL Age × German parents | | | | −0.113** | −0.107** | -0.109** |
| | | | | (0.0313) | (0.0261) | (0.0261) |
| $p$-value | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 |
| Observations | 485 | 485 | 485 | 41519 | 41502 | 41502 |
| R-squared | 0.0963 | 0.101 | 0.130 | 0.0177 | 0.0387 | 0.0390 |
| Controls | N | Y | Y | N | Y | Y |
| Share German in state in 1910 × Cohort FE | N | N | Y | N | N | Y |

**Notes:** The sample consists of males, born 1880–1916 in Indiana, Ohio, Michigan, or Kentucky, who enlisted in the US Army in 1942 and who were linked to the 1930 census. Columns [1]–[3] restrict the sample to individuals with German 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. Columns [4]-[6] include interactions of the indicator for German parents with state-of-birth and birth cohort dummies. Columns [2]–[3] and [5]–[6] control for marital status and the number of dependent family members. Standard errors are clustered at the state of birth level. $P$-values reported are calculated using wild-cluster bootstrap at the state of birth level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

## 1.7 Mechanisms

Here, I first test whether language restrictions are more likely to succeed in assimilating immigrants when immigrants are themselves relatively more assimilated into mainstream society. Then I provide suggestive evidence that the theoretical mechanism of parental reaction to language restrictions — as posited in Section 1.4 —

is valid; I do this by showing that the backlash effect depends on the composition of the community in the way that the model proposes.

### 1.7.1 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? The model suggests a mechanism: 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 at home. An additional mechanism, which I do not model explicitly, is the explicit reaction of parents to language restrictions. The very threat that the policy constitutes for parents' minority identity might increase their child enculturation efforts beyond the simple replacement of schooling with their own investment.

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) and those born to homogamous German couples. Ethnic identity is expected to be stronger when both parents are German, not only because the child then has two German role models in the family instead of one, but also 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).[23]

Table 1.7 repeats the analysis of Section 1.6.1 for the border data set, this time examining individuals whose father is German but whose mother is not. The effect of legislation on endogamy rates now becomes negative in the majority of specifications, suggesting a 2 to 10 p.p. lower likelihood of marrying another German among treated cohorts. A similar negative effect is estimated for the logarithm of the average GNI, though in this case estimates are noisy. The effect on the (logarithm of the) GNI of the first son is more precisely estimated in the range 13–50%.

Having one non-German parent makes it more likely for language restrictions to succeed in integrating German-Americans. To the extent that homogamous couples are more likely to be oppositional, this result is in line with Proposition 1. Both in the case of endogamy and of first names, estimates suggest an assimilating effect of legislation; however, this effect is almost always smaller in magnitude than the backlash observed in the group born to two German parents. Taken together with Section 1.6.1, these results suggest that language laws increase the variance in outcomes within the German group. This is in line with Proposition 2.

Figure 1.6 illustrates this divergence for the name index. Dashed and solid lines represent the difference in outcomes — between states with and without a law — for individuals with (respectively) one and two German parents. A gap between differences becomes evident starting with the cohort born in 1904, the oldest to be exposed to a language restriction (in its last year of compulsory education). The gap increases with exposure for subsequent cohorts and begins dropping for later cohorts, which are probably exposed to a different language regime in schools after the repeal of language legislation in 1923.

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

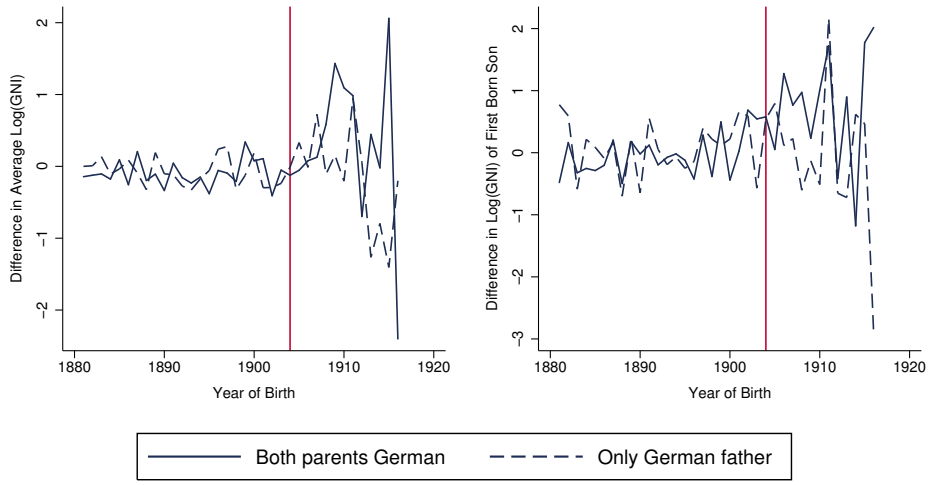
Table 1.7: Non-German mothers: Border data set

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Panel A | Dep. Variable: Spouse German | | | | | |
| Law × CSL age | −0.0365 | −0.0236* | −0.0274*** | 0.0101 | −0.0565** | −0.103* |
| | (0.0276) | (0.00897) | (0.00278) | (0.00524) | (0.0115) | (0.0295) |
| *p*-value | 0.372 | 0.236 | 0.004 | 0.280 | 0.004 | 0.000 |
| Observations | 6918 | 6918 | 6918 | 6918 | 2587 | 1890 |
| R-squared | 0.0253 | 0.0320 | 0.0560 | 0.0561 | 0.0801 | 0.0649 |
| Panel B | Dep. Variable: Log average GNI of children | | | | | |
| Law × CSL age | −0.00735 | −0.0372 | −0.0527 | 0.180 | −0.247*** | −0.103 |
| | (0.0719) | (0.0440) | (0.0475) | (0.0984) | (0.0258) | (0.0532) |
| *p*-value | 0.748 | 0.540 | 0.524 | 0.260 | 0.004 | 0.552 |
| Observations | 8829 | 8829 | 8829 | 8829 | 3475 | 2553 |
| R-squared | 0.0369 | 0.0452 | 0.0505 | 0.0507 | 0.0697 | 0.0561 |
| Panel C | Dep. Variable: Log GNI of first son | | | | | |
| Law × CSL age | −0.139 | −0.208** | −0.297** | −0.274*** | −0.270 | −0.495** |
| | (0.0600) | (0.0374) | (0.0541) | (0.00965) | (0.158) | (0.0624) |
| *p*-value | 0.356 | 0.004 | 0.064 | 0.004 | 0.416 | 0.004 |
| Observations | 6409 | 6409 | 6409 | 6409 | 2548 | 1858 |
| R-squared | 0.0347 | 0.0442 | 0.0603 | 0.0603 | 0.0970 | 0.0688 |
| Controls | Y | Y | Y | Y | Y | Y |
| State Controls × Cohort FE | N | Y | Y | Y | Y | Y |
| County FE | N | N | Y | Y | Y | Y |
| State trends | N | N | N | Y | N | N |
| Far from border | N | N | N | N | Y | N |
| Michigan border | N | N | N | N | N | Y |

**Notes:** In Panel A, the sample consists of males, born 1880–1916 to a German father and a non-German mother and living in a border county in 1920 and who were linked to the 1930 census. Regressions in Panels B and C are estimated in the pooled dataset of individuals linked in 1930 and 1940. All regressions include residence state in 1920 and birth cohort fixed effects, a border segment indicator and controls for the following name string properties: first and last name length and first and last name commonness. Regressions in Panels B and C include a census year indicator. State controls interacted with birth cohort dummies include the share of the German stock and the sex ratio among Germans in the state in 1910 in Panel A, and a border segment indicator in Panels B and C. Column [5] excludes individuals living within 5 km of the state border in 1920. Column [6] restricts the dataset to the northern border of Indiana and Ohio with Michigan. Standard errors are clustered at the state level. *P*-values reported are calculated using wild-cluster bootstrap at the state level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

Next I work with the sample of linked WWII enlistments. Columns [1]–[3] of Table 1.8 report specifications identical to those in Table 1.6 but for the subset of enlisted men with a German father and a non-German mother. The effect is negative, but smaller than in the case of individuals with two German parents, and only significant when interactions of birth cohorts and 1910 state controls are inserted in the specification. Volunteering rates decline by up to 8 p.p. for treated cohorts, indicating a less pronounced, but still present, strengthening of ethnic identification in response to language laws.

Figure 1.6: Difference in Germanness of first name by birthplace of parents




**Notes:** The figure plots the difference — between states with and without a language ban — in a measure of a first name's Germanness against the cohort of birth. The measure plotted in the left panel is the logarithm of the average German name index (GNI) of all children; the measure in the right panel is the logarithm of the first son's GNI (see Section 1.5.2 for details on construction of this index). In both cases, the data consist of males who were born 1880–1916 to a German father, who lived in a border county in 1920, and who were observed in 1930 or 1940. The red vertical line indicates the oldest cohort of children still at school (by compulsory schooling law) when German was banned.

Columns [4]–[6] of Table 1.8 reports the results of the triple-differences spec-

ification. Having a German father is associated with a lower average probability of volunteering; this effect is once again smaller in magnitude than the estimated coefficient in the case of two German parents. In any event, there is zero effect on volunteering rates for non-German cohorts that could be affected by school language legislation. As before, the negative effect on the volunteering rates of Germans (this time captured by the triple-interaction coefficient) is lower for individuals born to heterogamous couples.

Results from both datasets suggest a smaller (or absent) backlash effect for individuals whose parents do not share the same ethnic background. The finding is compatible with a theoretical mechanism in which the effort of enculturating children is higher for oppositional than for mainstream parents. For all our measures of assimilation, the variance in terms of outcomes within the German community broadly defined increases in response to an English-only policy.

Table 1.8: Non-German mothers: WWII enlistments

| Dep. variable: | Volunteered in 1942 | | | | | |
|---|---|---|---|---|---|---|
| | Difference-in-differences | | | Triple differences | | |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Law × CSL Age | −0.0633 | −0.0572 | −0.0774** | −0.00217 | −0.00702 | −0.00492 |
| | (0.0302) | (0.0271) | (0.0220) | (0.0267) | (0.0258) | (0.0285) |
| Only father German | | | | −0.0641 | −0.0743 | −0.0725 |
| | | | | (0.0985) | (0.102) | (0.102) |
| Law × CSL Age × Only father German | | | | −0.0605*** | −0.0452** | −0.0468** |
| | | | | (0.0104) | (0.00919) | (0.00898) |
| *p*-value | 0.232 | 0.013 | 0.256 | 0.004 | 0.004 | 0.004 |
| Observations | 543 | 542 | 542 | 41519 | 41502 | 41502 |
| R-squared | 0.0818 | 0.0997 | 0.147 | 0.0177 | 0.0387 | 0.0390 |
| Controls | N | Y | Y | N | Y | Y |
| Share German in state in 1910 × Cohort FE | N | N | Y | N | N | Y |

**Notes:** The sample consists of males, born 1880–1916 in Indiana, Ohio, Michigan, or Kentucky, who enlisted in the US Army in 1942 and who were linked to the 1930 census. Columns [1]–[3] restrict the sample to individuals with a German father and a non-German mother. 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. Columns [4]-[6] include interactions of the indicator for a German father with state-of-birth and birth cohort dummies. Columns [2]–[3] and [5]–[6] control for marital status and the number of dependent family members. Standard errors are clustered at the state of birth level. *P*-values reported are calculated using wild-cluster bootstrap at the state of birth level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

### 1.7.2 Ethnic composition and proximity to schools

**Community size.** Does the language ban's effect depend on the share of Germans in the community? The answer to this question is not clear a priori. On the one hand, communities with more Germans might be better organized and hence

better able to react against efforts to suppress their culture.[24] On the other hand, smaller communities tend to be more cohesive in their actions. Both my theoretical framework and the models of cultural transmission (e.g. Bisin and Verdier 2001) predict a stronger backlash among smaller minorities. Because a child is more likely to be assimilated when part of a small minority, parents are more incentivized to invest heavily in that child's identity. Thus smaller minorities have a stronger sense of ethnic identity.

I examine how the backlash effect of the law for individuals with German parents depends on the share of Germans in their county. For this I employ a triple-differences specification (cf. Section 1.6) in which the treatment dummy is interacted with the share $S_{sj}$ of first- and second- generation Germans in the county in 1910:

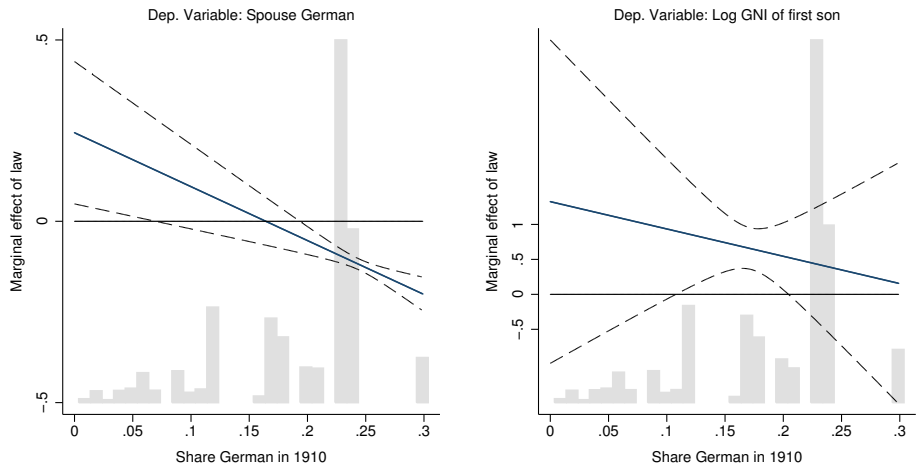
$$\begin{aligned} Y_{isjc} =& \alpha + \lambda_c + \theta_s + z_{sj} + \beta_1 T_{sjc} + \gamma_1 S_{sj} + \sum_{c=1}^{37} \gamma_{2c} S_{sj} \times \lambda_c \\ &+ \sum_{s=1}^{4} \gamma_{3c} S_{sj} \times \theta_s + \beta_2 T_{cjs} \times S_{sj} + \delta \mathbf{Z}_{isjc} + \varepsilon_{isjc} \end{aligned} \tag{1.8}$$

where $j$ denotes counties and $z_{sj}$ is a county fixed effect. Figure 1.7 plots the coefficient $\beta_2$ and 90% confidence intervals against the share of Germans, $S_{sj}$. The dependent variable in the left panel is an indicator for a German spouse; in the right panel, the dependent variable is the logarithm of the GNI of the first son.[25] In both cases, the magnitude of the coefficient is decreasing in the share of Germans, indicating a greater reaction in counties where Germans constitute a smaller minority (the claim of Proposition 3). In the case of names, the effect of the law is always one of backlash. If the community is large enough, it can (eventually) reverse the trend of a backlash that initially manifests as increased rates of endogamy.

[24] In a model of language assimilation emphasizing a trade-related mechanism, Lazear (1999) shows that smaller minorities are more likely to assimilate.

[25] Here and in the rest of Section 1.7.2, results are strongly similar qualitatively when the dependent variable is instead the log average GNI of all children.

Figure 1.7: Ethnic composition and effects of the language ban




**Notes:** The figure plots the triple interaction coefficient from a regression specified in (1.8) against the share of first- and second-generation Germans in a county in 1910. Dashed lines represent 90% confidence intervals. The underlying histograms show how the data is distributed across counties with different shares of Germans in 1910. The left-panel regression (with endogamy as the dependent variable) is estimated for the 1930 linked data set; the right-panel regression (with the logarithm of the first son as dependent variable) is estimated for the pooled 1930 and 1940 linked data sets. In all cases, the data are restricted to individuals with two German parents. Data on county shares of German ethnic stock is from Haines and Inter-university Consortium for Political and Social Research (2010).

**Strength of identity.** Both theory and common sense suggest that a reaction to language restrictions should be increasing in the strength of initial ethnic identity. Section 1.7.1 provides evidence for this by showing that the observed backlash weakens substantially or disappears among individuals with only one German parent. I use two additional approaches to test whether my data supports that hypothesis. First, I examine how the extent of the backlash differs by the share of Lutherans in a county. Although most German-Americans in the United States at the start of the 20th century were Catholics, it was Lutheranism that had the most German members (Wüstenbecker, 2007). The Lutheran religion was also the one

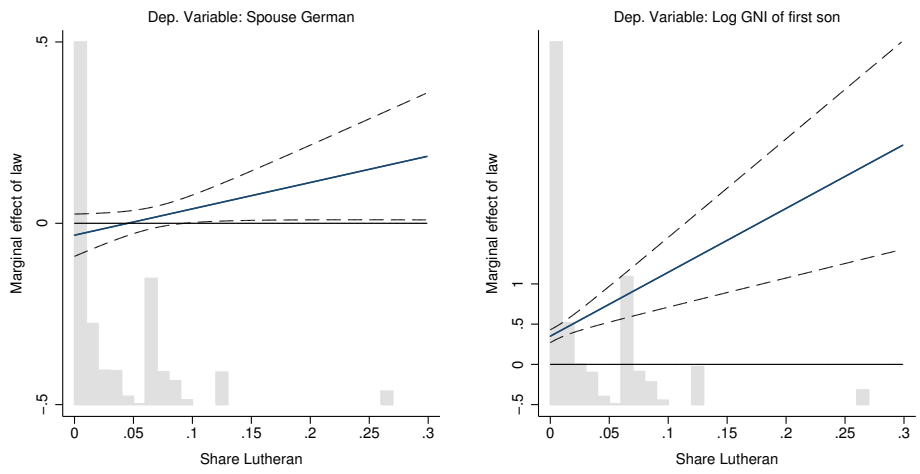
most strongly emphasizing conservation of the German language as a medium for transmitting the faith. Lutheran churches could follow this language policy more independently than could German Catholic churches, which were guided not by Germany but rather by the Pope in Rome (Rippley, 1985; Wüstenbecker, 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).

With Lutheranism as a proxy for German ethnic identity, I use a specification identical to equation (1.8) but with $S_{sj}$ now denoting the share of members of Lutheran churches in a county in 1906. Data for calculating this share are from the 1906 Census of Religious Bodies. Figure 1.8 plots the triple-interaction coefficient against the share of Lutherans for endogamy (left panel) and against the logarithm of the GNI of the first son (right panel) 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. To the extent that a strong Lutheran Church presence implies a greater sense of Germanness, this result verifies Proposition 4.

Another way to examine the effect of initial Germanness on the backlash is by using information on Lutheran schools. The Lutheran Church–Missouri Synod — one of the largest Lutheran synods in the United States, with both a German character and a strong presence in the Midwest[26] — operated a large network of parochial schools in the early 20th century. These schools provided instruction in a mixture of German and English, and most teachers were pastors born in Germany. The *Statistical Yearbook* of the Missouri Synod describes all the parochial schools operated by the synod. I use information from the 1913 yearbook (the last one published before WWI) to compute distances between the location of individuals in the border data set and their nearest parochial school. Figure 1.9 shows the spatial distribution of these schools in the set of four states together with the location of individuals in my data. The presence of the Synod in Kentucky was

[26]Until WWI, all the publications of the Lutheran Church-Missouri Synod were printed in German. Vocal advocates of conserving *Deutschtum* viewed the Missouri Synod as a protector of the German language (Viereck, 1903).

Figure 1.8: Strength of ethnic identity and effects of the language ban



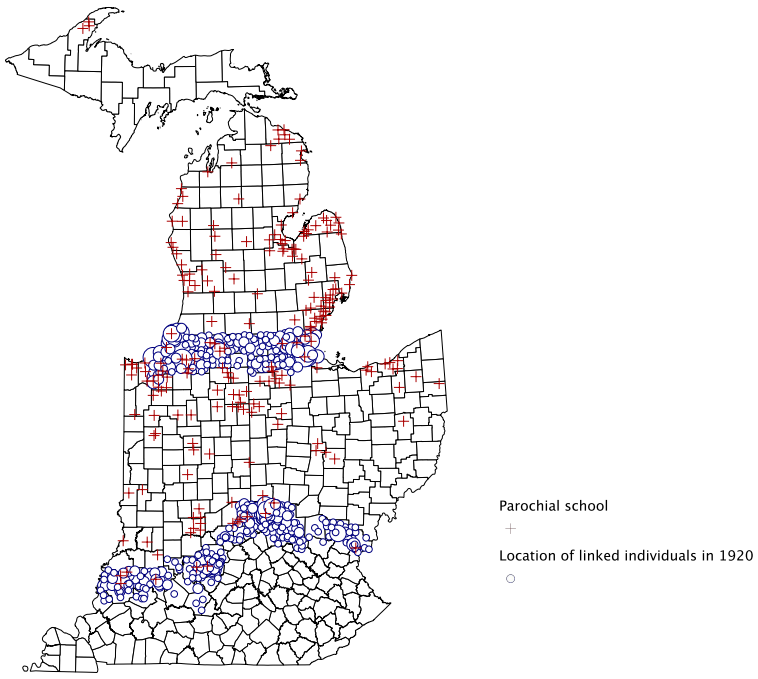

**Notes:** The figure plots the triple-interaction coefficient from a regression specified in (1.8) against the 1906 share of Lutherans in a county. Dashed lines represent 90% confidence intervals. The underlying histograms show how the data is distributed across counties with different shares of Lutheran church members. The left-panel regression (with endogamy as the dependent variable) is estimated for the 1930 linked data set; the right-panel regression (with the logarithm of the first son as dependent variable) is estimated for the pooled 1930 and 1940 linked data sets. In all cases, the data are restricted to individuals with two German parents. Data on county shares of Lutheran church members are from the 1906 Census of Religious Bodies .

weak, in line with the differences observed between Kentucky and the other three states in terms of German and Lutheran presence.

Two effects follow from closeness to a Lutheran school. First, it increases the probability that a child received German instruction and thus the probability that he was affected by the law. Second, proximity to a school increases the likelihood that both student and parents have a stronger sense of German identity — given that types with a preference for German education would naturally sort close to such schools. In the case of oppositional German individuals, both effects go in the same direction.

Table 1.9 reports the average effect of language laws by distance to a Lutheran

Figure 1.9: Schools of the Lutheran Church-Missouri Synod in 1913




**Notes:** Data on the location of parochial schools are from the *Statistical Yearbook* of the Lutheran Church-Missouri Synod for the year 1913.

school. An individual who is "close" to such a school lives within a 3-km radius of one on her side of the state border. Each pair of columns reports results for a different dependent variable (intermarriage, the log average GNI of all children, and the log GNI of the first son). In all cases the effect of a language law is much larger for individuals living close to a Lutheran school. These results are further evidence that the backlash effect of legislation is increasing in the strength of ethnic identity.

Table 1.9: Proximity to Lutheran schools

| Dep. variable: | Spouse German | | Log average GNI of children | | Log GNI of first son | |
|---|---|---|---|---|---|---|
| | Close | Far | Close | Far | Close | Far |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Law × CSL age | 0.119*** | −0.00235 | 0.911* | 0.243 | 1.281*** | 0.564*** |
| | (0.0123) | (0.00753) | (0.341) | (0.151) | (0.0357) | (0.384) |
| $p$-value | 0.000 | 0.804 | 0.000 | 0.368 | 0.000 | 0.000 |
| Observations | 3528 | 8307 | 3312 | 10656 | 2411 | 7754 |
| R-squared | 0.0789 | 0.0606 | 0.0553 | 0.0386 | 0.0400 | 0.0427 |

**Notes:** "Close" refers to individuals living within a 3-km radius of a Lutheran school located in their state in 1913. In columns [1] and [2], the sample consists of males born 1880–1916 to German parents, who lived in a border county in 1920 and who were linked to the 1930 census. Regressions in columns [3]–[6] are estimated in the pooled dataset of individuals linked in 1930 and 1940. All regressions include residence state in 1920 and birth cohort fixed effects, a border segment indicator, county 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] and [2] include interactions of the share of German stock and the sex ratio among Germans in 1910 with birth cohort dummies. Regressions in columns [3]–[6] include interactions of the share of German stock in 1910 and of the border segment with birth cohort dummies, as well as a census year indicator. Standard errors are clustered at the state level. $P$-values reported are calculated using wild-cluster bootstrap at the state level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

## 1.8 Extensions and robustness

In this section, I test whether prohibiting German affected assimilation by acting directly on the education outcomes of treated cohorts. Next, I provide evidence that language legislation had similar effects on Germans nationwide, as it did in the restricted set of states I focused on so far; I do this by examining how endogamy rates of Germans responded to language laws in the country as a whole.

### 1.8.1 Other outcomes

**Effects on educational achievement.** One way through which language in elementary school can affect life outcomes such as intermarriage, is via ethnic preferences. In particular, both the theoretical framework and the subsequent discussion focused on the channel of parental investment, with German parents reacting to language restrictions at school by adjusting their own investment in their children's ethnic identity. Yet it is also possible that banning the German language from elementary schools has direct effects on the content and quality of education of German-American children.[27] Such effects could in turn impact language proficiency, mobility rates, or cultural adaptabilty and thereby intermarriage (Bleakley and Chin, 2010; Wozniak, 2010; Furtado and Theodoropoulos, 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).

Panel A of Table 1.10 tests these notions, by examining how the German language ban affected the years of schooling completed by individuals in the border data set. Columns [1] and [2] report heterogeneous effects by nativity of the mother. There is a trend toward fewer (resp. more) years of schooling for children of German parents (resp. mixed couples), but none of the coefficients is significant and the respective magnitudes are small. Thus the evidence for direct effects of the law on schooling is weak.[28] This lends support to the claim that observed effects resulted mainly from ethnic preferences and the parents' socialization efforts.

[27] Eriksson (2014) and Ramachandran (2013) demonstrate that mother tongue instruction in primary school has positive effects on years of schooling, literacy and wages in South Africa and Ethiopia respectively.

[28] Lleras-Muney and Shertzer (forthcoming) show that English-only laws in the United States during the Americanization period had only small effects on the literacy of immigrant children in general and no effects for children in the second generation.

Table 1.10: Educational and labor market outcomes

| | Both parents German | Only father German | Both parents German | Only father German |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| | Panel A: 1940 | | | |
| Dep. Variable | Years of schooling | | Log yearly wage income | |
| Law × CSL age | -0.112 | 0.138 | −0.203** | 0.0687 |
| | (0.222) | (0.141) | (0.0615) | (0.0986) |
| $p$-value | 0.740 | 0.584 | 0.027 | 0.744 |
| Observations | 16338 | 11368 | 11443 | 7947 |
| R-squared | 0.0821 | 0.102 | 0.0246 | 0.0320 |
| | Panel B: 1930 and 1940 | | | |
| Dep. Variable | Lives in same state | | Lives in same county | |
| Law × CSL age | −0.0221 | −0.000349 | −0.00893 | 0.0555 |
| | (0.0162) | (0.0146) | (0.0227) | (0.0426) |
| $p$-value | 0.480 | 0.920 | 0.680 | 0.552 |
| Observations | 33765 | 23217 | 33765 | 23217 |
| R-squared | 0.0333 | 0.0412 | 0.0650 | 0.0865 |

**Notes:** In Panel A, the sample consists of males born 1880–1916 to a German father, who lived in a border county in 1920 and who were linked to the 1940 census. Regressions in Panels B and C are estimated in the pooled dataset of individuals linked in 1930 and 1940. All regressions include residence state in 1920 and birth cohort fixed effects, a border segment indicator, county fixed effects, interactions of the share of German stock in 1910 and a border segment indicator with birth cohort dummies and controls for the following name string properties: first and last name length and first and last name commonness. Regressions in Panel B include a census year indicator. When the dependent variable is log yearly wage income, the dataset is restricted to salaried workers. Standard errors are clustered at the state level. $P$-values reported are calculated using wild-cluster bootstrap at the state level (Cameron et al., 2008).
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

**Backlash costs.** The analysis in preceding sections, established that removing the German language from school curricula affected German children's assimilation along various dimensions; there is also evidence of a clear backlash effect, espe-

cially for children of homogamous couples. Is this backlash, or strengthening of ethnic identity, costless 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., 2013) — indicate that, notwithstanding the possibility of immigrants' self-selection, assimilation entails a premium in the labor market.[29] 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 community and thus may be sacrificing mobility by retaining strong ties with their communities.

Columns [3]–[4] in Panel A of Table 1.10 indicate that such costs might apply in the case of German-Americans. The estimated effect of the German language ban on the log of yearly wage earnings of individuals with German parents is large: exposure to that law implies a 20% reduction in yearly wage income for this group. There is no clear indication of an assimilation premium for the mixed group; the estimated coefficient is positive but far from significant, and the magnitude is small. Given that the reaction of this group was not uniform in terms of all outcomes examined, and also often moved in the direction of a backlash, this latter result makes sense.

Panel B of Table 1.10 examines spatial mobility. The dependent variable in columns [1]–[2] is an indicator for individuals who lived in the same state in 1930 or 1940 as they did in 1920; in columns [3]–[4] the indicator refers to living in the same county. In no case is there any effect of the language legislation for any subsets of the data. The mobility rates of the more German group are higher, if anything, but the effect is small and never significant.

### 1.8.2 Countrywide evidence

I use the 1930 5% and 1960 1% IPUMS samples (Ruggles et al., 2010) to examine how language laws affected the assimilation of German-Americans in the country

[29] A related body of work on blacks adopting a white racial identity in 19th and 20th century America (Mill and Stein, 2015; Nix and Qian, 2015) uncovers a substantial positive effect of "passing" on economic outcomes.

as a whole.[30] During the late 19th and early 20th centuries, endogamy rates among immigrants in the United States were high; among Germans, however, within-group marriage rates were trending downward (see Figure 1.16). I focus my attention on cohorts born to a German father between 1880 and 1916. Both the passage of English-only laws and the compulsory schooling age range differ by state, but cohorts born during the period 1902–1916 are at least partly exposed to the legislation, whereas earlier cohorts are old enough to have left school by the time a law was enacted in their state. I ignore cohorts born later than 1916, who first went to school after restrictive legislation was lifted in 1923. Although technically they were not exposed to a language law, comparing them with affected cohorts would not be especially informative because the post-war status of German in the schools remained low at least until the 1930s (Sowell, 1996).

I match individuals in the census with legislation enacted in their state of birth, so that exposed cohorts are those born in a state with an English-only law and in school according to the compulsory schooling law at the time that the language law was in effect. For this reason, the sample is restricted to second-generation immigrants. In the 1960 census, a person's foreign-born wife is coded as "foreign-born" and without any details about her particular birthplace. For this reason, in the 1960 sample I can distinguish only between whether an individual's mother is foreign or native; I am unable to determine whether she is German. I estimate

$$Y_{isc} = \alpha + \beta T_{cs} + \lambda_c + \theta_s + \varepsilon_{isc} \tag{1.9}$$

where $T_{cs}$ is an indicator for individuals born in a state with a law and who were within the age range for compulsory schooling at the time that law was in place. The terms $\lambda_c$ and $\theta_s$ signify cohort and state of birth/residence fixed effects.

Column [1] of Table 1.11 reports the estimates of a regression like equation (1.9) in the pooled 1930 and 1960 IPUMS samples of second-generation German males born 1880–1916 to German parents. Column [2] controls for a linear state-specific trend and column [3] adds interactions of baseline state characteristics with birth cohort fixed effects. These are: the share of first- and second-generation Germans in the state in 1910, the sex ratio (computed as the ratio of males to females) in

[30] I can only observe the ethnic background of the spouse of a native person in 1930 and 1960.

the state and the same ratio among first- and second-generation Germans. The magnitude of the estimated coefficient varies little across specifications. Being in a potentially affected cohort is associated with roughly a 2 p.p. increase in the probability of having a German spouse; compared with the 37.5% average endogamy rate in the entire sample of males with two German parents, this effect is not negligible.

Table 1.11: Endogamy in IPUMS

| | Both parents German | | | Only father German | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Law × CSL age | 0.0233* | 0.0270** | 0.0207* | 0.0111 | −0.0126 | −0.00884 |
| | (0.0117) | (0.0123) | (0.0108) | (0.0175) | (0.0205) | (0.0196) |
| Observations | 33432 | 33432 | 33432 | 17385 | 17385 | 17385 |
| R-squared | 0.0920 | 0.0948 | 0.0983 | 0.0649 | 0.0695 | 0.0770 |
| Residence state FE | Y | Y | Y | Y | Y | Y |
| State of birth trends | N | Y | Y | N | Y | Y |
| State of birth controls × Cohort FE | N | N | Y | N | N | Y |

**Notes:** Reported values are derived from a linear probability model. Regressions are estimated in the pooled 1930 5% and 1960 1% IPUMS samples; standard errors (reported in parentheses) are clustered at the state-of-birth level. The dependent variable is an indicator for a spouse that is German-born or has either parent German. The sample consists of males born in the United States (excluding Hawaii, Alaska, and the District of Columbia) during the period 1880–1916 to a German father. German mothers are identified as born in Germany in 1930 or as being foreign-born and married to a German-born spouse in 1960. All regressions contain both state-of-birth and birth cohort fixed effects (FE) as well as a census year indicator. Columns [2], [5], and [8] control for a linear trend specific to the state of birth. The state-of-birth controls that are interacted with birth cohort dummies in columns [3], [6], and [9] include the share of the German stock, the sex ratio, and the 1910 sex ratio among first- and second-generation Germans in the state of birth.
Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

Results are less clear for individuals born to mixed couples of German fathers and non-German mothers. Columns [4]–[6] report a non significant effect of lan-

guage laws on endogamy for this group; that effect becomes negative with the inclusion of state trends and becomes zero when the specification includes interactions between cohorts and state controls.

Table 1.11 verifies that the results for Indiana, Ohio and their neighboring states go through for the country as a whole. Language policies seem to have no assimilation effect — at least as measured by intermarriage rates. On the contrary, such policies lead to increasing endogamy for the more German group and to increasing variance in endogamy rates within the larger German population.

These results should be viewed as suggestive only — given the extensive heterogeneity in enacted legislation and pre-existing legislative frameworks across states as well as the heterogeneity in characteristics of the German population. They do, however, closely replicate the findings of Section 1.6 and lend support to the mechanism of cultural transmission proposed in the model.

## 1.9 Discussion

The findings reported here suggest that children of parents with a strong attachment to their ethnicity are more likely to react negatively to assimilation policies. My theoretical framework offers a plausible mechanism for this result. 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 may be insufficient to counteract the assimilating effect of an education that is less ethnic in character. In my model, the psychological benefit derived from social interactions with other oppositional types acts as a multiplier of parental investment. This peer effect channel can induce investment that is high enough to result in a greater number of oppositional children in society.

The mechanism is similar to the one suggested by Bisin et al. (2011) and leads to similar predictions. In their theoretical framework, the child is allowed to choose his 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. So in this model, a policy like the one I examine would lead to fewer but more intensely oppositional types.

Compared with Bisin et al. (2011), my framework places less emphasis on the child's choice and more on parental investment which, in the context of a language law in elementary school, is the mechanism more likely to affect children's cultural preferences. It is however reasonable to propose a mechanism through which children themselves react when their identity is threatened. Such is the framework in Bénabou and Tirole (2011), in which identity is an asset built from 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 against whom the majority population discriminates, can lead to costly investments in identity if the initial ethnic identification (here the sense of Germanness among the parents' generation) was strong.

## 1.10 Conclusion

Can cultural integration be engineered through government policies? I examine the prohibition of German in US 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 two main effects: (i) a negative effect on the assimilation of the more German group, those with both parents born in Germany (ii) an increase in the variance of assimilation outcomes within the German community. Both of these effects are 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, and for individuals who lived closer to Lutheran parochial schools. In other words, an ethnic community's initial degree of identity determines the magnitude of its reaction to assimilation efforts.

Can the historical case study of US Germans inform modern-day language and integration policies? The debate about language restrictions is very much alive in immigrant receiving countries and states, such as California and Germany.[31] This

[31]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

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 US[32] implies that negative consequences of assimilation policies may be even more likely amongst poor marginalized groups — such as the Muslim population in Europe.

This paper implies that policies favoring 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. 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.

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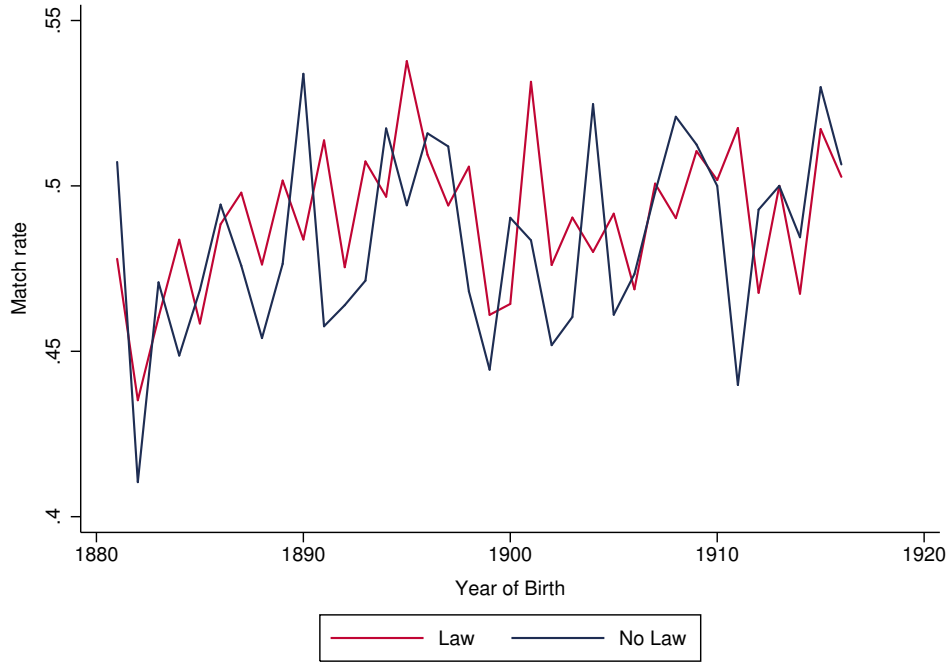
the language is a precondition for successful integration" (Crutchfield, 2007).

[32]They had the highest rates of naturalization among the foreign-born (Rippley, 1985).

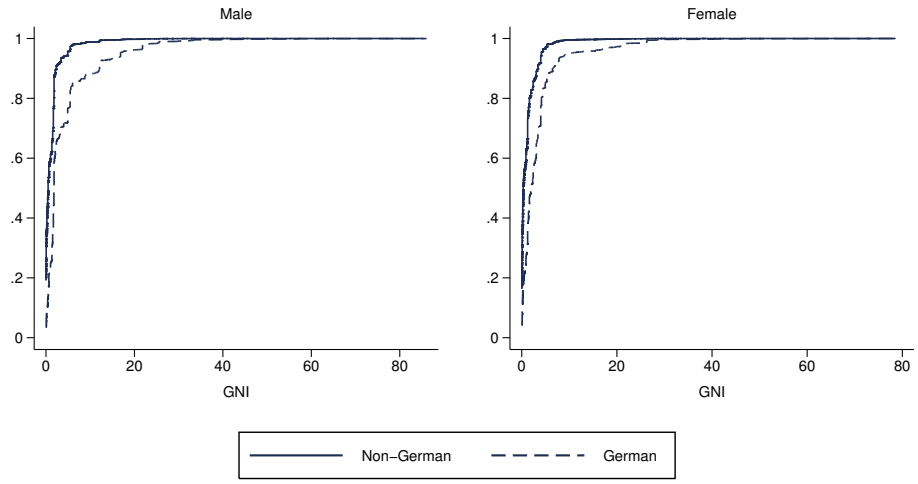
## 1.11 Appendix: Additional figures and tables

Figure 1.10: Match rate for border data set, by cohort and language law status



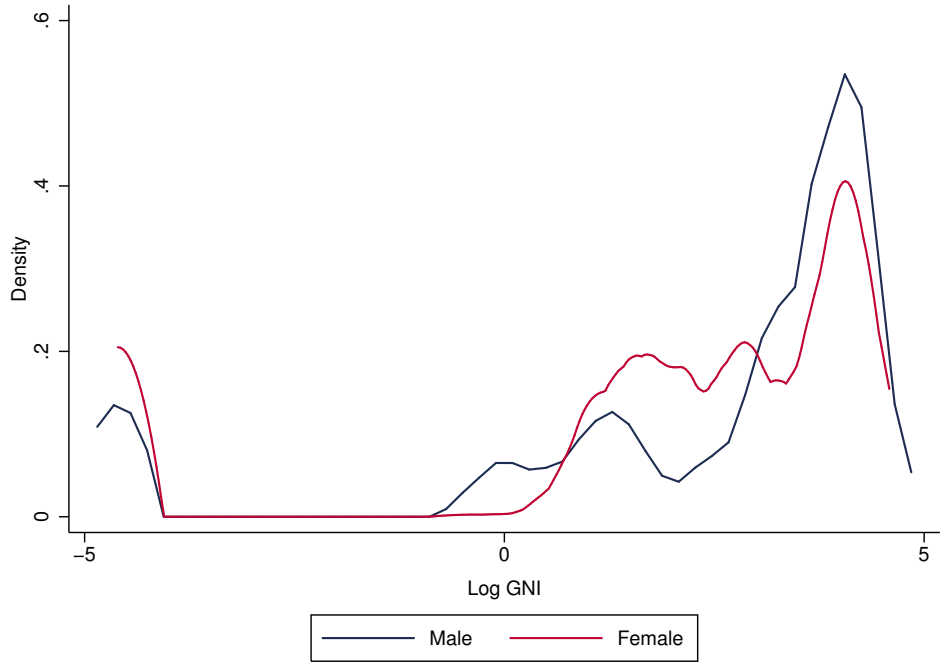

**Notes:** The figure plots the share of all males, born 1880–1916 to German parents and living in a border county in 1920, that could be linked to the 1930 or 1940 census against birth cohort for states with and without a language ban.

Figure 1.11: Cumulative distribution function of the GNI in the 1930 IPUMS by gender and German origin



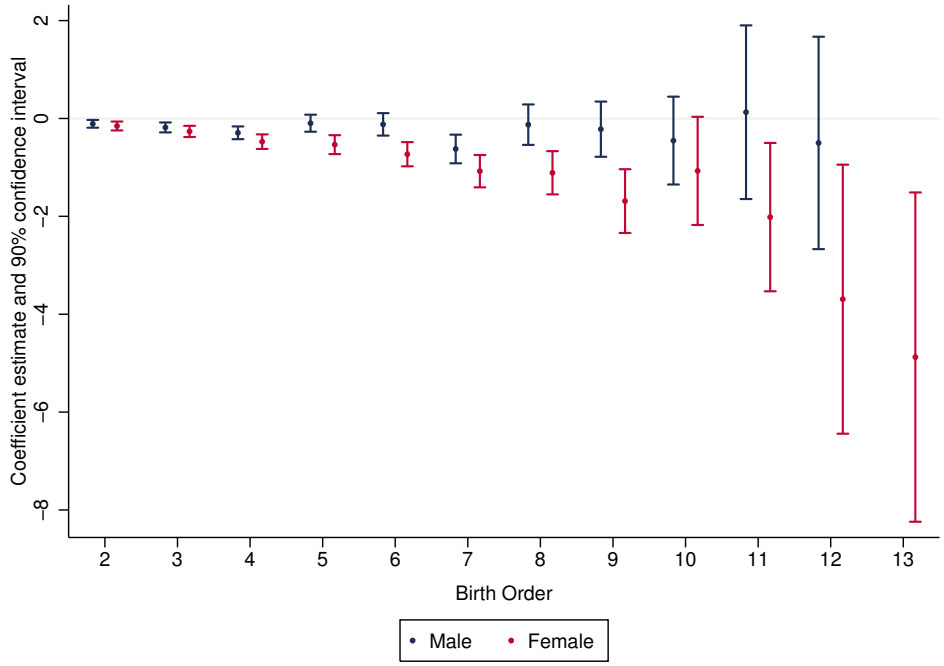

**Notes:** The figure plots the cumulative distribution function of the GNI in the 1930 5% IPUMS sample. German origin applies to individuals born in Germany or of German-born parents; non-German origin applies to the rest of the sample (whether of foreign or US nativity).

Figure 1.12: GNI density by gender: Children in border data set



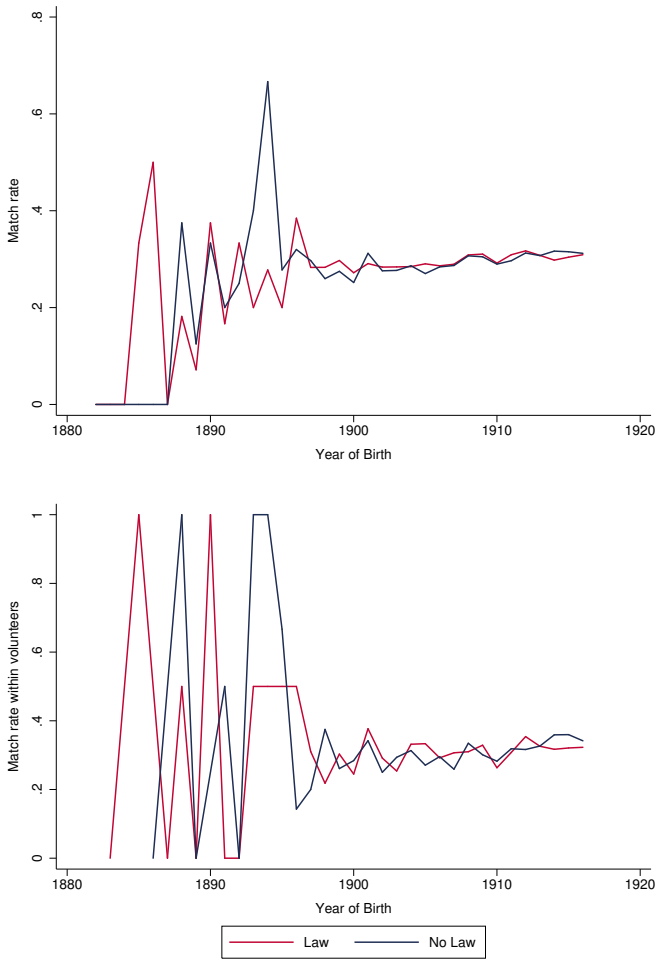

**Notes:** The figure plots the kernel density of the logarithm of the GNI for children of individuals in the border data set. The unit of observation is a child whose father (a) was born 1880–1916 to German parents, (b) lived in a border county in 1920, and (c) was linked to the 1930 or 1940 census. See Section 1.5.2 for details on construction of the GNI.

Figure 1.13: Correlation of German names with gender and birth order



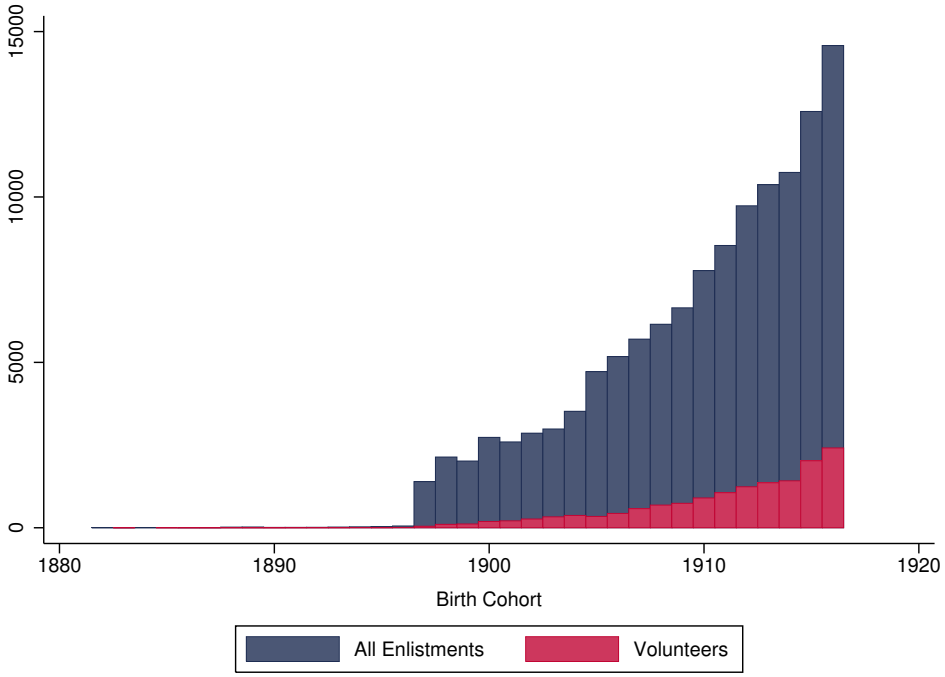

**Notes:** The figure depicts the coefficients and 90% confidence intervals from a regression of the logarithm of GNI on birth order indicators; results are plotted separately for males and females. The sample consists of children of males who were born 1880–1916 to German parents, who lived in a border county in 1920, and who were linked to the 1930 or 1940 census. See Section 1.5.2 for details on the construction of the GNI.

Figure 1.14: Match rate for WWII enlistments, by cohort and language law status



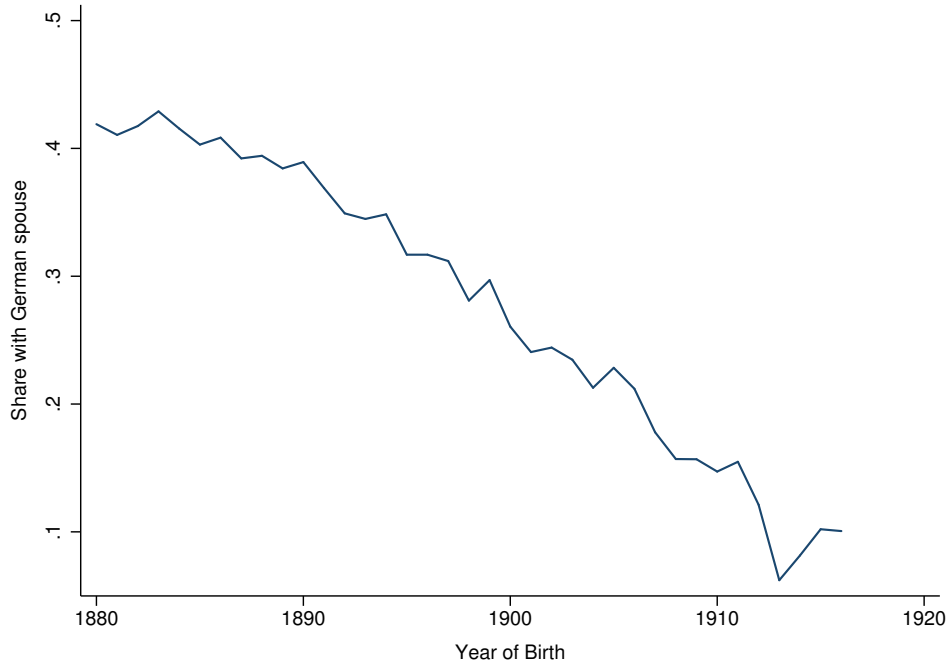

**Notes:** The upper panel plots the share of all enlisted men born 1880–1916 in Indiana, Ohio, Michigan, or Kentucky who could be linked to the 1930 census against birth cohort, for states with and without a language ban; the lower panel plots the respective share for the subset of enlisted men who were volunteers. Enlistment data are from the WWII Army Enlistment Records compiled by the National Archives and Records Administration.

Figure 1.15: 1942 enlistments and volunteers by birth cohort




**Notes:** This histogram plots the distribution of a random sample of men who enlisted in the US Army in 1942 by cohort of birth. Volunteers are identified as having a serial number belonging to the 11 through 19 million series, and the sample is restricted to males born 1880–1916 in Indiana, Ohio, Michigan, or Kentucky. Enlistment data are from the WWII Army Enlistment Records compiled by the National Archives and Records Administration.

Figure 1.16: Endogamy rates of second-generation Germans in IPUMS sample




**Notes:** The figure depicts the share of native-born married males born 1880–1916 to a German father and whose spouse is either German-born or has either parent German. The data are from the pooled 1930 5% and 1960 1% IPUMS samples and exclude individuals born in Hawaii, Alaska, and the District of Columbia.

Table 1.12: Summary of legislation enacted 1917–1923

| State | Year | English made language of instruction | Teaching of foreign languages prohibited |
|---|---|---|---|
| Wisconsin | 1917 | Public schools | Public schools |
| Louisiana | 1918 | All schools | All educational institutions, German only |
| North Dakota | 1918 | All schools | – |
| Alabama | 1919 | All schools, Grades 1–6 | All schools, Grades 1–6 |
| Arkansas | 1919 | Private and public schools | – |
| Colorado | 1919 | All schools | Public schools, elementary grades |
| Delaware | 1919 | All schools, Grades 1–6 | All schools, Grades 1–6 |
| Iowa | 1919 | All schools | All schools, Grades 1–8 |
| Indiana | 1919 | All schools | All schools, Grades 1–8, German only |
| Minnesota | 1919 | All schools, common school branches | All schools |
| Kansas | 1919 | Public, private and parochial schools | – |
| Maine | 1919 | All schools, common school branches | – |
| Nebraska | 1919 | All schools | All schools, Grades 1–8 |
| Ohio | 1919 | All schools | All schools, Grades 1–8, German only |
| Oklahoma | 1919 | All schools | All schools, Grades 1–8 |
| Oregon | 1919 | Public and private schools | – |
| Pennsylvania | 1919 | All schools, common English branches | – |
| South Dakota | 1919 | Public and private schools, enumerated subjects | – |
| West Virginia | 1919 | All schools, common school branches | – |
| Pennsylvania | 1921 | All schools | – |
| South Dakota | 1921 | – | All schools, during regular school year, Grades 1–8 |
| Connecticut | 1923 | All schools | All schools |

**Notes:** Data from Edwards (1923) and Knowlton Flanders (1925).

Table 1.14: Summary statistics: Non-German mothers, border sample

| | Found in 1930 | | | Found in 1940 | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | *N* | Mean | S.D. | *N* |
| Married | 0.631 | 0.482 | 11698 | 0.800 | 0.400 | 11519 |
| Spouse of German ancestry | 0.277 | 0.448 | 6930 | – | – | – |
| Number of children | 2.182 | 1.469 | 3610 | 2.516 | 1.640 | 5374 |
| Log average GNI of children | 2.284 | 2.405 | 3524 | 2.627 | 2.026 | 5305 |
| Log GNI of first son | 2.317 | 2.567 | 2452 | 2.333 | 2.662 | 3957 |
| Lives in same state as 1920 | 0.883 | 0.322 | 11698 | 0.812 | 0.391 | 11519 |
| Lives in same county as 1920 | 0.772 | 0.420 | 11698 | 0.606 | 0.489 | 11519 |
| Years of education | – | – | – | 9.127 | 3.028 | 11368 |
| Yearly salary earnings | – | – | – | 6.599 | 2.511 | 7947 |

**Notes:** The table shows summary statistics for males born 1880–1916 to a German father and non-German mother, who in 1920 lived in a county of Indiana (IN) and Ohio (OH) that bordered Michigan (MI) or Kentucky (KY) and who were linked to the census of 1930 (left panel) or 1940 (right panel). See Section 1.5.2 for details on construction of the GNI variables.

Table 1.13: Characteristics affecting the probability of a match: Border data set

| | Both parents German | | | Only father German | | |
|---|---|---|---|---|---|---|
| | 1920 data set | Found in 1930 | Found in 1940 | 1920 data set | Found in 1930 | Found in 1940 |
| Demographics | | | | | | |
| Native born | 0.874 | 0.882 | 0.892 | 0.992 | 0.998 | 0.995 |
| | (0.331) | (0.323) | (0.311) | (0.088) | (0.050) | (0.073) |
| Mother native born | — | — | — | 0.928 | 0.941 | 0.912 |
| | | | | (0.259) | (0.236) | (0.283) |
| Born in different state | 0.128 | 0.122 | 0.124 | 0.132 | 0.120 | 0.121 |
| | (0.334) | (0.327) | (0.329) | (0.338) | (0.325) | (0.326) |
| Name string properties | | | | | | |
| First name length | 5.582 | 5.567 | 5.521 | 5.643 | 5.645 | 5.611 |
| | (1.372) | (1.265) | (1.312) | (1.372) | (1.286) | (1.334) |
| Last name length | 6.669 | 6.504 | 6.500 | 6.623 | 6.520 | 6.496 |
| | (1.956) | (1.853) | (1.834) | (1.928) | (1.861) | (1.826) |
| First name commonness | 4.917 | 5.348 | 5.333 | 4.508 | 4.867 | 4.821 |
| | (5.149) | (5.148) | (5.190) | (4.916) | (4.977) | (4.972) |
| Last name commonness | 0.151 | 0.153 | 0.134 | 0.162 | 0.165 | 0.139 |
| | (0.791) | (0.794) | (0.676) | (0.791) | (0.778) | (0.654) |
| Observations | 34830 | 17208 | 16557 | 22982 | 11698 | 11519 |
| Match rate | | 49.4% | 47.5% | 50.9% | 50.1% | |

**Notes:** The table reports means and standard deviations (in parentheses) for several characteristics of the border data set. Columns [1] and [4] refer to all males born 1880–1916 (respectively) to German parents and to a German father and a non-German mother and who lived in a border county in 1920. Columns [2]–[3] and [5]–[6] refer to the part of these data sets that could be linked to the census of 1930 and 1940. "Name commonness" is computed as the share of people in the 1920 1% IPUMS sample with the same first or last name multiplied by 1,000.

Table 1.15: Characteristics affecting the probability of a match: WWII enlistments

| | All 1942 enlistments | Records found in 1930 census |
|---|---|---|
| Demographics | | |
| Father foreign | 0.026 | 0.183 |
| | (0.159) | (0.387) |
| Mother foreign | 0.023 | 0.165 |
| | (0.151) | (0.371) |
| Father German | 0.025 | 0.025 |
| | (0.155) | (0.155) |
| Mother German | 0.021 | 0.021 |
| | (0.144) | (0.144) |
| Both parents German | 0.002 | 0.012 |
| | (0.041) | (0.107) |
| Name string properties | | |
| First name length | 5.719 | 5.735 |
| | (1.270) | (1.242) |
| Last name length | 6.381 | 6.362 |
| | (1.696) | (1.618) |
| First name commonness | 3.802 | 3.319 |
| | 4.905 | (4.591) |
| Last name commonness | 0.549 | 0.385 |
| | (1.496) | (1.094) |
| Observations | 293694 | 41519 |
| Match rate | | 14.1% |

**Notes:** The table reports means and standard deviations (in parentheses) for several of characteristics of the WWII enlistments data set. The first column refers to a random sample of all males — born 1880–1916 in Indiana, Ohio, Michigan, or Kentucky — who enlisted in the US Army in 1942. The second column refers to the part of this data set that could be linked to the 1930 census. "Name commonness" is computed as the share of people in the 1930 1% IPUMS sample with the same first or last name, (multiplied by 1,000).

Table 1.16: Summary statistics: Non-German mothers, WWII enlistments

| | Mean | S.D. | *N* |
|---|---|---|---|
| Age | 32 | 5.434 | 543 |
| Married | 0.232 | 0.423 | 542 |
| With dependents | 0.135 | 0.342 | 542 |
| Volunteer | 0.112 | 0.316 | 543 |
| High school graduate | 0.357 | 0.480 | 543 |
| College graduate | 0.048 | 0.214 | 543 |

**Notes:** The table reports summary statistics for a random sample of males who enlisted in the US Army in 1942 and were linked to the 1930 census. The sample comprises of cohorts born 1880–1916 in Indiana, Ohio, Michigan, and Kentucky, to a German father and a non-German mother. Volunteers are identified as having a serial number in the 11 through 19 million series.

# Chapter 2

# REPRISALS REMEMBERED: GERMAN-GREEK CONFLICT AND CAR SALES DURING THE EURO CRISIS

## 2.1 Introduction

When German Chancellor Angela Merkel visited Athens in 2012, she was greeted by demonstrators waving placards depicting her in Nazi uniform; protesters were denouncing the alleged rise of a "Fourth Reich". During the period 2010–12, the German and the Greek governments clashed repeatedly and in public over the terms of EU bailout packages. Greeks blamed German politicians for harsh austerity measures; officials from Germany made disparaging remarks about the country. The popular press in both Germany and Greece printed incendiary headlines and insulting images. Greek consumer groups called for a boycott of German goods. Strikingly, memories of World War II played an important role in Greek condemnations of German attitudes and policies: German institutions were defaced with swastikas; politicians suddenly demanded reparations for German war crimes committed a half-century earlier; and newspapers both foreign and Greek recounted massacres during the German occupation.

In this paper, we examine how German-Greek animosity affected consumer behavior — and how these events interacted with memories of earlier conflict. In particular, we test if areas in Greece that suffered German reprisals during World War II saw sharper changes in purchasing patterns than other parts of

the country. We focus on car purchases because cars are an archetypal German product.[1] Examining car purchases also constitutes a demanding test — for the average consumer, they represent a major (and rare) investment, which makes it less likely that political feelings and the desire to make a public statement influence consumer behavior.

We first compile an index of German-Greek political clashes based on newspaper reports in Greece and on the frequency of internet search terms. These indicators show an explosion of conflict after 2010, with three separate periods affecting a total of six months characterized by particularly intense public conflict. Sales of German-made cars suffered marked declines during these clashes. We combine this time-series information with detailed geographical data on the location of German massacres during the occupation, 1941–44. Following an upsurge in partisan activity, the German occupying forces adopted a policy of harsh reprisals. These involved burning whole villages, and killing the entire (male) civilian population in the vicinity of attacks (Mazower, 1995). To measure the severity of these attacks at the local level, we use lists drawn up by the Greek government designating localities as "martyred towns". These are based on a set of criteria including the percentage of homes destroyed, as well as the loss of human life. The locations of these martyred towns are then matched to prefecture-level car registration data.

We find strong evidence that public conflict and calls for a boycott of German products reduced sales of German automobiles in general; strikingly, these reductions were greater where the *Wehrmacht* had destroyed entire villages, committing large-scale massacres. Figure 2.1 shows the distribution of changes in the market share of German cars, during months of conflict and for tranquil periods, for prefectures with a large share of population affected by atrocities during World War II and the rest. The upper panel shows that in months without prominent clashes, changes in the market share of German-made automobiles were indistinguishable between prefectures with many and relatively few massacres. This changes drastically in times of conflict — now the entire distribution is shifted to the left for

[1]Some care manufactures use highly idiomatic and hard-to-pronounce German slogans in their advertising abroad to polish their German credentials (for exampe, Audi used "Vorsprung durch Technik" in English-speaking countries).

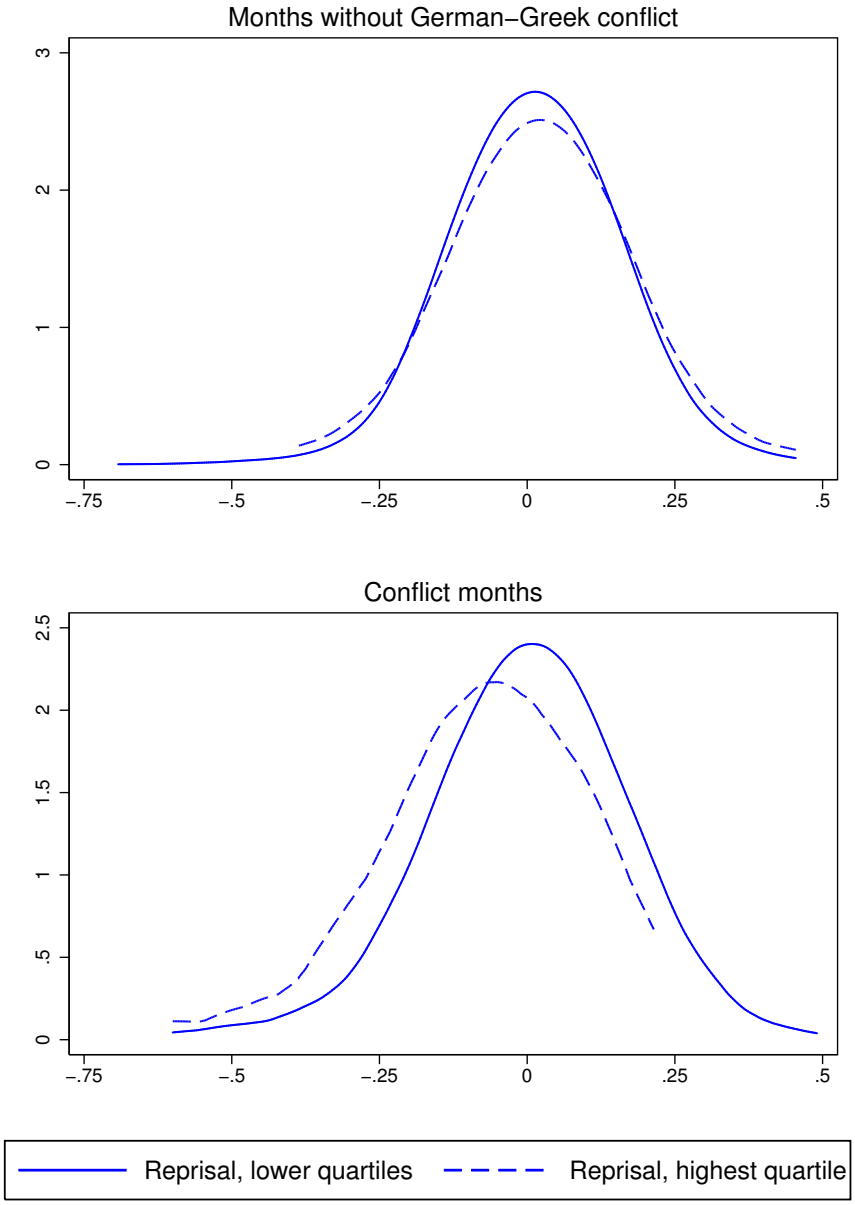
prefectures that suffered heavy German reprisals. The effect is large and significant, as we show in the main empirical section below — the difference-in-differences estimate of the effect of a conflict month on the German car market share in areas of Greece with a history of German war crimes is 3.8 percentage points. These findings suggest that consumer behavior — the purchase of big-ticket items like cars — responds strongly to general public sentiment; where local memories of earlier German atrocities could easily be activated by current events, purchases were curtailed sharply.

Boycotts are frequently used to articulate political views, but their effectiveness is doubtful. More than half of top brands in the US were targeted by a call for a boycott in the period 1980-2000. Nonetheless, there is only limited evidence that consumer behavior is directly influenced by calls for political action.[2] For example, boycotts of French wine after the country's failure to support the US in Iraq were probably ineffective (Ashenfelter et al., 2007).[3] Teoh et al. (1999) examine the effect of the South African Boycott on the stock prices of affected firms, and find that it had no clear impact. In general, the valuations of firms affected by boycotts do not react (Koku et al., 1997). The only counter-examples include a decline in tourist visits by Americans to France after 2003 (Michaels and Zhi, 2010); lower French car sales in China during the 2008 Olympics (Hong et al., 2011); and suggestive evidence that French-sounding products saw their sales slump in the aftermath of the Iraq war (Pandya and Venkatesan, forthcoming). The paper that is closest in spirit to ours is Fisman et al. (2013). They examine changes in stock market values of Chinese and Japanese firms after a cooling of Sino-Japanese relations in 2005 and 2010. This followed the introduction of new Japanese textbooks that downplayed events during the Japanese invasion of China in the 1930s such as the infamous "Rape of Nanking". Stock prices fell more for firms that had a higher sales exposure in terms of foreign sales; effects are strongest in industries with major public sector involvement. At the same time, there appears to be no effect on the stock prices of firms in consumer goods sectors. What is currently missing in the literature is clear evidence that politically-motivated conflict di-

[2]Sen et al. (2001) and John and Klein (2003) argue that free-rider problems make it highly unlikely for boycotts to succeed.

[3]Chavis and Leslie (2009) earlier concluded that French wine sales in the US suffered after the start of the Iraq war.

Figure 2.1: Changes in the market share of German cars




**Notes:** The graph shows changes in the market share of German cars, comparing to the same month a year earlier. The upper panel shows changes during periods without German-Greek conflict; the lower panel, during months of public conflict (for definition of conflict, cf the data section). In each panel, we plot the distribution of market share changes separately for prefectures in the lower three and in the upper quartile of population affected by German massacres during World War II, relative to the size of the pre-1940 population.

rectly influences consumer behavior, and that location-specific interpretations of the past modify these responses.

Our paper also contributes to an emerging literature on the importance of cultural factors in economic and social behavior. Countries that fought numerous wars in the past continue to trade less with each other to the present day, and they engage in less FDI (Guiso et al., 2009). Fertility behavior of immigrants' children is still influenced by their parents' country of origin (Fernández and Fogli, 2009), language characteristics are associated with savings behavior (Chen, 2013), and inherited trust can influence national growth rates (Algan and Cahuc, 2010). Many attitudes persist over long periods: Italian cities that were self-governing in the Middle Ages are richer and more civic-minded today (Guiso et al., 2008), areas of Africa affected by 19th century slave-hunts have lower trust in the present (Nunn and Wantchekon, 2011), and German cities that persecuted their Jews during the Black Death were markedly more anti-Semitic in the 1920s and 1930s (Voigtländer and Voth, 2012).

At the same time, culture is not only persistent, it can also change quickly: Attitudes towards pre-marital sex have been transformed in the last century (Fernández-Villaverde et al., 2014); Islam changed from an open and tolerant religion to a relatively intolerant one (Chaney, 2008); Franco-German conflict in the last 200 years was repeated and seemingly deeply rooted in cultural differences (Mann, 1916), but has vanished in the last 50 years. One of the key challenges for cultural economics is to analyse the conditions for persistence, and the context in which contemporary attitudes are no longer influenced by the past.

Our work also relates to a growing literature in psychology and economics that focuses on the role of memory as a cause of behavioral biases. Mullainathan (2002) presents a model in which memory limitations can explain a host of phenomena, from stock market volatility to consumption patterns. Kahneman and Tversky (1982) show that remembered events are typically regarded as more representative and more likely. Gennaioli and Shleifer (2010) show how "what comes to mind" can shape decision-making and lead to distorted decisions. Similarly, emotions (Kahneman, 2011) and fairness considerations (Fehr and Gächter, 2000) can also affect economic decision-making, especially when experiences related to the economic decision at hand have left individuals a strong emotional impression in the

past.

Relative to the existing literature, we make the following contribution: First, we are among the first to show that consumer behavior reacts to political events, in a way that is consistent with time-varying cultural aversion.[4] Second, during the Greek debt crisis, "reasons to hate" mattered more in periods of general conflict; the harshness of the remembered past influenced cross-sectional differences in the response to political events. Third, our paper demonstrates that purchases of big-ticket items (like cars) can be affected by political conflict and calls for boycott.

We proceed as follows. Section 2 describes the history and background of German-Greek conflict since 1941, and it introduces our data sources. Section 3 summarizes the main results; section 4 presents robustness checks and extensions. Section 5 concludes.

## 2.2 Historical background and data description

In this section, we briefly summarize the history of German-Greek conflict during World War II, as well as the period of crisis after the outbreak of the sovereign debt crisis in 2010. We also introduce our data on car registrations, and describe how our measures of news-based conflict and German reprisals are constructed.

### 2.2.1 German retribution measures in Greece during WWII

Following a six-month Greco-Italian war and a German military campaign that lasted less than a month, Greece was occupied by Axis forces in May 1941. The country was divided into three occupation zones. The largest one was administered by Italy. Germany occupied a smaller part of the territories, but controlled crucial locations including Athens, Thessaloniki and Crete. Bulgaria administered a relatively small part of the country close to its own borders. From the beginning, the civilian population suffered under the harsh measures of expropriation and plunder that followed the occupation. The German armed forces requisitioned

[4]Here, our findings echo those of Michaels and Zhi (2010), Hong et al. (2011), and Pandya and Venkatesan (forthcoming). In contrast, the main effect in Fisman et al. (2013) is that the stock-market valuation of large firms mainly selling to the public sector declined in peirods of conflict.

foodstuffs on a vast scale, leading to a major famine during the winter 1941–1942. An estimated 300,000 people died, and the period still survives in Greek collective memory (Hionidou, 2006).

Throughout Eastern Europe, the German armed forces targeted the civilian population in a bid to deal with partisan attacks. Shooting of potentially uninvolved civilians as a reprisal measure in areas of armed resistance was first authorized in April 1941 in Yugoslavia (Mazower, 1995). It was standard practice in anti-partisan operations in Russia. The OKW[5] early on laid down precise quotas on how German troops should spread fear and terror throughout occupied territory — 100 civilians were to be shot for each German solider killed in a partisan attack, 50 for each wounded man, etc. Following the capture of Crete — involving heavy losses by the *Wehrmacht* in the face of determined local and Allied resistance — reprisal measures were also used (Nessou, 2009). General Student, the temporary commander of Crete after the German invasion of the island, instructed his forces to "leave aside all formalities and deliberately dispense with special courts", since these were not fit for "murderers and beasts".

The town of Kondomari in Crete was the first to witness a mass execution of civilians by the Germans on Greek soil: 19 people were shot on June 2, 1941, in retaliation for the death of a German officer in the town's vicinity (Meyer, 2002). Both mass shootings and the burning down of villages became common. Until 1944, an estimated 2–3,000 Greek civilians were executed by the German armed forces on Crete alone, and 1,600 (out of a total of 6,500) towns and villages were destroyed (Nessou, 2009, p. 204).

After Italy surrendered to the Allies in September 1943, the Italian-occupied zone of Greece was taken over by German forces. The Italian troops had been notably lax in their attempts to subdue local partisan groups ("andartes"). Following the expansion of German-occupied territory, conflict between guerrilla groups (mostly the Communist-led ELAS) and the *Wehrmacht* intensified. The German forces increasingly used terror tactics against the local population. Partisan attacks were often followed by indiscriminate shootings of civilians and the destruction of every village in a certain radius from the attack. For example, the town of Mousiotitsa in the northwestern part of Greece had 153 of its inhabitants

[5]Oberkommando der Wehrmacht — Central Command of German Armed Forces.

killed, including women and children, on July 25th 1943. Another 15 localities in the area were destroyed by the Germans (Nessou, 2009). The massacre was part of a mopping-up operation in response to the killing of a German officer in the town of Zita. Similarly, the entire male population of the town of Kalavryta in the Peloponnese was shot, along with inhabitants of several neighboring towns (with a total number of 696 dead) after guerrillas abducted and killed soldiers of the 117th Gebirgsjäger Division in October 1943. One of the last massacres of civilians before the end of the occupation occurred in Distomo, near Delphi. In total, 218 people, including infants, were killed by a Waffen-SS unit on June 10th 1944. Post-war reports of the Ministry of Reconstruction estimate that the total number of dead in Greece may have been as high as 30,000 (Doxiadis, 1947).

Memories of Nazi massacres during the occupation are not far from the surface in Greece today. Family members of the victims of Distomo have sued for reparation payments, taking their case to the German courts and to the International Court of Human Rights. Despite the fact that the Constitutional Court in Germany dismissed the case in 2003, it was recently revived when an Italian court awarded the descendants of the victims a property belonging to a German NGO in Italy. The case reached the International Court in 2012 in the middle of the Greek sovereign crisis, and featured prominently in the Greek press.[6]

Data on towns that suffered reprisal measures by the Wehrmacht during the German occupation of Greece come from Presidential Decrees no. 2130 (1993), 399 (1998), 99 (2000), 40 (2004) and 140 (2005). These decrees designate a number of municipalities and communes throughout the country as "martyred towns". Localities in this category were determined — by a committee created in 1997 by the Ministry of Internal Affairs and Public Administration — to have suffered material and human losses in the period 1941–1944, and fulfil one of the following criteria:

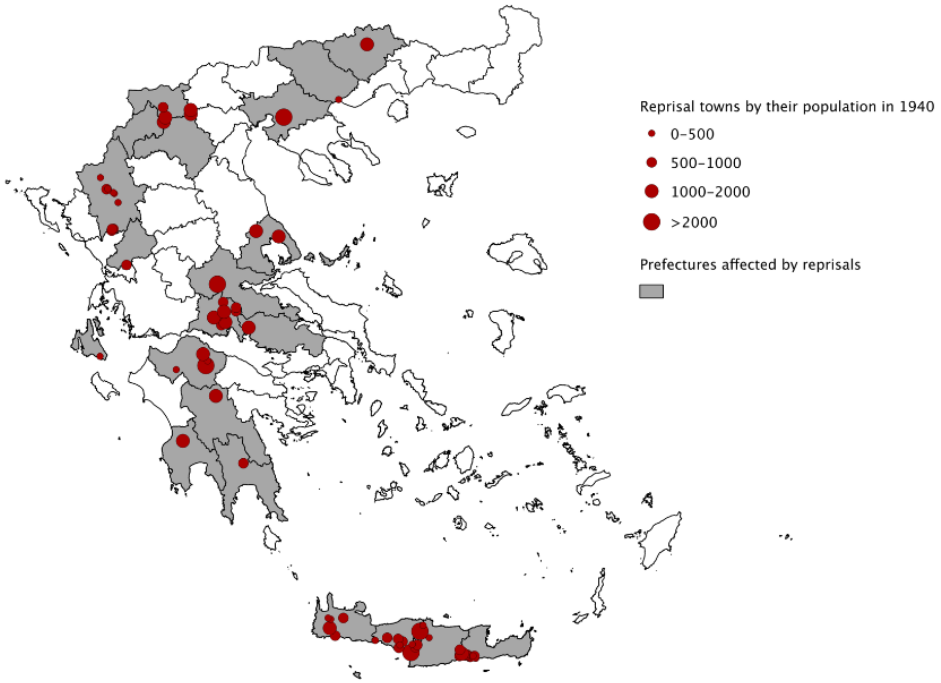
1. Complete destruction of housing stock by arson, bombings or explosions.

2. Loss of 10% of the period's total population by individual or mass executions, as well as by other causes, e.g. blind shootings of civilians.

[6] "The government in the Hague for Distomo", Kathimerini, 13 January 2011.

3. Destruction of housing stock that approaches 80% of the total and loss of population that approaches 10% of the total, also taking into account the absolute magnitudes of the losses.

Figure 2.2: Map of towns affected by German reprisals, 1941–1944




**Notes:** "Martyred towns", as characterized by presidential decrees no. 399 (1998), 99 (2000), 40 (2004) and 140 (2005). Population data from the 1940 Greek Census.

This list of locations includes a total of 78 towns, from which we exclude the following: Doxato, Drama, Choristi (under Bulgarian occupation and destroyed by the Bulgarians) and Domeniko, Tsaritsani, Nea Agchialos (destroyed by the Italians). Figure 2.2 depicts the regional distribution of affected localities. All places on the list of martyred towns suffered due to German reprisals; they were not destroyed by bombing during the war or during the invasion. 54 out of 72 witnessed mass executions of civilians; the rest were burnt to the ground in retaliation for an insurgency attack against German armed forces in the vicinity (Nessou, 2009). Since data on car registrations, our main dependent variable, are not available at

a lower level of aggregation than the prefecture, we construct a prefecture-level index of exposure to German reprisals, in the form of the share of the prefecture's total population in 1940 that lived in "martyred" localities.

### 2.2.2 German-Greek relations during the Greek crisis

The Greek sovereign debt crisis began to unfold in late 2009, when revised budget deficit figures revealed the country's dire financial situation. This led to successive downgrades of its credit rating. Eventually, with debt markets all but closed to the Greek state, an EU bailout became inevitable. From the beginning, the German government was sceptical of a financial rescue for Greece.[7] It finally agreed to the bailout in exchange for harsh austerity measures. From the onset of the crisis, Greek public opinion saw Germany as the instigator of foreign-imposed austerity. The reaction was immediate and intense: In February 2010, the Greek Consumers Association called for a boycott on German products — explicitly highlighting the importance of cars — and instructed consumers on how to identify the national origin of a good by its barcode.

Animosity was further aggravated by incendiary articles in the popular press. German newspapers routinely portrayed Greeks as notorious and lazy cheaters living it up at the expense of the German taxpayer.[8] A German weekly featured Aphrodite making a rude gesture on the cover page; German populist politicians suggested that Greece should sell some of its islands to repay its debts.[9] As the Greek economy contracted and unemployment surged amid severe austerity measures, anti-German feelings in Greece deepened. Greek politicians openly referred to the German special envoy as a "military commander". In early 2012, Greek president Karolos Papoulias publicly complained that the entire country was being insulted by the German finance minister Wolfgang Schäuble. During the 2012 visit of German chancellor Angela Merkel to Athens, thousands of people demonstrated in the streets of Athens.

Much of the criticism of German policy in Greece after 2010 used references

[7] "German "no" to facilitating the repayment of the 110 billion euros", Kathimerini, 13 October 2010.

[8] "Die Griechenland-Pleite", Focus Magazine, Nr. 8, 2010.

[9] "Verkauft doch eure Inseln, ihr Pleite-Griechen", Bild, 27 October 2010.

to the German occupation during World War II, and employed Nazi-era symbols to protest against the way Greece was being treated. Mentions of war crimes and unpaid German reparations became much more frequent in the press. Former foreign minister Stavros Dimas, addressing the Greek parliament in March 2011, reminded everyone that Greece never waived its right to claim reparations, and that a forced loan taken out by Germany during the occupation had not been repaid.[10] Populist newspapers printed swastikas surrounded by the stars of the European union to symbolize that EU policy was as harsh as Nazi occupation.

An article by the English *Daily Telegraph* illustrates the way in which past conflict suddenly came to matter for Greeks after the start of the debt crisis. In the issue of February 11, 2012, the Telegraph profiled the life of Eleftherios Basdekis, who spent his "entire life beneath a German cloud". A survivor of the Distomo massacre, he eventually built a successful trucking business, which went bankrupt after the start of the crisis. The article also cited a mother from Distomo saying that she "hated Germany", that Angela Merkel was "a monster", and that the Germans "killed Distomo; they stole our gold; they belittle Greece." A bar owner is quoted as saying "five years ago, no one had any problem with Germany. But now people are getting upset. The Germans say we are lazy, which is not fair".

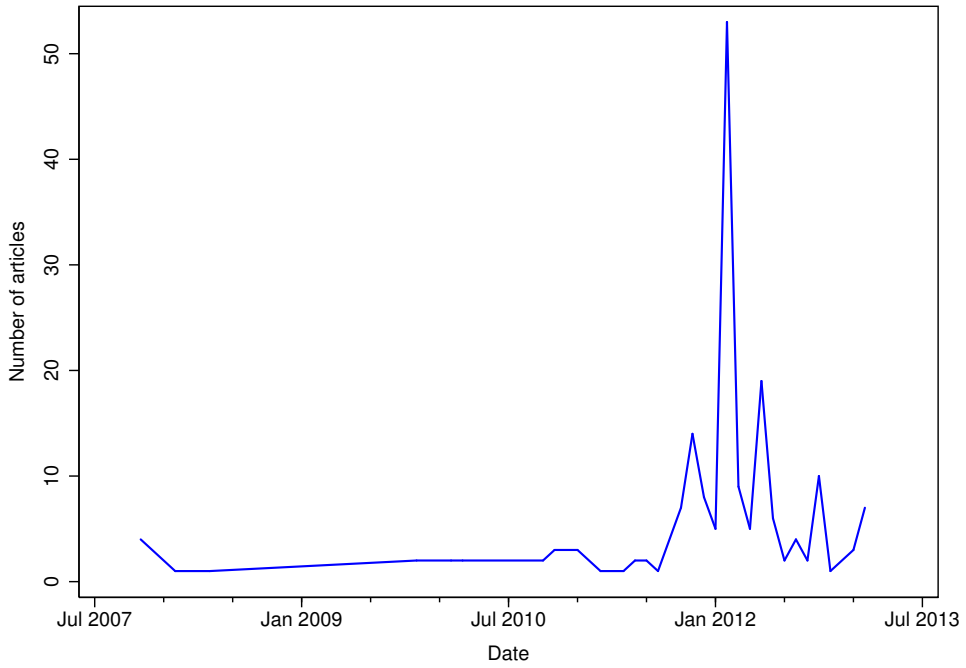
As the Telegraph article illustrates, hatred of Germans suddenly resurfaced after the outbreak of the debt crisis. In addition, Greeks from towns destroyed after 1941 often interpreted recent acrimony in the light of earlier conflict. Our hypothesis is that the persistence of collective memories of the German occupation is stronger in areas of Greece that actually fell victim to German atrocities, and that the revival of these memories during specific conflict events manifests itself through consumer decisions.

In order to identify months of heightened conflict in German-Greek relations during the euro crisis, we use a measure based on the frequency of newspaper references to political tension between the two countries. Our approach can be illustrated with an example from Lexis-Nexis. Figure 2.3 shows the frequency of the joint occurrence of the words "anti-German" and "Greece" in articles appearing in international news media. For the years before 2009, the word pair is virtually

[10] "The issue of German reparations is open but...", Kathimerini, 28 March 2012.

inexistent. Thereafter, the frequency count increases sharply, reaching peaks in 2010 and 2011.

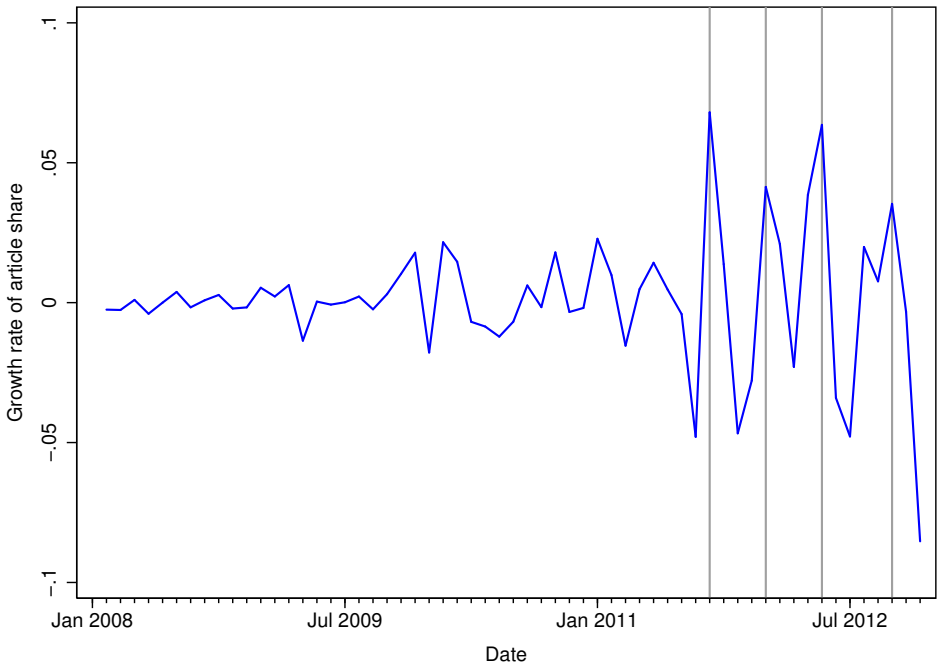
Figure 2.3: Number of international news articles referring to German-Greek conflict




**Notes:** Number of international newspaper articles in English that mention the words "anti-German" and "Greece". Source: LexisNexis.

To obtain a measure of perceived German-Greek conflict within Greece, we compute the frequency of conflict-related articles in a leading Greek newspaper, Kathimerini. It is the largest daily newspaper by circulation during the period 2008–2012; its entire archive of articles is digitized and available electronically. Our database consists of a total of 64,854 articles published in the sections on "Greece", "Politics" and "Economy". We compute the monthly share of articles related to German-Greek conflict using an approach similar to Baker et al. (2013). Starting from a human audited sample of articles, this procedure selects the terms that jointly best identify articles referring to German-Greek conflict. The resulting set of terms used to identify the relevant articles is the one that performs closest to the "gold standard" of human readings.

Figure 2.4: News-based index of German-Greek conflict




**Notes:** The figure depicts the growth rate in the monthly share of *Kathimerini* articles relating to German-Greek conflict (for details, cf. the data section and data appendix). The vertical reference lines indicate a local peak in the series that is larger than one standard deviation.

Using this procedure, we classify an article as conflict-related if it contains the stem "german-" and at least one of the words in the set {memorandum, troika, haircut, Distomo, default, austerity, Schäuble}. This gives us a monthly count of conflict-related articles, which we normalize by the total number of articles Kathimerini published in the month. More details on the term-selection algorithm are given in the Data Appendix.

Table 2.1: Chronology of Greek crisis and German-Greek conflict month identification

| Date | Event Description |
|---|---|
| February 2010 | Deal with EU/ECB/IMF on bailout and first austerity package |
| | Cover of Focus magazine with title "Cheaters in the Euro-family" displays Aphrodite of Milos making rude gesture |
| | Greek Consumer Association calls consumers to boycott German products |
| October 2010 | Germany refuses time extension for repayment of Greek loans |
| | Brussels EU summit sees acceptance of German-engineered new bailout mechanism |
| | Merkel-Sarkozy suggestion that indebted countries are stripped of voting rights causes angry responses from Greek politicians |
| January 2011 | Case of German reparations for WWII crimes on trial in den Haag |
| May 2011 | Discussions for new round of austerity measures (Midterm plan) |
| | Merkel comment on "lazy Southerners" at political rally attracts large attention in Greek press |
| **September 2011** | Eurogroup meeting in Brussels pressures Greece to go through with reforms |
| | Greek government implements new measures including firings and pension cuts |
| | German Finance minister says it is the Greeks' decision whether they want to leave the euro, while FDP members suggest a Greek orderly default |
| October 2011 | New austerity package is voted amidst severe rioting |
| | 50% "haircut" of Greek debt takes place |
| **January 2012** | EU commission rejects "German plan" for Greece to relinquish budget control |
| February 2012 | Parliament approves new austerity plan |
| | International court rules in favor of Germany in trial regarding WWII reparations |
| | Greek President Carolos Papoulias declares "I cannot accept Mr Schäuble insulting my country" |
| **May 2012** | Month of Greek national elections |
| | German ministers remind Greece that measures have to be carried through irrespective of government outcome, if the country wants to remain in the Eurozone |

Dates in bold are turning points in the growth series of the conflict-related article share of Kathimerini.

We identify event months as those showing a large jump in the share of articles devoted to German-Greek conflict. This is based on the turning points in the growth rate series; these are defined as local maxima ($y_t > y_{t-1}$ and $y_t > y_{t+1}$) that are larger than one standard deviation. Because of delays between purchasing decisions and car registrations, we define as a conflict event each turning point in the news-based series and the subsequent month.[11] The identified turning points are depicted in Figure 2.4. Most of them coincide with major episodes in the Greek debt crisis. Table 2.1 highlights these episodes and offers a brief chronology of German-Greek relations during the crisis.

### 2.2.3 Car registrations

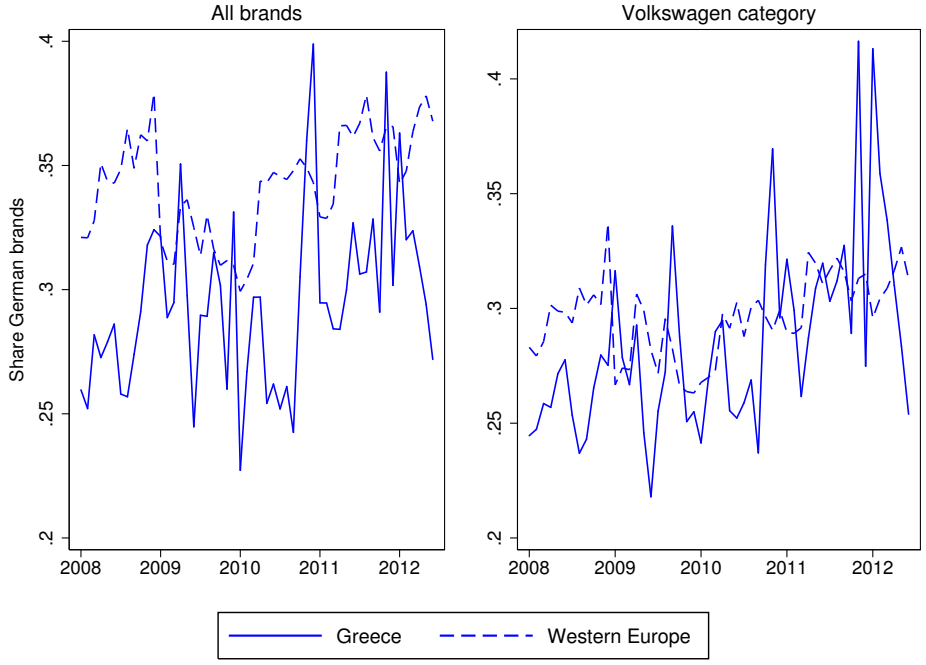
The Greek Ministry of Transport and Communications collects data on registrations of new passenger vehicles. These are disseminated by the Hellenic Statistical Authority (ELSTAT). We have access to monthly data on the number of new passenger vehicles registered in each prefecture for the period from January 2008 to August 2012, by manufacturing plant.

Aggregate car sales slumped after the start of the financial crisis. Annual unit sales had totalled close to 180,000 before 2007. By 2011, with the Greek economy contracting rapidly, car sales fell to barely 60,000 per annum, a decline by almost two-thirds over four years. Analysing sales trends of cars in Greece is complicated by the fact that German car manufacturers performed strongly over the last decade. World-wide, the share of German brands has been rising. This partly reflects the recovery of Volkswagen sales and the significant decline in Toyota's market share.[12] Figure 2.5 compares the share of German cars in the Greek car market with that in the European market overall. The overall trend is broadly similar.

[11] According to Cosumer Service Centers and the Ministry of Transport, a registration can take from 3 days to 2 months, depending on whether the car has already passed through customs at the time of the purchase.

[12] "VW conquers the world", The Economist, 7 July 2012.

Figure 2.5: Share of German cars, Greece vs Western Europe




**Notes:** Monthly share of German cars in the total number of new cars registered. Western Europe includes Austria, Belgium, Denmark, Finland, France, Greece, Germany, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden, the UK, Iceland, Norway and Switzerland. The Volkswagen category includes Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda and Toyota. Data from ELSTAT and Association Auxiliaire de l'Automobile (AAA).

The raw data from ELSTAT does not contain information on the brand of registered vehicles. However, ELSTAT provides a correspondence list that allows us to match production plants to car manufacturers. This correspondence does not always distinguish between brands produced by the same manufacturer. This is true for the Daimler group, producer of both Smart and Mercedes vehicles, and for the Fiat group, which also produces Alfa Romeo and Lancia. Despite this issue, we are able to distinguish German from non-German brands in our sample; the former include Volkswagen, Opel, Audi, BMW, Porsche and the brands of the Daimler group.[13] For our purposes, a car's "nationality" is not determined

[13]Data on vehicle registrations are available from January 2004 on, but we are unable to distinguish German brands in the earlier sample, due to the fact that Daimler was also owner and producer of Chryslers.

by ultimate ownership of the company, but the place of manufacture of (most) cars – we count Seat as a Spanish car maker despite the fact that it is owned by Volkswagen.[14]

Summary statistics for the monthly sales of brands in our sample are given in Table 2.2. Toyota is the firm with the highest average sales number, followed by Opel and Volkswagen. At the opposite end of the spectrum are small luxury car makers such as Ferrari and Maserati, with average sales of only one car per month. To compare like with like, we exclude small manufacturers with less than 10 vehicles sold in the total period 2008–2012.

Table 2.2: Monthly car registrations in Greece

| Manufacturer | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| AUDI | 389 | 277 | 99 | 1543 |
| BMW | 455 | 318 | 90 | 1334 |
| BENTLEY | 1 | 1 | 0 | 5 |
| CHANGAN | 3 | 3 | 0 | 15 |
| CHEVROLET | 230 | 188 | 5 | 698 |
| CHRYSLER | 91 | 94 | 0 | 361 |
| CITROEN | 493 | 296 | 87 | 1358 |
| DACIA | 41 | 30 | 0 | 99 |
| DAIHATSU | 243 | 232 | 4 | 783 |
| DAIMLER | 645 | 472 | 51 | 1785 |
| FERRARI | 1 | 2 | 0 | 7 |
| FIAT[a] | 953 | 488 | 269 | 2513 |
| FORD | 897 | 506 | 216 | 2087 |
| GENERAL MOTORS | 25 | 32 | 0 | 129 |
| HONDA | 281 | 232 | 36 | 1025 |
| HYUNDAI | 841 | 625 | 133 | 2524 |
| JAGUAR | 7 | 11 | 0 | 48 |
| JIANGLING | 0 | 0 | 0 | 2 |

[14]To the extent that Seat is actually perceived as German, we will understate the shift away from German cars, biasing our results downwards.

Table 2.2: Monthly car registrations in Greece (cont.)

| Manufacturer | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| KIA | 332 | 285 | 33 | 1636 |
| LADA | 10 | 13 | 0 | 52 |
| LAMBORGHINI | 0 | 1 | 0 | 3 |
| LAND ROVER | 15 | 20 | 0 | 79 |
| LOTUS | 0 | 1 | 0 | 2 |
| MASERATI | 1 | 1 | 0 | 4 |
| MAZDA | 285 | 311 | 2 | 1081 |
| MITSUBISHI | 221 | 169 | 25 | 675 |
| NISSAN | 685 | 444 | 86 | 1998 |
| OPEL | 1202 | 613 | 384 | 2806 |
| PEUGEOT | 504 | 335 | 97 | 1319 |
| PORSCHE | 19 | 19 | 0 | 67 |
| RENAULT | 215 | 136 | 65 | 723 |
| SAAB | 36 | 40 | 0 | 194 |
| SEAT | 423 | 308 | 45 | 1227 |
| SHUANGHUAN AUTO | 4 | 5 | 0 | 18 |
| SKODA | 511 | 294 | 138 | 1439 |
| SSANGYONG | 10 | 13 | 0 | 57 |
| SUBARU | 71 | 66 | 0 | 251 |
| SUZUKI | 695 | 532 | 91 | 1896 |
| TOYOTA | 1452 | 888 | 284 | 3909 |
| VOLKSWAGEN | 1182 | 622 | 220 | 2436 |
| VOLVO | 136 | 73 | 30 | 319 |

[a] Includes Alfa Romeo and Lancia.

**Source:** ELSTAT Data for the period January 2008 to August 2012.

Many German cars are luxury products. These suffered greater declines in sales as a result of the crisis. To avoid biasing our results upwards, we perform key parts of our empirical analysis for the "Volkswagen category" only. This is composed

of a group of manufacturers focusing on compact vehicles, and mid-sized family cars. This category includes the following brands: Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda, Toyota.[15]

### 2.2.4 Data descriptives, control variables and balancedness

Our dataset contains information on 51 prefectures over the period January 2008 to August 2012. The main features of the data are summarized in Table 2.3. Massacres during the German occupation occurred in 21 out of 51 prefectures, equivalent to 41% of the sample. The share of the (pre-war) population living in towns and villages later destroyed serves as our main explanatory variable. On average, a little more than one percent of Greeks in 1940 were so affected; Fokida, on the Northern shore of the Gulf of Corinth, is the worst-affected prefecture with a share of 12%. The average prefecture in our sample saw monthly sales of 267 cars during the period; sales were as low as zero in some prefectures, and could reach as many as 16,365 cars per month. The share of German cars sold was on average 28%; especially in the smaller prefectures, the share fluctuates strongly from month to month, and in some cases, it can reach either 0 or 100%. Finally, we classify six (out of 56) months in our sample as "conflict months".

[15]Including other brands in this category (Daewoo, Daihatsu, Isuzu, Kia, Mitsubishi, Subaru, Audi) does not significantly alter the results.

Table 2.3: Summary statistics of main variables

| Variable | Mean | S.D. | Min | Max | N |
|---|---|---|---|---|---|
| Cross-section | | | | | |
| Massacre in prefecture (0/1) | 0.412 | 0.497 | 0 | 1 | 51 |
| Share population in reprisal towns | 1.082 | 2.415 | 0 | 11.99 | 51 |
| Panel | | | | | |
| Total car sales | 266.8 | 1,138 | 0 | 16,361 | 2,856 |
| Share german cars (all brands) | 0.251 | 0.117 | 0 | 1 | 2847 |
| Share german cars (VW category) | 0.278 | 0.138 | 0 | 1 | 2,837 |
| Conflict month (0/1) | 0.107 | 0.309 | 0 | 1 | 2,856 |

Our main control variables come from the 2001 Greek Census, the latest one for which data is available at the prefecture level. It includes information on population size, employment in agriculture and industry, the share of civil servants, education, and the unemployment rate. Table 2.4 compares these variables for prefectures with and without reprisals. Overall, there are few meaningful differences between the two groups — the share of employment in agriculture is similar, as is the proportion of the labor force in industry. The share of civil servants, a group that was hit hard by the crisis, is almost identical. Education levels are also comparable — the largest difference is for the share of citizens with secondary education (19% in reprisal prefectures, 17% in the others). Unemployment rates differed by one percentage point, with a baseline of 12%. Unfortunately, few variables are available at an annual frequency for the later years of the sample, in which months of German-Greek conflict are concentrated.

Table 2.4: Balancedness

| Variable | All | Non-reprisal | Reprisal | Difference |
|---|---|---|---|---|
| Population | 428,711 | 481,133 | 353,823 | 127,309 |
| | (1,093,791) | (1,385,816) | (442,978) | (270,851) |
| Share employed in agriculture | 0.264 | 0.277 | 0.245 | 0.0326 |
| | (0.107) | (0.110) | (0.102) | (0.0300) |
| Share employed in industry | 0.219 | 0.211 | 0.230 | −0.0189 |
| | (0.058) | (0.045) | (0.073) | (0.0179) |
| Share civil servants | 0.014 | 0.014 | 0.013 | 0.001 |
| | (0.004) | (0.004) | (0.003) | (0.001) |
| Share secondary education | 0.179 | 0.170 | 0.192 | −0.0212** |
| | (0.031) | (0.030) | (0.030) | (0.009) |
| Share higher education | 0.110 | 0.107 | 0.114 | −0.0075 |
| | (0.024) | (0.024) | (0.023) | (0.007) |
| Unemployment rate | 0.122 | 0.119 | 0.126 | −0.007 |
| | (0.029) | (0.025) | (0.035) | (0.009) |
| Population in 1940 | 146,868 | 147,034 | 146,637 | 397 |
| | (177,369) | (223,671) | (83,881) | (45,389) |
| Share of 1940 population in reprisal towns | 1.082 | - | 2.627 | - |
| | (2.415) | | (3.213) | |
| Share destroyed housing | 0.137 | 0.134 | 0.142 | −0.008 |
| | (0.122) | (0.143) | (0.087) | (0.033) |
| Share seats to communists in 1936 | 0.028 | 0.026 | 0.031 | −0.0047 |
| | (0.057) | (0.056) | (0.060) | (0.0167) |
| Ruggedness | 248.92 | 232.01 | 273.08 | −41.07* |
| | (77.25) | (79.66) | (68.40) | (20.84) |
| Average distance from 1940 road | 15.31 | 22.37 | 5.23 | 17.13** |
| | (35.42) | (45.09) | (2.52) | (8.25) |
| Average distance from 1940 railway line | 78.17 | 73.54 | 84.77 | −11.23 |
| | (92.13) | (83.06) | (105.53) | (27.57) |
| N | 51 | 30 | 21 | |

Reprisal prefectures have at least one "martyred" town. Source: 2001 and 1940 Greek Census, Hellenic Subministry of Reconstruction and Hellenic Parliament, Registry of Parliament Members.

Except for the share of population with secondary education (which is higher in reprisal prefectures) there are no significant differences between reprisal and non-reprisal prefectures. It is more plausible that the location of partisan attacks and subsequent retaliation by German troops was affected by geography. Ruggedness is also correlated with reprisals. More rugged terrain provided cover for the partisans, who had most of their operational bases in the mountains of central and northern Greece. Distance from a main road is also (negatively) correlated with reprisals. This reflects the German tactic of punishing villages in the vicinity of a partisan attack, many of which occurred near roads, and bridges. Though roads seem to a be predictor of reprisals, railways are not. Finally, the share of seats that each prefecture allocated to the Communist Party in the 1936 parliamentary elections is very similar between reprisal and non-reprisal prefectures. Communist-dominated ELAS was the main guerilla group during the occupation, and its activities might have been more welcome in prefectures that showed more communist support. This could be correlated to both the locations of reprisals and to a general reactionary stance in certain prefectures that persists until today. However, ideological preferences are not correlated with the location of military action. We will control for the above variables in all our empirical specifications.

## 2.3 Empirical analysis

In this section, we present our main empirical result — the dramatic decline in German car sales during the German-Greek crisis in prefectures affected by World War II massacres, compared with sales in other areas. We first present our results by using simple conditional averages before proceeding to panel regressions. The robustness section shows that our conclusions are not affected if we use alternative measures of German-Greek conflict, or of the scale of German atrocities. Finally, we show that memories of German war crimes had bigger adverse effects on the market share of German cars in prefectures where the population joined boycott groups on Facebook.

### 2.3.1 Baseline results

We first establish the extent to which German car sales fell in times of German-Greek political conflict — and how much larger this decline was in areas affected by war crimes committed after 1941. As a first step, we perform a difference-in-differences tabulation of changes in market shares for German cars, in crisis and non-crisis months, for reprisal and non-reprisal prefectures. To adjust for seasonal effects, we compare the share of German cars in each month with sales 12 months earlier. Table 2.5 presents the results. In panel A, we analyse the shift for all German brands; in panel B, for the Volkswagen category. In prefectures without reprisals, the gain in market share for German cars was 0.35 percentage points (VW-category: 0.7) lower in crisis months than in normal times. In contrast, in prefectures that saw German reprisals, the relative decline is much sharper — 3.84 percentage points (6.79 in the VW category). Overall, the difference-in-difference estimator suggests an effect of 3.5% (VW-category: 6.1%) — a marked shift over a period of a few months.

Next, we establish the differential effect of an event month on the market share of German brands by estimating the following specification:

$$y_{jt} = \alpha + \lambda_t + \beta_1 C_t + \beta_2 D_j + \gamma C_t * D_j + \mathbf{X}_j \delta + \epsilon_{jt} \tag{2.1}$$

where $y_{jt}$ is the 12-month difference of the share of vehicles of German manufacturers registered in prefecture $j$ at time $t$, $\lambda_t$ are year fixed effects, $C_t$ is a dummy for a conflict month, $D_j$ is the share of the prefecture's 1940 population that lived in towns affected by German reprisals, and $\mathbf{X}_j$ is a vector of prefecture controls. The empirical model amounts to a difference-in-differences strategy, comparing the share of German brands between prefectures with and without a past history of German reprisals, in months of conflict relative to months without a conflict event. The only difference from classical DID is that the treatment variable $D_j$ is not a dummy, but a continuous index proxying for exposure to reprisals. We are principally interested in the sign and magnitude of the interaction coefficient $\gamma$.

Table 2.5: Change in the average share of German cars

| | Panel A: All brands | | |
|---|---|---|---|
| | Conflict months | Other months | Difference |
| | (1) | (2) | (3) |
| Reprisal prefectures | −0.0192 | 0.0193 | −0.0384*** |
| | (0.0124) | (0.0049) | (0.0133) |
| Non-reprisal prefectures | 0.0155 | 0.0189 | −0.0035 |
| | (0.0104) | (0.0041) | (0.0112) |
| Difference | −0.0346** | 0.0035 | −0.0350** |
| | (0.0162) | (0.0112) | (0.0174) |
| | Panel B: VW category | | |
| | Conflict months | Other months | Difference |
| | (1) | (2) | (3) |
| Reprisal prefectures | −0.0338 | 0.0341 | −0.0679*** |
| | (0.0145) | (0.0057) | (0.0155) |
| Non-reprisal prefectures | 0.0220 | 0.0291 | −0.0070 |
| | (0.0122) | (0.0048) | (0.0131) |
| Difference | −0.0559*** | 0.0050 | −0.0608*** |
| | (0.0189) | (0.0075) | (0.0204) |

**Notes:** The share of German cars is seasonally adjusted and expressed as the difference between month t and t-12. Reprisal prefectures have at least one "martyred" town. The Volkswagen category includes the following brands: Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda, Toyota. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Standard errors in parentheses.

Table 2.6, Panel A reports baseline estimates for all car brands. Columns [1] and [2] restrict the sample to prefectures with and without reprisals respectively. The estimated effect of a conflict month on the German market share is negative in both cases. However, for regions that suffered reprisals, the estimate is almost 8 times larger than for non-reprisal areas. To test if the difference is statistically

significant, we interact the conflict month with the measure of massacre intensity. The results in column [3] show that the effect of conflict is systematically and significantly larger in areas that suffered German reprisals. This result is unaffected when we add our set of standard controls. The results are also quantitatively important: the estimated interaction coefficient implies that a one standard deviation increase in the share of affected population in reprisal prefectures leads to an additional drop of 4.2 percentage points in the share of German cars.

Column [5] adds interactions of the baseline controls with the conflict month dummy to the regression setup. Since we are not able to control for prefecture observables that vary at a monthly frequency, this approach allows us to proxy for effects of a conflict month that differ among prefectures and depend on observable characteristics other than the share of population affected by reprisals. Adding these interactions does not affect the results: the coefficient of the interaction implies that a standard deviation increase in the share of the population affected by reprisals leads to a drop of 4.3 percentage points in the German car share in conflict months. Column [6] estimates an alternative specification, controlling for prefecture fixed effects and prefecture-year interactions. Including these actually increases the size of the coefficient for the interaction term.

Panel B of Table 2.6 repeats the exercise for cars in the Volkswagen category. After removing luxury cars from the sample, there is an even larger (differential) drop in the German car share in months of conflict. This is contrary to the intuitive prediction that sales of luxury cars should have slumped more in months when there was, in effect, bad news about the future of the Greek economy. The average difference between reprisal and non-reprisal prefectures for the Volkswagen sample grows to almost 4 percentage points. The implied difference between reprisal and non-reprisal prefectures corresponds to a drop equivalent to 10.7% of the average market share of German brands in the Volkswagen category in the period 2008–2012. This is an economically large shift, particularly for a durable good and over a short period of time.

Table 2.6: Share of German cars, baseline

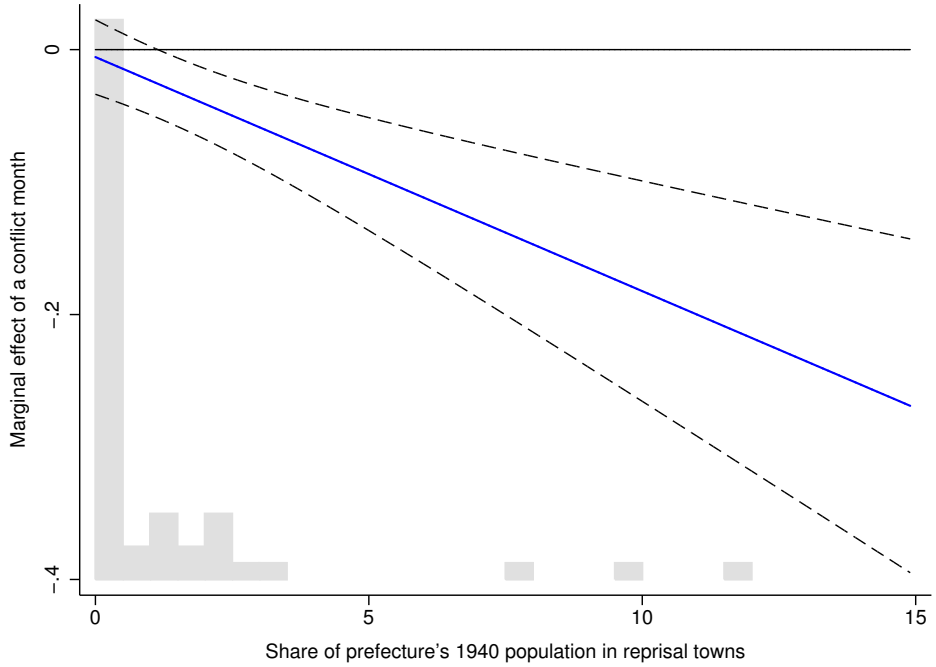
| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Panel A: All brands | | | | | | |
| Conflict month | −0.0405** | −0.00508 | −0.00554 | −0.00552 | 0.152 | 0.203 |
| | (0.0189) | (0.0183) | (0.0112) | (0.0113) | (0.290) | (0.229) |
| Share pop. affected | | | 0.00105 | 0.000573 | 0.000691 | |
| | | | (0.00131) | (0.000969) | (0.000961) | |
| Conflict month* | | | −0.0132*** | −0.0132*** | −0.0141*** | −0.0157** |
| Share pop. affected | | | (0.00279) | (0.00280) | (0.00397) | (0.00606) |
| Observations | 924 | 1309 | 2233 | 2233 | 2233 | 2233 |
| R-squared | 0.0697 | 0.0362 | 0.0431 | 0.0487 | 0.0508 | 0.159 |
| Panel B: Volkswagen category | | | | | | |
| Conflict month | −0.0555** | −0.00819 | −0.00567 | −0.00571 | 0.171 | 0.282 |
| | (0.0253) | (0.0213) | (0.0143) | (0.0143) | (0.412) | (0.396) |
| Share pop. affected | | | 0.00261 | 0.00196 | 0.00225 | |
| | | | (0.00168) | (0.00157) | (0.00148) | |
| Conflict month* | | | −0.0177*** | −0.0177*** | −0.0199*** | −0.0181*** |
| Share pop. affected | | | (0.00460) | (0.00461) | (0.00573) | (0.00643) |
| Observations | 923 | 1298 | 2221 | 2221 | 2221 | 2221 |
| R-squared | 0.0611 | 0.0373 | 0.0372 | 0.0429 | 0.0475 | 0.149 |
| Controls | No | No | No | Yes | Yes | No |
| Prefecture×Year | No | No | No | No | No | Yes |
| Controls× | No | No | No | No | Yes | Yes |
| Conflict month | | | | | | |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the 12-month difference in the monthly German share of new cars registered in a prefecture. The Volkswagen category includes the following brands: Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda, Toyota. Column [1] is restricted to prefectures with at least one reprisal town. Column [2] is restricted to prefectures wthout reprisals. Columns [3]-[6] use the entire sample. All regressions include year fixed effects. Prefecture controls include log(population) in 2001, share employed in agriculture in 2001, share employed in industry in 2001, share with higher education in 2001, share with secondary education in 2001, unemployment rate in 2001, ruggedness, average distance from the nearest road in 1940 and average distance from the nearest railway line in 1940. Standard errors are clustered at the prefecture level.

The magnitude of the interaction effects is best judged graphically. How high

does the share of the population affected by German reprisals have to be for effects to become large and significant? Figure 2.6 plots the marginal effect of a conflict month for different values of the share of the population living in locations that suffered massacres. As is readily apparent, the implied effect is negative from the start; it becomes significant from a share of 3%.

Figure 2.6: Effect of a conflict month on the market share of German cars




**Notes:** Estimated marginal effect and 95% confidence interval from the regression reported in column [3] of Table 2.6, Panel B. The histogram plots the distribution of prefectures by share of their 1940 population affected by reprisals.

### 2.3.2 Robustness

In this section, we stratify our dataset by observables in order to examine when the effect we find is most pronounced. We also examine the robustness of our results to using alternative measures of conflict and reprisals.

#### 2.3.2.1 Sample stratification

In Table 2.7, we examine if our estimated effect of conflict on the German market share is broadly similar when we subdivide the sample according to the structure of employment, education levels, unemployment levels, and a measure of political preferences. Both above and below median subsamples show a drop in the German share and a difference between reprisal and non-reprisal prefectures in most cases. We find stronger and more tightly estimated interaction effects in areas that are less agricultural and more industrial, with lower education, less unemployment in 2011 and a lower increase in unemployment between 2008 and 2011. Areas with more votes for *Golden Dawn* (a neo-Fascist party campaigning on a strongly nationalistic and xenophobic platform) show both a larger drop in the German share and a bigger difference in the drop by reprisal status. We also find stronger, more tightly estimated effects for areas with slower population growth after 1940. Since most of the differences in population growth reflect migration, this suggests that the grip of the past in crisis times is strongest in areas with low in-migration (or net outflows); areas that gained population (as a result of migration from the rest of the country) show no clear interaction effect.[16] This is not due to over-representation of reprisal prefectures in the part of the sample with lower population growth: 10 out of 21 reprisal prefectures had an above median change in population from 1940 to 2000.

Finally, we examine if areas that supported the Communist Party before 1940 show stronger reactions to German-Greek conflict today. Table 2.7, column [10] points in this direction, with coefficients being larger in the subsample of prefectures that allocated a seat to the communists in the 1936 parliamentary elections. However, the size of the subsample is small and the interaction coefficient — though smaller in magnitude — is only significantly estimated for prefectures that did not support the Communist party.

[16] This finding is similar to the result in Voigtländer and Voth (2012), who show that anti-Semitism in Germany persisted strongly in areas with low population growth.

Table 2.7: Sample subdivision

| | Share agriculture | Share industry | Share higher education | Unemployment 2011 | ΔUnemployment 2008-2011 | Votes to Golden Dawn | ΔPopulation 1940-2000 | Communist seats in 1936 |
|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| | | | Above median | | | | | |
| Conflict month | −0.00160 | −0.0192 | −0.0138 | −0.0341 | −0.0260 | −0.0179 | −0.0211 | −0.0107 |
| | (0.0212) | (0.0186) | (0.0205) | (0.0204) | (0.0199) | (0.0167) | (0.0173) | (0.0358) |
| Conflict month* | −0.0119 | −0.0220*** | −0.00840 | −0.00274 | −0.00236 | −0.0226*** | 0.000403 | −0.0245 |
| Share pop. affected | (0.0270) | (0.00293) | (0.00747) | (0.00965) | (0.00813) | (0.00253) | (0.00581) | (0.0159) |
| Observations | 1122 | 1123 | 1142 | 1254 | 1254 | 1144 | 1144 | 483 |
| R-squared | 0.134 | 0.148 | 0.163 | 0.162 | 0.149 | 0.155 | 0.195 | 0.133 |
| | | | Below median | | | | | |
| Conflict month | −0.0200 | 0.00153 | −0.00800 | 0.0149 | 0.00529 | −0.000257 | −0.00407 | −0.00736 |
| | (0.0199) | (0.0213) | (0.0203) | (0.0205) | (0.0222) | (0.0249) | (0.0244) | (0.0164) |
| Conflict month* | −0.0150** | −0.0101 | −0.0250*** | −0.0212*** | −0.0212*** | −0.0113 | −0.0211*** | −0.0160*** |
| Share pop. affected | (0.00579) | (0.00744) | (0.00347) | (0.00336) | (0.00332) | (0.00749) | (0.00354) | (0.00551) |
| Observations | 1099 | 1098 | 1079 | 967 | 967 | 1077 | 1077 | 1738 |
| R-squared | 0.156 | 0.146 | 0.136 | 0.125 | 0.142 | 0.140 | 0.130 | 0.147 |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the 12-month difference in the monthly German share of new cars registered in a prefecture. The sample is restricted to the Volkswagen category, which includes Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda and Toyota. Each column splits the sample into prefectures with a value equal to or higher than the median of the variable defined in the column heading (upper panel) and prefectures with a value lower than the median (lower panel). In column [8] the upper panel is restricted to prefectures strictly above the median share of communist seats in the 1936 elections, since the median share of communist seats in these elections was zero. All columns include prefecture and year fixed effects and prefecture-year interactions. Standard errors are clustered at the prefecture level.

#### 2.3.2.2 Measures of political conflict

Next, we examine the robustness of our findings with respect to two alternatives – the linearity of the assumed response to news, and the type of news index used. In our baseline specification, we assume that German car sales were affected in a non-linear fashion by news about the German-Greek conflict. Here, we also examine a linear relationship. Columns [1]-[3] of Table 2.8 replace the conflict month dummy with the monthly share of conflict-related Kathimerini articles. Column [1] is our baseline specification with a standard set of controls. The share of Kathimerini articles related to German-Greek conflict is negatively correlated with the German car market share and the interaction coefficient is strongly negative and significant. The coefficient for the share of the population affected is positive, but relatively small. Once we add interactions of the controls with the share of articles, we get an interaction coefficient that is larger in magnitude, but less precisely estimated. Significance is lost when we use prefecture fixed effects and prefecture-year interactions, though the coefficient remains negative and of similar size.

Table 2.8 also examines robustness with respect to using an alternative index of political conflict. The extent to which public reaction precedes or follows the news is debatable. If news outlets cater to the views of their audience, then reporting volume will spike (shortly) after an important event. If, conversely, reporting itself creates the adverse reaction, then the pattern will be reversed. We explore an alternative search-based measure of German-Greek conflict that proxies more for the demand-side of news. We use data on web searches from Google Insights for terms related to sources of tension in German-Greek relations during the period 2008–2012. For a given search term, the frequency index provided by Google Insights is a normalization of the share of total searches represented by the term in a given time and region.[17] We use this index to construct a measure of public interest in German-Greek related issues, based on searches — conducted in the Greek language, in Greece — for the following terms: "Germans", "German reparations" and "Distomo". It is surprising that the vague term "Germans"

[17]Only terms with hits above a certain threshold are considered for the construction of the index. As a result, the index often takes on the value 0 when the search volume for a term is low.

first appears with a non-zero value in the Google index in February 2010, the very month when the first austerity measures were announced and the consumer boycott started.

Table 2.8: Alternative measures of German-Greek conflict

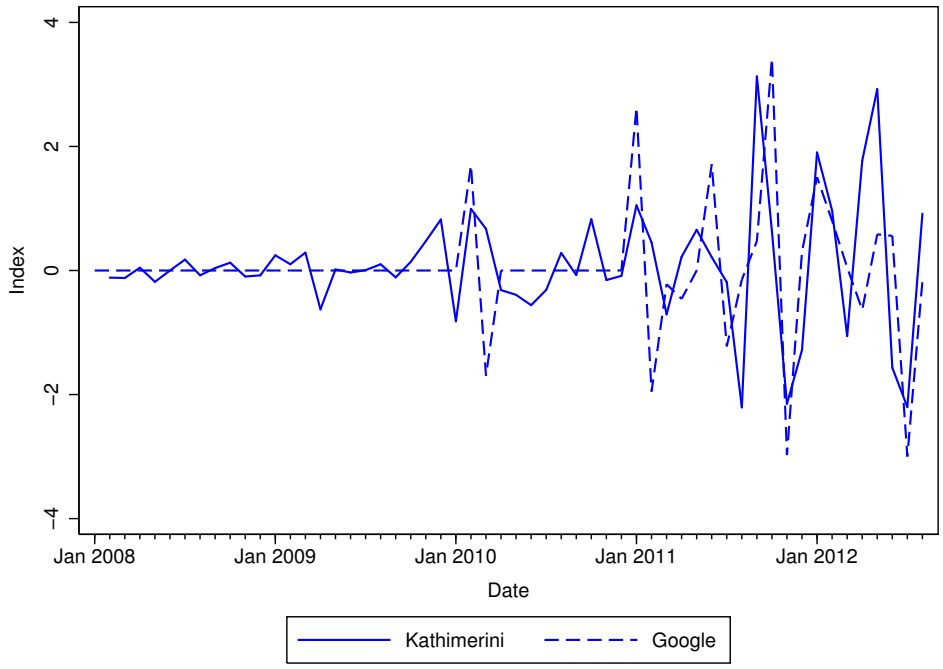
| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Share pop. affected | 0.00415** | 0.00520** | | 0.00296* | 0.00345** | |
| | (0.00195) | (0.00212) | | (0.00150) | (0.00162) | |
| Share articles | −0.461* | −1.096 | −1.537 | | | |
| | (0.230) | (4.388) | (6.769) | | | |
| Share articles* | −0.0974*** | −0.120** | −0.0843 | | | |
| Share pop. affected | (0.0281) | (0.0476) | (0.102) | | | |
| Google Index | | | | 0.000778 | 0.00457 | 0.0113 |
| | | | | (0.00105) | (0.0197) | (0.0207) |
| Google Index* | | | | −0.000745*** | −0.000858*** | −0.00119* |
| Share pop. affected | | | | (0.000152) | (0.000293) | (0.000641) |
| Observations | 2221 | 2221 | 2221 | 2221 | 2221 | 2221 |
| R-squared | 0.0406 | 0.0437 | 0.147 | 0.0387 | 0.0413 | 0.146 |
| Controls | Yes | Yes | No | Yes | Yes | No |
| Prefecture×Year | No | No | Yes | No | No | Yes |
| Controls× conflict month | No | Yes | Yes | No | Yes | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the 12-month difference in the monthly German share of new cars registered in a prefecture. The variable Share articles is the monthly share of Kathimerini articles related to German-Greek conflict. The variable Google Index is the average growth rate in the Google Trends search index for the terms "Germans", "German reparations" and "Distomo". The sample is restricted to the Volkswagen category, which includes Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda and Toyota. Prefecture controls include log(population) in 2001, share employed in agriculture in 2001, share employed in industry in 2001, share with higher education in 2001, share with secondary education in 2001, unemployment rate in 2001, ruggedness, average distance from the nearest road in 1940 and average distance from the nearest railway line in 1940. Standard errors are clustered at the prefecture level.

For each of the terms above we compile a monthly search index from Google for the period 2008–2012. The value of the index is practically zero until early 2010. Figure 2.7 plots the growth rate of the index alongside the growth rate of the

Kathimerini conflict-related article share. There is substantial overlap between the two. We use the growth rate of the Google index as a continuous measure of conflict in columns [4]–[6] of Table 2.8. Here, the interaction term is robustly negative across specifications. The effect is more modest than the baseline using newspaper articles — a standard deviation increase in the Google index increases the difference between reprisal and non-reprisal prefectures by 0.5 percentage points. However, the results suggest that independent of the type of indicator for the time-varying intensity of political conflict used, there is strong evidence of a differential effect according to reprisal status on car purchasing.

Figure 2.7: Comparison of news and search-based conflict index




**Notes:** The solid line is the growth rate in the monthly share of Kathimerini articles related to German-Greek conflict. The dashed line is the average growth rate in the Google Trends search index for the terms "Germans", "German reparations" and "Distomo". Both series are normalized by their standard deviation.

#### 2.3.2.3 Measures of reprisal status

Table 2.9 examines the effect of using alternative measures of reprisal status. In the first three columns, we use a simple dummy variable for prefectures that contain at least one town that suffered reprisals during the German occupation. Estimated coefficients suggest that the German market share drops only in reprisal prefectures, with the interaction coefficient in column [1] being significant. When adding interactions of the baseline controls with the conflict month and prefecture-year fixed effects, the coefficient retains its sign, but loses significance.

In columns [4]–[6], we repeat the exercise using a dummy for the top quartile of destroyed towns — in effect comparing the top 25 percent of the sample in terms of population affected by reprisals with the other 75 percent. In this way, only areas with a substantial amount of destruction are counted as affected by German massacres. The results show large and highly significant effects — in all three specifications, the interaction effect is negative, suggesting a collapse in German market share by 9–12 percentage points in reprisal prefectures compared to the rest. Finally, in columns [7]–[9] of Table 2.9, we use an alternative measure of wartime destruction. This is the share of each prefecture's 1940 building stock that was destroyed in the years 1941–1944. It is correlated with the extent of German reprisals, but it is a noisier proxy that also reflects other types of wartime destruction. According to the Subministry of Reconstruction (1946), which provides the data for the construction of this variable, German reprisal measures were responsible for one fourth of the total number of buildings destroyed. The rest was destroyed by wartime bombing, razed to make space for fortifications, or affected by Bulgarian reprisals in their zone of occupation. We find negative coefficients throughout, but only the baseline specification in column [7] is statistically significant.

### 2.3.3 The potential effect of unobservables

One of the potential weaknesses of our analysis is the paucity of high-frequency, prefecture-level control variables. To gauge the potential effect of unobserved variables, we implement the method of Altonji et al. (2005) of examining how

Table 2.9: Alternative definitions of reprisals

| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] |
|---|---|---|---|---|---|---|---|---|---|
| Conflict month | 0.000363 | 0.193 | 0.355 | −0.0154 | 0.235 | 0.333 | −0.00285 | 0.243 | 0.339 |
| | (0.0181) | (0.404) | (0.400) | (0.0139) | (0.411) | (0.400) | (0.0172) | (0.415) | (0.407) |
| Reprisal | −0.00183 | −0.00360 | | | | | | | |
| | (0.00609) | (0.00581) | | | | | | | |
| Conflict month* | −0.0611** | −0.0479 | −0.0161 | | | | | | |
| Reprisal | (0.0264) | (0.0332) | (0.0321) | | | | | | |
| Reprisal top quartile | | | | 0.00917 | 0.0114 | | | | |
| | | | | (0.0124) | (0.0127) | | | | |
| Conflict month* | | | | −0.0988* | −0.115** | −0.117** | | | |
| Reprisal top quartile | | | | (0.0497) | (0.0511) | (0.0552) | | | |
| Share destroyed housing | | | | | | | 0.0532** | 0.0465** | |
| | | | | | | | (0.0252) | (0.0217) | |
| Conflict month* | | | | | | | −0.160* | −0.109 | −0.112 |
| Share destroyed housing | | | | | | | (0.0906) | (0.0917) | (0.0948) |
| Observations | 2221 | 2221 | 2221 | 2221 | 2221 | 2221 | 2177 | 2177 | 2177 |
| R-squared | 0.0396 | 0.0432 | 0.145 | 0.0387 | 0.0444 | 0.148 | 0.0389 | 0.0440 | 0.146 |
| Controls | Yes | Yes | No | Yes | Yes | No | Yes | Yes | No |
| Prefecture×Year | No | No | Yes | No | No | Yes | No | No | Yes |
| Controls× conflict month | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the 12-month difference in the monthly German share of new cars registered in a prefecture. The variable Reprisal is an indicator equal to one if the prefecture includes at least one "martyred" town. The variable Reprisal top quartile is an indicator equal to one if the prefecture is in the 75th percentile of the share of 1940 population living in martyred towns. The variable Share destroyed housing is a measure of the percentage of a prefecture's housing stock that was destroyed in the years 1941–1944. Data for the construction of this variable are from Doxiadis (1947). The sample is restricted to the Volkswagen category, which includes Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda and Toyota. Prefecture controls include log(population) in 2001, share employed in agriculture in 2001, share employed in industry in 2001, share with higher education in 2001, share with secondary education in 2001, unemployment rate in 2001, ruggedness, average distance from the nearest road in 1940 and average distance from the nearest railway line in 1940. Standard errors are clustered at the prefecture level.

much the coefficient of our main variable of interest changes when we add control variables — and then ask how big the effect of unobservables on the key variable's coefficient would have to be for results to become insignificant. In the unrestricted regression, we use no controls other than year-fixed- effects; in the first exercise, we include prefecture-level controls as used in our baseline regression specification (Table 6, column [4]). Subsequently, and to proxy for the effect of time-varying unobservables, we add to this the interactions of these baseline controls with the confict month.

Table 2.10 presents the results, for both the full and the Volkswagen sample. For the baseline specification and the full sample, the effect of unobservables would have to be 19 times larger than the effect of the existing control variables (and act to the opposite direction) before the interaction effect between the conflict month and the reprisal prefecture dummy becomes zero. For the VW sample, the ratio is 13. These very large ratios capture the fact that our estimated coefficient is largely unaffected when we add controls. When extending the baseline set of controls to include interactions with the conflict month, the Altonji ratio in the VW category is 6.7 — again, this implies that only variables that are much stronger than existing controls could reduce the key effect to insignificance.

Table 2.10: Assessing selection on unobservables

| Controls restricted | Controls full | Sample | |
|---|---|---|---|
| | | All brands | VW Category |
| None | Baseline prefecture-level controls from Table 6, Column [4] | −19.6 | −13.5 |
| None | Baseline prefecture-level controls and interactions of controls with the conflict month | −9.8 | −6.7 |

**Notes:** The table reports the relative strength of unobservables needed to completely explain away the effect of a conflict month on the difference of the seasonally adjusted German car share between reprisal and non-reprisal prefectures (Altonji et al., 2005). Both restricted and unrestricted regressions include year fixed effects.

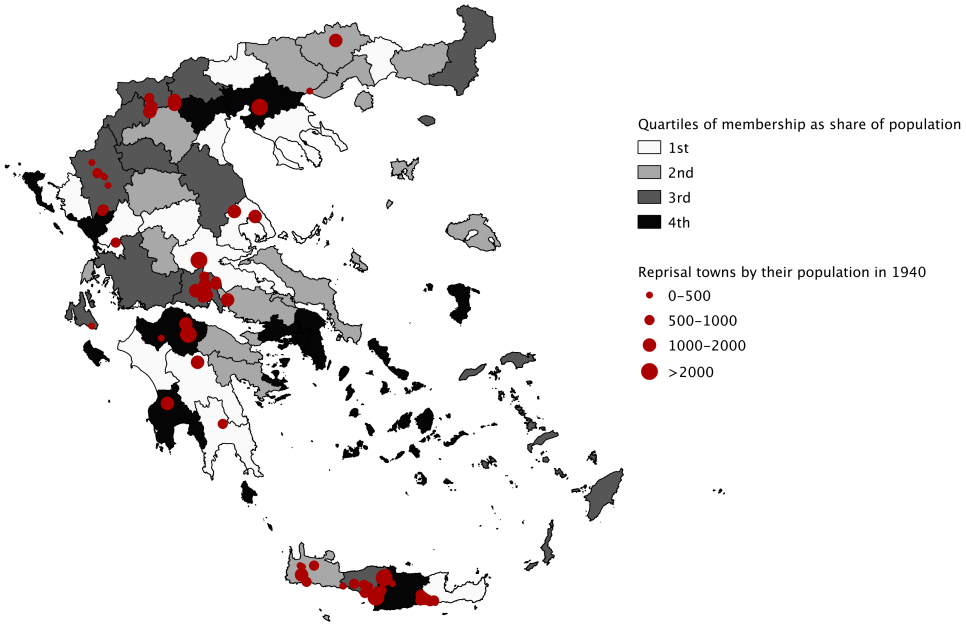
### 2.3.4 De-friending Germany

Is the differential effect on German car sales between reprisal and non-reprisal provinces really indicative of anti-German sentiment? To explore this issue further, we turn to the membership of Facebook groups dedicated to boycotting German products. We search for various versions of the phrases "boycott german products", "boycott foreign products" or "boycott Germany", and identify around 40 Facebook groups devoted to this purpose. Using the list of members for open groups, we assemble geo-coded information on members. To this purpose, we use the entry for "current city", for members making this information publicly available. In this way, we collect data on 1,900 boycott group members. These are then aggregated into a membership count for each prefecture. We normalize this measure by the prefecture's population. The spatial distribution of membership is shown in Figure 2.8. Prefectures that witnessed major massacres, such as Achaia and Viotia, belong to the highest quartile of the distribution of Facebook boycott groups.

In Table 2.11, we examine if the interaction between reprisals and political events is stronger in areas where more Greeks joined Facebook boycott groups.[18] We find that the interaction effect is large and significant for all prefectures that have more than the median membership of anti-German boycott groups. In the group of prefectures with below-average membership, only one estimated interaction effect is negative, and the result is non-significant. This pattern suggests that the decline in the market share of German cars — especially in provinces that suffered German reprisals, and in months of conflict — was part of a broader pattern of politically charged animosity. Where people did not join Facebook groups aimed at boycotting German products, contemporary events did not interact with memories of the past to create bigger declines in German market share; where political activism was high, the past mattered a great deal in amplifying changes in consumer behavior. These findings can be interpreted as highlighting the importance of social networks in overcoming collective action problems, such as in the case of boycotts.

[18] We cannot replicate our main analysis using Facebook membership as the dependent variable, since the time of joining a boycott group is not available.

Figure 2.8: Facebook membership in boycott groups




**Notes:** The map depicts by prefecture the number of members of Facebook groups calling for the boycott of German products, normalized by prefecture population. Member locations come from publicly viewable entries in the field "Current City" on users' profile pages. Data on reprisal towns are from presidential decrees no. 399 (1998), 99 (2000), 40 (2004) and 140 (2005). Population data are from the 1940 and 2001 Greek Census.

## 2.4 Discussion

In this section, we discuss the implications of our findings. In particular, we examine for how long the effects of German-Greek conflict are visible in the data, and what the main results imply for the effects of memory on economic behavior.

### 2.4.1 Explaining the shift in market share

One possibility is that consumers simply postpone purchases of German cars. In that case, a lost sale in one month will be made up by additional sales in later months. While the basic finding would still hold, the interpretation would be different — and the overall economic relevance would be less.

Table 2.11: Effects by membership in Facebook boycott groups

| | Below median | | | Above median | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Share pop. affected | −0.00450 | | | 0.00346* | | |
| | (0.00486) | | | (0.00183) | | |
| Conflict month | 0.000600 | 0.480 | 1.628* | −0.0115 | 0.220 | 0.165 |
| | (0.0276) | (0.783) | (0.829) | (0.0166) | (0.487) | (0.416) |
| Conflict month* | −0.0176 | −0.00908 | 0.0320 | −0.0179*** | −0.00969*** | −0.00969* |
| Share pop. affected | (0.0222) | (0.0303) | (0.0352) | (0.00482) | (0.00320) | (0.00512) |
| Observations | 1127 | 1127 | 1127 | 1094 | 1094 | 1094 |
| R-squared | 0.0614 | 0.0763 | 0.165 | 0.0395 | 0.0459 | 0.143 |
| Controls | Yes | Yes | No | Yes | Yes | No |
| Prefecture×Year | No | No | Yes | No | No | Yes |
| Controls× conflict month | No | Yes | Yes | No | Yes | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the 12-month difference in the monthly German share of new cars registered in a prefecture. The sample is divided into prefectures with above and below median membership in Facebook groups that call for the boycott of German products. The number of members is computed based on the entry in the field "Current City" in a user's Facebook profile and is normalized by the prefecture population. The sample is restricted to the Volkswagen category, which includes Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda and Toyota. Prefecture controls include log(population) in 2001, share employed in agriculture in 2001, share employed in industry in 2001, share with higher education in 2001, share with secondary education in 2001, unemployment rate in 2001, ruggedness, average distance from the nearest road in 1940 and average distance from the nearest railway line in 1940. Standard errors are clustered at the prefecture level.

To deal with the possibility that purchases are postponed, we use a triple-difference specification, with log car sales as the dependent variable. Table 2.12 reports the results of this exercise, starting from a baseline specification of the form:

$$\log(y_{ijt}) = \alpha + \lambda_t + \beta_1 C_t + \beta_2 D_j + \beta_3 G_i + \gamma_1 C_t * D_j + \gamma_2 C_t * G_i + \gamma_3 D_j * G_i + \delta(C_t * D_j * G_i) + \mathbf{X}_j \pi + \epsilon_{ijt}$$

and successively adding a number of fixed effects and fixed effects interactions. Here, $G_i$ is a dummy that takes on the value 1 if brand $i$ is German. The first thing that one can observe is that both German and other car brands experience a drop in their sales in conflict months. This is not surprising, given that our measure of conflict is bound to capture, apart from German-Greek political tension, part of the general effect of the crisis. This drop is larger for German cars in columns [5]–[7], when brand-year interactions are included. The triple-difference coefficient, which captures the effect of a conflict month on the gap between German and non-German cars in reprisal vs non-reprisal prefectures is always negative and highly significant.

If consumers in reprisal prefectures substituted German cars for non-German ones, we would expect non-German cars to rise in these prefectures in conflict months. Indeed, the interaction coefficient of a conflict month with the share of the population affected by reprisals captures just that. It is positive in all specifications and insignificant only in column [7], when all fixed effects and interactions are included together. Furthermore, its magnitude makes up for almost all of the extra drop that German car sales experience in reprisal prefectures. These results support the hypothesis that substitution is taking place. People are not simply waiting to buy their VW, they are buying a Peugeot instead.

### 2.4.2 Duration of effects

For how long does public conflict affect consumer behavior? It could well be that even in regions with a history of reprisal attacks, the effects of German-Greek conflict are short-term in nature, and disappear quickly. In that case, one interpretation of our results would be that even in places with a strong disposition towards animosity, not even severe public conflict can affect behavior for long — in line with the general conclusion in the boycott literature that effects are small

Table 2.12: Triple differences

| | [1] | [2] | [3] | [4] | [5] | [6] | [7] |
|---|---|---|---|---|---|---|---|
| Conflict month | −0.0685*** | −0.0685*** | −0.0685*** | −0.0685*** | −0.0555** | −0.0555** | −0.0322** |
| | (0.0223) | (0.0223) | (0.0223) | (0.0223) | (0.0220) | (0.0222) | (0.0136) |
| Share pop. affected | −0.0281 | −0.0307*** | −0.0307*** | | | | |
| | (0.0476) | (0.0105) | (0.0105) | | | | |
| German brand | 0.579*** | 0.579*** | | | | | |
| | (0.0442) | (0.0442) | | | | | |
| Conflict month* | 0.0251* | 0.0251* | 0.0251* | 0.0251* | 0.0251* | 0.0251* | 0.00354 |
| Share pop. affected | (0.0134) | (0.0134) | (0.0134) | (0.0134) | (0.0134) | (0.0135) | (0.00444) |
| Conflict month* | 0.0278 | 0.0278 | 0.0278 | 0.0278 | −0.0503* | −0.0503* | −0.0503* |
| German brand | (0.0287) | (0.0287) | (0.0287) | (0.0287) | (0.0281) | (0.0283) | (0.0284) |
| German brand* | 0.0123 | 0.0123 | 0.0123 | 0.0123 | 0.0123 | −0.0131*** | −0.0131*** |
| Share pop. affected | (0.0109) | (0.0109) | (0.0109) | (0.0110) | (0.0110) | (0.000767) | (0.000770) |
| Conflict month*German brand* | −0.0273*** | −0.0273*** | −0.0273*** | −0.0273*** | −0.0273*** | −0.0273*** | −0.0273*** |
| Share pop. affected | (0.00709) | (0.00709) | (0.00709) | (0.00710) | (0.00710) | (0.00716) | (0.00718) |
| Observations | 34272 | 34272 | 34272 | 34272 | 34272 | 34272 | 34272 |
| R-squared | 0.137 | 0.586 | 0.667 | 0.684 | 0.695 | 0.756 | 0.767 |
| Controls | No | Yes | Yes | No | No | No | No |
| Brand FE | No | No | Yes | Yes | Yes | Yes | Yes |
| Prefecture FE | No | No | No | Yes | Yes | Yes | Yes |
| Brand×Year FE | No | No | No | No | Yes | Yes | Yes |
| Prefect×Brand FE | No | No | No | No | No | Yes | Yes |
| Prefect×Year FE | No | No | No | No | No | No | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Years 2008–2012. The dependent variable is the logarithm of new cars registered in a prefecture. The Volkswagen category includes the following brands: Volkswagen, Opel, Citroen, Ford, Honda, Hyundai, Nissan, Peugeot, Renault, Seat, Skoda, Toyota. All regressions include year fixed effects. Prefecture controls include log(population) in 2001, share employed in agriculture in 2001, share employed in industry in 2001, share with higher education in 2001, share with secondary education in 2001, unemployment rate in 2001, ruggedness, average distance from the nearest road in 1940 and average distance from the nearest railway line in 1940. Standard errors are clustered at the prefecture level.

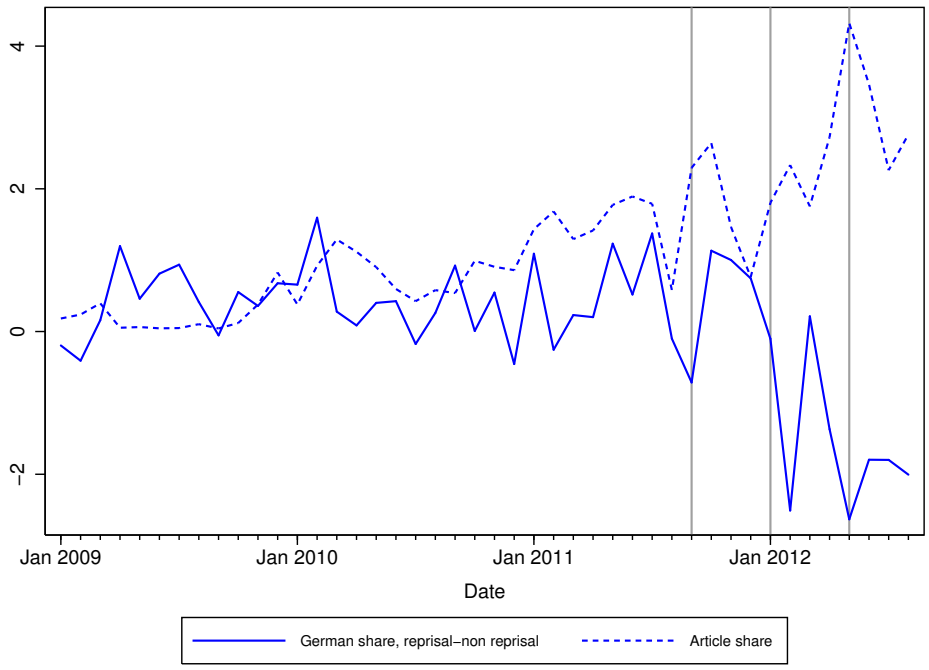
at best.

In Figure 2.9, we plot the share of Kathimerini articles devoted to German-Greek conflict side-by-side with the difference in market share for German cars between reprisal and non-reprisal prefectures. Vertical lines indicate conflict months. Until the first event, the average difference in German market share between reprisal and non-reprisal towns fluctuates around zero; thereafter, the difference becomes large and negative, with particularly sharp declines during the months identified as periods of extreme negative sentiment between the two sides. This suggests that, while market share changes sharply from month to month in any one prefecture, the effect of conflict accumulates. By the end of our sample period, the difference in the market share of German cars between reprisal and non-reprisal prefectures has grown to more than 2 percentage points, approximately 2 standard deviations. There is certainly no evidence that the effect dissipates quickly after the three crisis months.

### 2.4.3 Interpretation

How do we make sense of our main finding — the fact that past conflict influences economic behavior to a different extent, depending on current conditions, and in a way that varies by location? Our results are compatible with a broader set of findings that emphasize cognitive limitations as a source of behavioral biases. Becker (1993) already highlighted the limitations of human memory as a key feature of economic behavior. Mullainathan (2002) provides a unified model in which imperfect recall can explain a range of experimental findings suggesting seemingly irrational behavior. Psychologists emphasize two aspects of memory — recall is associative, and it responds to rehearsal. Cues, such as the famous taste of a madeleine recalled by Proust's hero in *A la recherche du temps perdu*, can evoke memories. Remembering past incidents is systematically easier if they are similar to events in the present. Also, the more often a particular memory is evoked, the easier it will be to recall in the future (Schacter, 1996). This implies that, in the final analysis, even random events in the present may shape behavior, by recalling certain elements of the past and altering future memory recall (Mullainathan, 2002). Finally, memory seems to be reconstructive — people remember

Figure 2.9: German-Greek conflict and evolution of German market share in prefectures with and without reprisals




**Notes:** The solid line is the difference in the seasonally adjusted (expressed as difference of month t from month t-12) share of German car registrations in the Volkswagen category in reprisal vs non-reprisal prefectures. The dotted line is the monthly share of Kathimerini articles related to German-Greek conflict. Both series are normalized by their standard deviation.

those aspects of the past that allow them to tell consistent stories. Parts that do not fit the explanatory model are much more likely to be left out (Bartlett, 1932).

The model that speaks most closely to our setting is Gennaioli and Shleifer (2010) on "what comes to mind". In their work, agents act as local thinkers. They possess only a limited capacity to process and store information. When evaluating a particular course of action, agents only consider a subset of all possible scenarios — the ones that are made salient by the context. In their model, agents have a preference for recalling representative scenarios. In their setting, acting on stereotypes will often be a useful strategy that imposes almost no costs on agents; in certain situations, however, these cognitive shortcuts will lead to major misapprehensions.

The behavior we document is compatible with the features of memory highlighted by psychologists and in earlier theoretical work in economics. The Euro crisis reduced the freedom of action of the Greek government, which had to rely on bailouts from its European partners. The German government in particular imposed harsh conditions, including severe expenditure cuts. As the placards carried by demonstrators show, the situation reminded many Greeks of the German occupation during World War II, which also left Greeks powerless to oppose any German diktat. As memories of World War II became more salient, seemingly consistent stories about the aberrant German national character gained traction. The surge in protest groups, calls for boycott, and the hatred expressed in demonstrations suggest that memories of war crimes were reactivated by the economic crisis. Such memories can become salient more readily in areas where people's families suffered at the hand of the occupying forces after 1941. Finally, as conflict erupted several times, its effects accumulated — repetition made it harder to forget the past, and the accumulated difference in market shares for German cars in reprisal prefectures kept increasing.

Our findings also highlight the role of emotions and fairness considerations in economic-decision making (Loewenstein, 2000). The heuristic of affect, or "gut feeling", is commonly used by humans to guide behavior (Kahneman, 2011), and probably has evolutionary origins (Boyd and Richerson, 1985a). When is its use more likely to be triggered? In an experimental study, Maheswaran and Chen (2006) show that, when primed with negative associations about a country, subjects are less likely to buy its products. Ethnocentrism and an emphasis on distinctions between in- and outgroups (Kinder and Kam, 2009) only intensifies adverse reactions against the perceived mistreatment of one's own country. Studies in experimental psychology find that affect is more likely to inform a decision when experiences related to it have left a strong emotional impression in the past (Serman and Kim, 2002). The German occupation was a historical event that had such an emotional impact on many Greeks, and all the more so in areas that saw massacres and villages burned to the ground. Such events were likely to trigger an emotional response at the time of the Greek debt crisis, which underlined the Germans' position of power.

This also relates to the notion of fairness, which has been the subject of ex-

tensive research in game theory and experimental economics (Fehr and Gächter, 2000). Individuals are willing to deviate from individually optimal behavior when perceived fairness is violated, by, for example, losing money to punish those whose behavior was perceived as unfair. The notion of fairness is also known to be reference-dependent (Kahneman et al., 1986). A perceived lack of fairness could have driven Greek consumer behavior during the crisis. The German side insisted on severe cutbacks in expenditures. Salary cuts combined with tax hikes strongly reduced disposable incomes compared to the recent past. Fairness considerations alone cannot explain the *differential* sales pattern we identify among Greek prefectures, but it is possible that they form part of the explanation for the general drop in German car sales during conflict months.

In the final analysis, our research cannot identify the precise channel responsible for the differential slump in car sales. The evidence is compatible with an interpretation emphasizing emotional responses, and in particular, anger at perceived unfair treatment at the hands of Germany. Where such anger is combined with memories of earlier war crimes, consumer behavior suggests that the public-good logic — which in normal times stifles boycott participation — can be overcome, resulting in a powerful backlash.

## 2.5 Conclusions

Boycotts are among the most common means of collective political action today. Remarkably, few empirical studies have documented that consumers actually change their purchasing behavior in response. Free-riding on the moralizing behavior of others is a common temptation (Sen et al., 2001). At the same time, there is strong evidence that memories of past conflict linger, and continue to influence economic outcomes in the present. For example, countries that often went to war with each other in the past still trade less today (Guiso et al., 2009). This begs the question how armed conflict in the past can still influence consumer choice in the present, especially if boycotts in general "don't work".

We examine the case of Greece after the outbreak of the debt crisis in 2010. Forced to borrow from EU partners, the country had to implement severe austerity measures. Many of the policies implemented in exchange for the EU bailout pack-

ages were blamed on German policies. Public spats between German and Greek politicians deepened the impression of deeply-rooted antagonisms. The Greek public, when protesting, used Nazi-era symbols to express its outrage about German demands for more spending cuts and the perceived unfairness of conditions imposed on Greece. Press articles about German massacres during World War II increased in frequency.

These events affected consumer behavior in Greece. German car sales suffered in months of conflict, but not in a uniform way. In those areas where German occupying forces after 1941 had carried out reprisal measures — torching villages and killing the civilian population — saw the sharpest declines in German car sales. Prefectures where no major war crimes had been committed saw much smaller declines in car sales, or no significant change at all. This strongly suggests that public conflict matters for economic behavior when it revives latent animosity, reflecting an earlier history of conflict. In this way, past conflict begets more acrimony in the present. Cycles of conflict are driven by the way in which current events are interpreted through the lens of past experiences.[19]

In normal times, boycotts are subject to severe collective action problems. Just as in the case of voting, the costs accrue at the individual level, while the benefits are generated in the aggregate. Our results suggest that this logic of individual choice can be overcome if public outrage is large enough. In particular, when contemporary events interact with memories of earlier outrages, consumer behavior can change in a major way — as was also the case with French automobile sales in China in the 2000s (reviving memories of the humiliation of China at the hands of Western powers) and with Sino-Japanese trade in 2005 (recalling the war crimes committed by Japan's armed forces during the invasion of China in the 1930s).[20] These findings are compatible with an interpretation emphasizing the importance of salience in economic decision-making (Gennaioli and Shleifer, 2010).

[19] Acemoglu and Wolitzky (2014) show that cycles of conflict can also contain the seeds of their own destruction — eventually, after repeated cycles of conflict, a Bayesian agent will conclude that bad actions are not necessarily a sign of bad intentions.

[20] Hong et al. (2011); Fisman et al. (2013).

## 2.6 Appendix A: Data description

### 2.6.1 Variable descriptions

| Variable | Description and Source |
|---|---|
| German car share | The monthly share of German-manufactured cars in a prefecture's total new car registrations, expressed as the difference of the share in month t from month t-12. Manufacturers defined as German are Volkswagen, Opel, BMW, Audi, Porsche, Mercedes and Smart. |
| Conflict month | Defined as a local peak in the growth series of the monthly share of Kathimerini articles relevant to German-Greek conflict, and the month after the peak. For details on the selection of the relevant articles see Appendix C. Source: Kathimerini electronic archive 2008–2012, sections on "Greece", "Politics" and "Economy". |
| Share pop. affected | The share of a prefecture's 1940 population that lived in towns designated as "martyred towns" by Presidential Decrees no. 399 (1998), 99 (2000), 40 (2004) and 140 (2005). Population data are from the 1940 Greek census. |
| Share destroyed housing | The share of a prefecture's pre-war housing stock that was destroyed in the period 1941–1944. Source: Doxiadis (1947). |
| Share articles | Monthly Kathimerini articles relevant to German-Greek conflict, normalized by the total number of Kathimerini articles in the month. For details on the selection of the relevant articles see Appendix C. Source: Kathimerini electronic archive 2008–2012, sections on "Greece", "Politics" and "Economy". |
| Google Index | Average growth rate of the monthly Google search index series for the terms "Germans", "German reparations" and "Distomo" in the period 2008–2012, for the geographic area of Greece. The Google search index is a normalization of the share of total searches represented by each term in a given time and region. |

| Variable | Description and Source |
|---|---|
| Facebook membership | Number of members in Facebook groups that call for the boycott of German or foreign products, normalized by the prefecture's population in 2001. Members' location is their current location and not their place of origin. |
| Population | Source: 2001 Greek census. |
| Share employed in agriculture | Source: 2001 Greek census. |
| Share employed in industry | Source: 2001 Greek census. |
| Share with secondary education | Source: 2001 Greek census. |
| Share with higher education | Source: 2001 Greek census. |
| Share civil servants | Source: 2001 Greek census. |
| Unemployment rate | Source: 2001 Greek census. |
| Share seats to communists | The share of a prefecture's seats allocated to the coalition of the Greek Communist Party and the Greek Agrarian Party (Pallaiko Metopo) in the 1936 parliamentary elections. Source: Hellenic Parliament, Registry of Parliament Members. |
| Votes to Golden Dawn | The share of a prefecture's votes to the party of Golden Dawn in the June 2012 parliamentary elections. Source: Hellenic Ministry of Interior. |
| Ruggedness | Terrain ruggedness index computed as in Riley et al. (1999) and averaged over each prefecture's surface. The shapefile of prefecture boundaries is from ELSTAT and elevation data from GMTED2010. |
| Average distance to nearest road in 1940 | To compute this measure we first compute the distance to the nearest road from the centroid of each 50×50 km grid cell in an equidistant projection and then average over each prefecture's surface. We digitize a physical map of Greece's pre-WWII road network from Doxiadis (1947). The shapefile of prefecture boundaries is from ELSTAT. |

| Variable | Description and Source |
|---|---|
| Average distance to nearest railway line in 1940 | Similarly to the above measure, we first compute the distance to the nearest railway line from the centroid of each 50×50 km grid cell in an equidistant projection and then average over each prefecture's surface. We digitize a physical map of Greece's pre-WWII railway network from Doxiadis (1947). The shapefile of prefecture boundaries is from ELSTAT. |

### 2.6.2 List of martyred towns

1. Aetos, Messinia
2. Agia Efthimia, Fokida
3. Agii Anargiri, Lakonia
4. Agios Vassilios, Iraklio
5. Amira, Iraklio
6. Ano Meros, Rethymno
7. Ano Viannos, Iraklio
8. Anogia, Rethymno
9. Arginia, Kefallonia
10. Aspragеloi, Ioannina
11. Chondros, Iraklio
12. Chortiatis, Thessaloniki
13. Damasta, Iraklio
14. Distomo, Viotia
15. Drakia, Magnisia
16. Drosopigi, Florina
17. Elati, Ioannina
18. Emparos, Iraklio
19. Eptalofos, Fokida
20. Erimanthia, Achaia
21. Gdochia, Lasithi
22. Gerakari, Rethymno
23. Ipati, Fthiotida
24. Kakopetro, Chania
25. Kalami, Iraklio
26. Kalavryta, Achaia
27. Kaloskopi, Fokida
28. Kaloutas, Ioannina
29. Kandanos, Chania
30. Karoutes, Fokida
31. Kato Simi, Iraklio
32. Kato Viannos, Iraklio
33. Kefalovryso, Iraklio
34. Klisoura, Kastoria
35. Kommeno, Arta
36. Koxare, Rethymno
37. Kria Vrissi, Rethymno
38. Lechovo, Florina
39. Lidoriki, Fokida
40. Ligiades, Ioannina
41. Lilea, Fokida
42. Lochria, Rethymno
43. Magarikari, Iraklio
44. Malathiros, Chania
45. Manassis, Ioannina
46. Mesovouni, Ioannina
47. Mesovouno, Kozani
48. Mirtos, Lasithi
49. Mournies, Lasithi
50. Mousiotitsa, Ioannina
51. Nea Kerdillia, Serres
52. Pefko, Iraklio
53. Pentapoli, Fokida

54. Pirgi, Kozani
55. Prosilio, Fokida
56. Pteri, Achaia
57. Riza, Lasithi
58. Rizomilo, Magnisia
59. Rodakinou, Rethymno
60. Rogi, Achaia
61. Saktouria, Rethymno
62. Sarchos, Iraklio
63. Sidironero, Drama
64. Sikologos, Iraklio
65. Skines, Chania
66. Sougia, Chania
67. Tibaki, Iraklio
68. Vachos, Iraklio
69. Vlacherna, Arkadia
70. Vorizia, Iraklio
71. Vounichora, Fokida
72. Vrisses, Rethymno

### 2.6.3 News index construction

For the construction of the index, we use the full 2008–2012 Kathimerini archive of the sections "Greece", "Politics" and "Economy", containing a total of 64,854 articles. As a first step, we sample 10% of articles containing the stem "german-" and manually classify them into relevant and non-relevant to political tension between Germany and Greece. An article classified as relevant must contain a reference, however short, to German-Greek conflicting political interests or political interactions in the context of foreign relations, the eurozone or the issue of German war reparations. Articles that refer to German-Greek relations in another context — e.g. tourism flows, economic transactions between German and Greek firms etc. — are classified as non-relevant.

We split the audited sample into a training and test set and use the test set to evaluate the classification performance of our algorithm. We start by assigning a frequency score to each term appearing more than three times in the articles of the training set. This score captures how frequently the term appears in conflict-related articles *relative to* non-conflict-related articles. The frequency score for term i is an empirical index constructed as:

$$\text{Frequency score}_i = \frac{\Pr(\text{i}|\text{c})}{\Pr(\text{i}|\text{c}) + \Pr(\text{i}|\text{nc})} * 100$$

where c is a conflict-related article and nc an unrelated article. The score takes on the value 100 when a term appears only in conflict-related articles and the value 0 when it appears only in unrelated articles. Terms like *bankruptcy*, *memorandum*,

*austerity*, but also subtler terms like *discipline*, *painful*, *tolerance*, score above 95 in this index.

We use the list of high-scoring terms and form all combinations of 5 or more terms from this list. For each combination, we classify an article in our test set as conflict-related if it contains at least one of the terms in the combination. We then compare this classification to the human audit and evaluate each combination of terms based on a compound measure of precision and recall known as the F1-score. Since we are interested in minimizing both false positive and false negative classifications of articles, we put equal weight on the two types of errors and pick the combination that maximizes:

$$F_1 = 2 * \frac{2 * \text{true positive}}{2 * \text{true positive} + \text{false negative} + \text{false positive}}$$

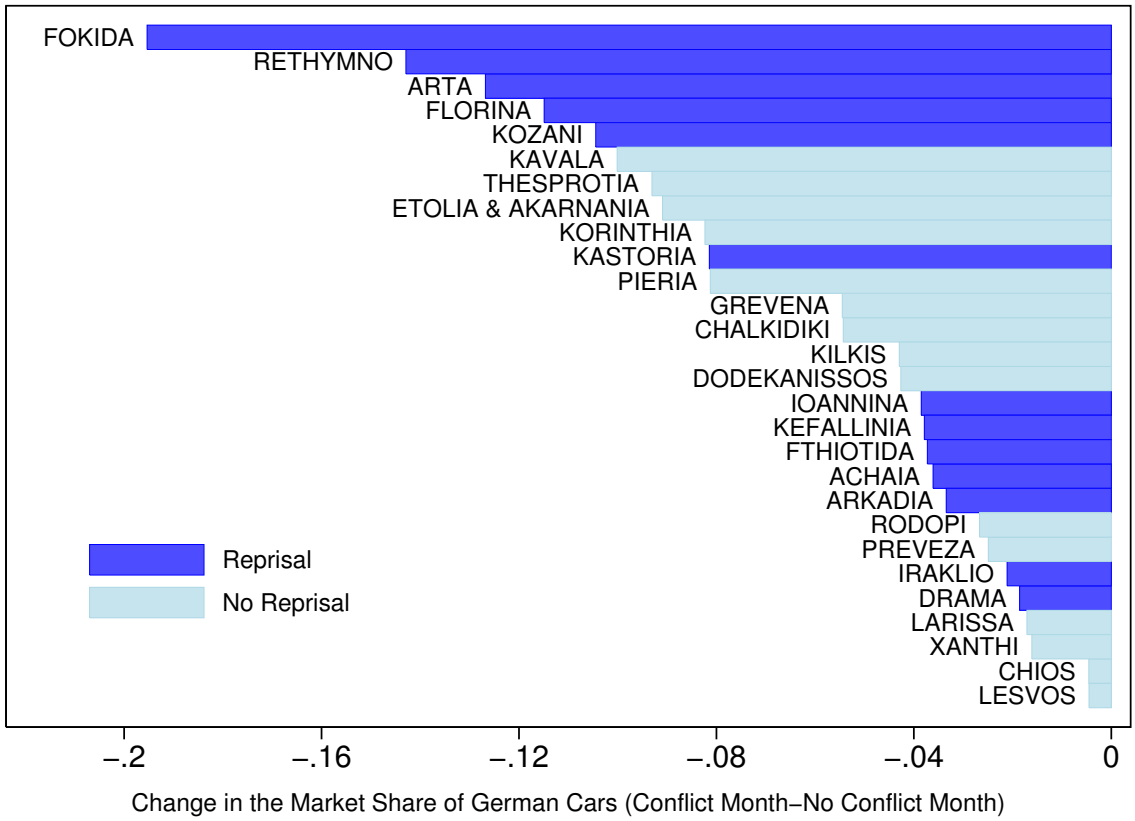
Based on the above procedure, we end up classifying an article as related to German-Greek conflict if it contains the stem "german-" and at least one of the words in the set {memorandum, troika, haircut, Distomo, default, austerity, Schäuble}. This gives us a monthly count of conflict-related articles, which we normalize by the total number of articles Kathimerini published in the month.

## 2.7 Appendix B: Additional figures and tables

Figure 2.10 looks at the seasonally adjusted change in the market share of German cars by prefecture. Bars in dark blue indicate that a massacre was carried out by the German forces after 1941. All five prefectures with the largest declines in the share of German cars were also the site of German massacres. The prefecture of Fokida, for example, home to the martyred town of Distomo, saw almost 12 percent of its 1940 population killed or made homeless by German retribution measures. During periods of German-Greek conflict in the period 2008–2012, the German market share dropped by almost 20 percentage points (and close to 30 percentage points in the Volkswagen category).[21]

[21]Rethymno, a prefecture on Crete, saw massive German reprisals after the capture of the island. In one famous incident, German parachute regiments perpetrated the so-called "Holocaust

Figure 2.10: Decline in the market share of German cars




**Notes:** The figure shows the seasonally adjusted change in the market share of German cars during coflict months. We adjust seasonally by examining the difference in market share between month t and t-12.

of Kedros" — a pre-emptive strike against partisan forces, with 8 villages burnt to the ground and all male inhabitants (164 men and boys) shot (Nessou, 2009). Rethymno registered the second-largest decline in the share of German cars in our sample — some 14%. Third on the list is the prefecture of Arta, site of the massacre of Kommeno, with 317 civilian victims (Mazower, 1995). It shows a drop of 13 percentage points in the share of German cars in conflict months (14 for the VW category). In contrast, the neighboring prefecture of Preveza — not affected by German reprisals — experienced a drop of only 3 percent.

# Chapter 3

# AGRICULTURAL LABOR INTENSITY AND THE ORIGINS OF WORK ETHICS

## 3.1 Introduction

Attitudes towards work have been connected to economic development since Max Weber's famous thesis on the Protestant work ethic and the rise of capitalism. Changing work patterns (de Vries, 1994; Voth, 1998) and an increasing importance placed on the values of hard work and diligence (Anthony, 1977) marked the passage from a peasant society to industrialization in England, while the Confucian work ethic has been credited with part of the success of the East Asian "miracle" economies (Liang, 2010). Today, attitudes toward work and leisure vary widely across countries, with the divide between the US and Europe being the most well known example of this variation (Alesina et al., 2006). Though one can see how hard-working individuals and societies might end up doing well, the origin of such values is not obvious, since work also entails disutility. In fact, for some authors, the question is "not why people are lazy or why they goof off but why, in absence of compulsion, they work hard" (Lipset, 1992). This study suggests that a norm of hard work develops when returns to work outweigh its costs. In particular, we examine the hypothesis that a work ethic forms when labor constitutes a relatively important input in the production process.

Studies in evolutionary anthropology suggest that attitudes are shaped as part of the interaction of humans with their environment and that cultural norms that

have been proven useful will be selected and transmitted more successfuly than others, through both vertical and horizontal socialization (Boyd and Richerson, 1985b). A relatively recent literature in economics has used these insights to show how preferences can be endogenously chosen and transmitted from parents to offspring as a response to the environment (Bisin and Verdier, 2001; Tabellini, 2008; Doepke and Zilibotti, 2008). A number of empirical studies have shown that geography and the mode of production has an impact on culture, from cooperative behavior (Henrich et al., 2001), to trust (Durante, 2010), to gender norms (Alesina et al., 2013).

Our study builds on these ideas and develops a theory of how a preference for work can arise and persist in societies where labor is a relatively important factor in production. We look for the origins of work ethic in the pre-industrial agricultural production structure of modern economies, both because agriculture was the main mode of production for human societies for a very long time, and also because it continues to play an important role in many developing countries today. Our main hypothesis is that a high labor intensity in agricultural production, or, alternatively, a high equilibrium labor to land ratio, should provide an incentive for investment in a preference for work. Other things equal, societies cultivating crops more dependent on labor effort, will have to provide a higher labor input in equilibrium. Since a larger share of the total output depends on the provision of labor, norms that reduce the disutility of labor will be useful in these societies, and will prevail, just as the anthropological literature suggests. Such norms can then persist and be perpetuated through socialization mechanisms. As in models of cultural transmission (Bisin and Verdier, 2000, 2001), altruistic parents who care about their children's utility, will invest more in their offsprings' preference for work when their future income relies more on it.

Equilibrium utilization of labor in agriculture depends on many things, including the availability of capital or other production factors, the production technology and environmental conditions. Nevertheless, when we hold the rest of these factors constant, different crops are produced through different cultivation processes and impose "technological contraints" on the optimal labor to land ratio by the very nature of their cultivation. Rice is perhaps the most notable example of a labor intensive crop (Bray, 1986). A number of studies document its higher re-

quirement of labor input in equilibrium, as demonstrated by the choices of farmers who cultivate rice alongside other crops. Esther Boserup records that farmers in India allocate 125 work days per hectare for wet paddy rice, while only 33–47 days per hectare for dry wheat (Boserup, 1965). Similar observations in contemporary China show that farmers spent 12–25 days of work per mu (approx. 0.165 acres) of rice versus 4–10 days of work per mu of wheat (Bell, 1992). These studies are supported in their conclusions by studies from environmental scientists. Ruthenberg (1976) notes that marginal returns to labor in wheat production are "lower and decrease more rapidly with greater employment of labor" when compared with rice production.

The laborious nature of rice cultivation has been theorized to have an impact on the work ethic of those societies that have historically depended on this crop for sustenance (Davidson, 2009). "No one who can rise before dawn three hundred sixty days a year fails to make his family rich", is a proverb from Northern China that illustrates how hard work formed part of the transmission mechanism of culture (Arkush, 1984). Certain studies go as far as accounting for the high academic achievement of Asian students through their industriousness, shaped by the "tradition of wet-rice agriculture and meaningful work" (Gladwell, 2008).

In this study, we test the intuition that agricultural labor intensity leads to a culture of high work values in a systematic way. We start by showing theoretically that a high equilibrium labor share in agricultural production will endogenously lower the disutility from work, when altruistic parents can invest in their offsprings' work preferences. We then take this prediction to the data. The first step in this process is to obtain an estimate of how labor intensive is the production of different crops under conditions of traditional and largely non-mechanized agriculture. We use data from the 1886 Prussian agricultural census, which is, to our knowledge, one of the oldest available censuses containing yield information disaggregated by crop. Assuming that the agricultural production function is a Cobb-Douglas and that the crop allocation of farmers follows the principle of profit maximization, we can back out the share of labor relative to land in each crop's production. This provides us with an implicit ranking of crops in terms of labor intensity. We then combine this ranking with data on soil and climate suitability for each crop from FAO, in order to create a composite measure of "potential" labor intensity. Our

measure is in practice a weighted average of suitabilities for different crops, where the weights are the crops' estimated labor intensities, and it is meant to capture the likelihood that agricultural production in an area will be on average more dependent on labor.

We then show that this measure of potential labor intensity predicts attitudes towards work in European regions today. Using data from the European Values Study and the European Social Survey, we find that a higher potential labor intensity leads to a higher reported importance placed on work and a higher number of desired weekly work hours, controlling for country fixed effects and a number of individual and regional socioeconomic and geographic controls. These results do not depend on the specific Prussian data we use to compute the labor intensity of different crops. Using information from the US Census of Agriculture for the years 1880–1900, we obtain a very similar ranking of crops in terms of labor requirements and recomputing our potential labor intensity measure with US data yields an effect on work attitudes that is both qualitatively and quantitatively similar.

We provide some evidence that part of the persistent effect of labor intensity on work attitudes is through cultural transmission. Our estimates get generally larger in magnitude when we progressively exclude from our sample first and second generation immigrants, whose culture has been shaped by historical conditions in the region of their ancestors and not of their current home. Conversely, when looking at the children of European immigrants in the US, who carry different cultures but face a similar institutional environment, we find that potential labor intensity in their parents' country of origin has a significant and positive effect on the number of hours they work weekly.

Our study contributes to a growing literature investigating the historical determinants of norms and preferences. Similarly to Alesina et al. (2013), we emphasize the role played in the formation of norms by historical long-lasting production processes. Other studies stressing the role of history for the formation of culture are Guiso et al. (2013), who show that Italian cities with a past of self-governance have higher levels of social capital today, Nunn and Wantchekon (2011), who demostrate that trust levels in Africa today can be explained by historical exposure to the slave trade, and Voigtländer and Voth (2012) who find that anti-semitic

attitudes persist at the city-level in Germany over more than 800 years.[1]

Most empirical studies investigating the determinants of work norms have focused on the role of Protestantism, in an attempt to test part of the original Weber hypothesis. Spenkuch (2011) uses data from the German Socio-Economic panel to show that historical adoption of protestantism in German precincts affects work hours and earnings of individuals today. Brügger et al. (2013) find significant differences in attitudes towards unemployment in the two sides of the border dividing Protestants from Catholics in Switzerland. Andersen et al. (2013) find that the historical presence of Cistercian monasteries, that pre-dated Protestantism, but were characterized by similar values of hard work and thrift, affects work attitudes in England today.

Various papers have treated theoretically the transmission of values for work and leisure (Bisin and Verdier (2001), Lindbeck and Nyberg (2006), Doepke and Zilibotti (2008)). The only study we are aware of that in any way deals with the effects of labor intensity in agricultural production is Vollrath (2011). This paper finds that labor intensive pre-industrial agriculture can stall industrialization, since it causes a larger share of the population to be employed in agriculture and lowers output per capita. Using relative suitabilities for wheat versus rice, the paper establishes this correlation in cross-country data. Our study suggests an alternative path through which labor intensity can affect industrialization, when preferences are endogenous. When work norms are generally strong, the incentive for capital accumulation is more pronounced, as, for any given level of capital, more labor will imply a higher marginal return from its use. This can in fact lead to more capital accumulation in labor intensive hard-working societies, once an industrial sector has been introduced.[2]

The paper is organized as follows. In section 2 we present a simple model

[1]Becker et al. (forthcoming) document the persistent effects of being part of the Habsburg empire on attitudes towards the state, while Grosjean (2014) finds empirical support for the persistence of a culture of honor in the US South dating back to settlement of the area by Scots-Irish immigrants in the late 18th century.

[2]Confucian values, which place an important weight on hard work and discipline, are thought by many scholars to contribute the cultural basis for the recent "miracle" growth of — labor-intensive, traditionally rice-growing — East Asian economies, much in the same way that the Protestant work ethic led to the rise of capitalism in the West (Hofstede and Bond, 1988; Chan, 1996; Liang, 2010).

of endogenous preferences, in which a high agricultural labor intensity leads to a higher work ethic. Section 3 explains the construction of our measure of potential labor intensity. In section 4 we test our main hypothesis with European regional data and provide evidence for the cultural transmission of work attitudes. In section 5, we present robustness checks, using alternative estimation strategies and computing crop-specific labor intensity with alternative data. Finally, section 6 discusses the caveats and limitations of our approach and findings and offers possible further extensions.

## 3.2 A model of work ethic formation

To study the formation of a work ethic, we use a standard flow utility function in consumption and hours worked, of the form $u = \log(c) - \frac{1}{\gamma}\frac{h^{1+\phi}}{1+\phi}$, where $\frac{1}{\gamma}$ denotes the endogenous disutility of work. $\gamma$ represents the work ethic and is formed through a parental transmission mechanism similar to the one used by Doepke and Zilibotti (2008). The law of motion for $\gamma$ is

$$\gamma' = \rho\gamma + \Psi(I) \tag{3.1}$$

with $\Psi(0) = 0, \Psi_I > 0, \Psi_{II} < 0$. $I$ represents the investment of the parents (in utility terms) in their offspring's work ethic. Individuals live for two periods, one as a child and one as a parent, and work and consume only in the latter. An altruistic parent then solves the dynamic program

$$V(\gamma) = \max_{c,h,I} \left\{ \log(c) - \frac{1}{\gamma}\frac{h^{1+\phi}}{1+\phi} - I + \delta V(\gamma') \right\}$$

subject to the law of motion (3.1) and a resource constraint $c = AT^{1-\beta}h^{\beta}$, where $T$ is a fixed endowment of land and $\beta$ is the labor intensity of production. The

first order and envelope conditions are

$$\frac{\beta}{h} = \frac{1}{\gamma} h^{\phi} \tag{3.2}$$

$$1 = \delta V_{\gamma}(\gamma') \Psi_I(I) \tag{3.3}$$

$$V_{\gamma}(\gamma) = \frac{1}{\gamma^2} \frac{h^{1+\phi}}{1+\phi} + \delta \rho V_{\gamma}(\gamma') \tag{3.4}$$

It follows directly from (3.2) that equilibrium labor supply is given by

$$h = (\beta\gamma)^{\frac{1}{1+\phi}}$$

For a given value of $\gamma$, labor supply is increasing in labor intensity $\beta$, which in turn implies a higher return to work ethic, as can be seen in (3.4).
Solving for the unique steady state, we get

$$\gamma^{ss} = \frac{\Psi(I^{ss})}{1-\rho} \tag{3.5}$$

$$1 = \frac{\delta}{1-\delta\rho} \frac{1-\rho}{\Psi(I^{ss})} \frac{\beta}{1+\phi} \Psi_I(I^{ss}) \tag{3.6}$$

Notice that the right hand side of (3.6) is decreasing in $I^{ss}$, which implies that steady state parental investment is increasing in $\beta$. It then follows directly from (3.5) that the work ethic is an increasing function of the labor intensity in production.

While this model is kept very parsimonious for purposes of exposition, we discuss extended versions in the appendix to address two potential sources of concern. We first introduce an endogenous fertility choice, to investigate whether Malthusian population growth may counteract the development of a high work ethic in a labor intensive environment. We show that while the relationship between labor intensity and steady state population size is ambiguous, its effect on work ethics remains strictly positive. Intuitively, the first result comes from the fact that in an economy with high labor intensity, the possibility of the parent to invest in a valuable work ethic introduces a quality versus quantity trade-off, potentially reducing the optimal number of children.

We further study the case of subsistence agriculture by introducing a minimum consumption requirement. Whether this constraint is binding is an endogenous outcome in our model. In the region where the constraint binds, labor productivity becomes a crucial determinant of attitudes towards work. We derive the conditions under which labor intensity, as measured by $\beta$, continues to positively affect work ethics in subsistence agriculture. These conditions are more likely to hold as the economy moves closer to leaving the constrained area, and are essentially the same as in Vollrath (2011). To deal with the potential confounding effect of productivity when the subsistence consumption level is barely reached, we will control for the overall suitability for rainfed agriculture in our estimations.

## 3.3 Measuring labor intensity

The main challenge in empirically testing the relation between agricultural labor intensity and work ethics lies in the measurement of labor intensity. Societies with similar production modes and comparable productivity potentials will differ in how much labor they utilize relative to other factors depending on the nature of the main crops they cultivate. As several studies indicate, wheat and other cereals demand a lower labor to land ratio than rice (Boserup, 1965; Ruthenberg, 1976; Bell, 1992). This ranking in terms of labor intensity can presumably be generalized to include all important staple crops.

Agronomic studies often offer estimates of labor requirements in agricultural production. Unfortunately, few studies do so systematically for different crops, and those who do are focused on contemporary mechanized agriculture, usually in the US (Cooper, 1916; Wakeman Lenhart, 1945). FAO's Ecocrop database is the closest to a systematic survey of the characteristics of various crops under different production modes. Though labor intensity is included in the recorded characteristics of crops in Ecocrop, its values are missing for most crops, with non-missing entries for only 3 out of the 15 most important staple crops wordwide.[3]

In order to obtain a more detailed and systematic ranking of crops in terms of labor intensity, we follow a procedure similar to the one suggested by FAO (Lee and

[3]According to this classification, wetland rice is a high labor intensity crop, while barley and rye are low labor intensity ones.

Zepeda, 2001) for gauging the crop-specific marginal returns of various inputs in agricultural production. We describe a simplified version of this procedure below.

To derive the crop-specific equilibrium share of labor, we need to make some minimal assumptions on the behavior of farmers and the form of agricultural production. In particular, we assume that farmers use their resources efficiently and allocate their available land to different crops in such a way that marginal returns to land are equalized.[4] This implies the additional assumption that land, at least at the margin, is not crop-specific, namely that all crops from the farmer's available crop set can potentially grow on the same land. Finally, we consider a Cobb-Douglas production function with constant returns to scale in land and labor.[5] We can then write the profit maximization problem of a representative farmer in region $j$ as

$$\max_{H_{i,j},T_{i,j}} \sum^{i} (P_{i,j}Y_{i,j} - r_j T_{i,j} - w_j H_{i,j})$$

where $P_{i,j}$ is the market price of crop $i$ in region $j$, $Y_{i,j}$ is the output of crop $i$ with $Y_{i,j} = A_{i,j}T_{i,j}^{1-\beta_i}H_{i,j}^{\beta_i}$, and $T_{i,j}$ and $H_{i,j}$ are usage of land and labor with respective region specific prices $r_j$ and $w_j$. Finally, $\beta_i$ represents the crop specific labor intensity of production.

Efficient usage of land by the farmer implies the following first order condition resulting from the above optimization problem

$$(1-\beta_i)\frac{P_{i,j}Y_{i,j}}{T_{i,j}} = r_j$$

[4]In other words, farmers behave as profit maximizers, though, if we substitute crop-specific prices with calories, we can also think of them as maximizing agricultural surplus in calorie terms. The problem set-up in terms of profits also assumes that markets of both agricultural inputs and output are competitive.

[5]For the moment, we abstract from capital. To the extent that its use is negligible or does not differ across crops, this simplification will not be important for our results, and is often assumed by studies estimating factor shares in traditional agriculture (see for example, Wilde (2013)), including Kopsidis and Wolf (2012), who estimate agricultural productivity in Prussia using census data. In theory, we can include capital — or any number of crop-specific inputs — in the production function, so long as we have data on their use. The problem in practice is that almost no agricultural census, contemporary or historical, includes information on crop-specific use of machinery or animals.

Reshuffling terms and taking logs this relation becomes

$$\log(P_{i,j}Y_{i,j}) = \log(r_j) - \log(1-\beta_i) + \log(T_{i,j}) \tag{3.7}$$

which can be estimated with data on crop values and on land allocated to the cultivation of different crops. This is information available in most contemporary agricultural censuses. Notice that $\log(1-\beta_i)$ is the share of land in the production of crop $i$, a crop-specific characteristic that can be empirically captured by a crop fixed effect. $\log(r_j)$ is the region-specific price of land, which is in turn captured by a regional fixed effect. The regression form of (3.7) then becomes

$$\log(P_{i,j}Y_{i,j}) = \gamma_j + \delta_i + \alpha \log(T_{i,j})$$

Using the estimates of the crop fixed effects $\delta_i$, it is then straightforward to back out the share of labor $\beta_i$, since from the structural model $\delta_i = -\log(1-\beta_i)$. In practice, since one of the crop fixed effects will be dropped in the estimation, we express the labor shares of the rest relative to that numeraire.

We estimate the above equation using data from the 1886 Prussian agricultural census, the earliest historical census that we are aware of which provides information on crop-specific yields per unit of land harvested for a number of food crops. We have data on total output and output per hectare for wheat, barley, rye, oats, potato, field bean and pea for 518 Prussian counties (Kreise). We combine this with price information from the same year collected by the Prussian Statistical Office. Price information is not available at the county level, so our estimation rests on the assumption that agricultural output prices are equalized across Prussia. Normalizing the labor share of wheat to equal 0.4, we derive estimates for the labor shares of the remaining crops, presented in Table 3.1.[6] It is reassuring for our choice of specification that the estimate of $\alpha$ is statistically not distinguishable from one with high levels of confidence, as theory would suggest.

[6]We choose 0.4 for the labor intensity of wheat production, following Clark (2002) and Allen (2005), who both estimate a value close to 0.4 for labor's share of income in wheat, using historical data from England. The estimates of relative labor intensities do not depend on the specific value chosen for this normalization.

Table 3.1: Estimates of crop-specific labor shares from Prussian agricultural data

| | Wheat | Rye | Barley | Oat | Potato | Bean | Pea |
|---|---|---|---|---|---|---|---|
| Labor share | 0.400 | 0.149 | 0.079 | 0.370 | 0.571 | 0.601 | 0.299 |

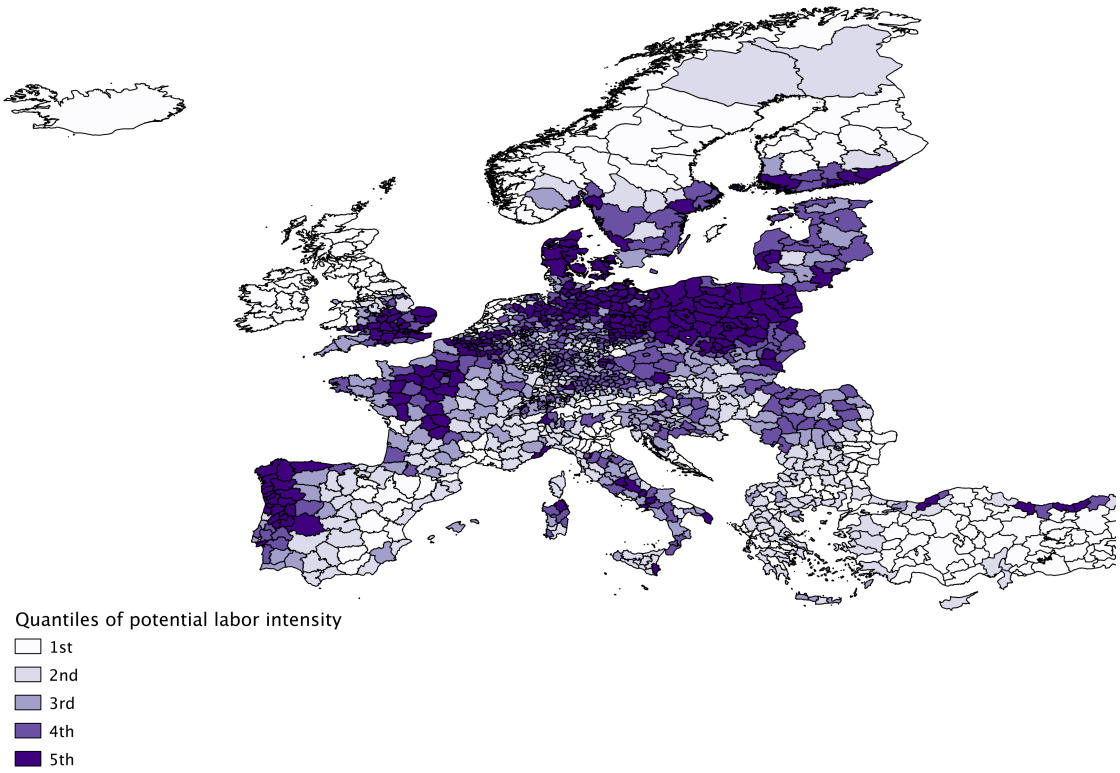
Having obtained a measure of the share of labor in the production of these 7 crops, under the assumptions previously laid out, we proceed to construct our main variable of interest, an index of *potential labor intensity*. We use data on agroclimatic suitability for each crop from FAO's Global Agro-Ecological Zones Database (Fischer et al., 2002)[7] and combine them with the estimated labor shares in an index of the form

$$\text{Potential labor intensity}_r = \sum_i \beta^i \frac{\text{suitability}_{ir}}{\sum_j \text{suitability}_{jr}}$$

where $r$ indexes regions and $i$ indexes crops. The index for each region is a weighted average of the relative suitabilities for different crops, where the weights are the crops' labor intensities. We normalize this to take on values from 0 to 100. The intuition behind it is that labor intensity will more likely be higher in a region that is relatively more suitable for more labor intensive crops. Figure 3.1 depicts the distribution of potential labor intensity across European regions. There is significant variation both across and within countries. In the following section, we will investigate whether this variation predicts various regional-level proxies of attitudes towards work in Europe today.

[7]The database reports the suitability of each 5 by 5 arc-minute grid cell globally for the cultivation of different crops. The model used to compute it considers each crop's technical production requirements and their interaction with each location's land and agroclimatic resources and constraints. In the empirical analysis we will directly control for the most important factors that affect suitability of a location for any given crop, such as temperature, precipitation, slope or altitude, as well as for overall suitability for rainfed agriculture.

Figure 3.1: Potential labor intensity in the regions of Europe



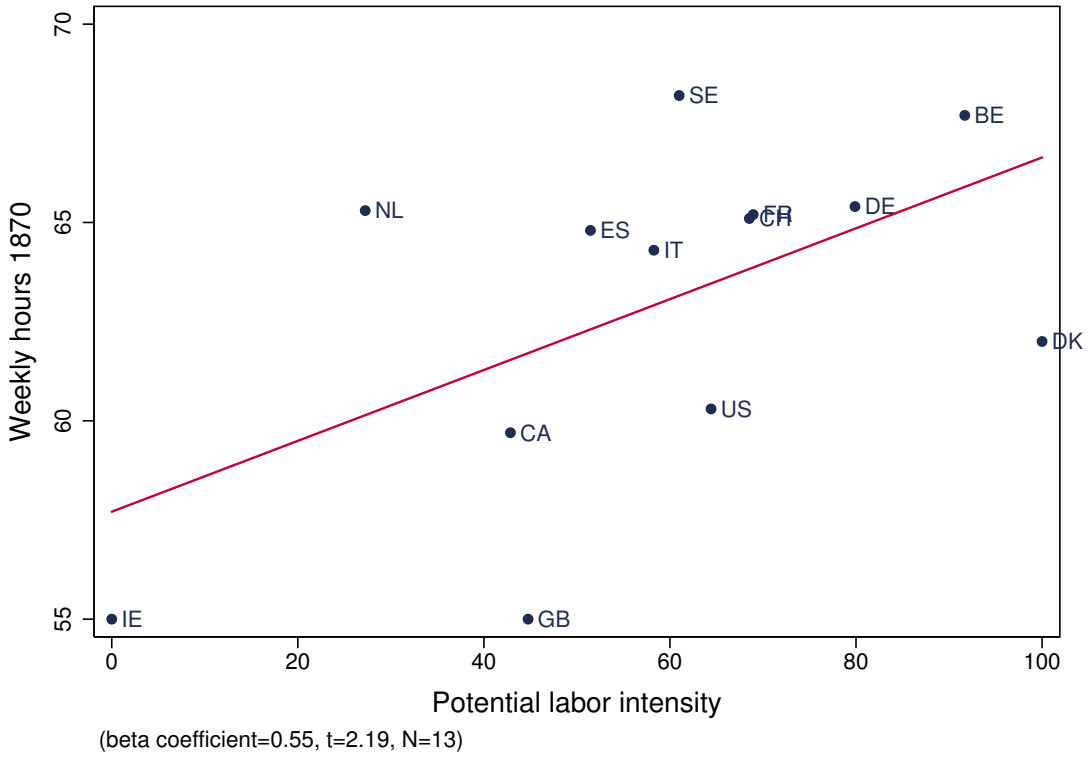

## 3.4 Empirical Analysis

### 3.4.1 Main results

Before examining whether potential labor intensity is correlated with contemporary work ethics, it would be desirable to show that the intermediate link between labor intensity and attitudes, namely hours worked in the past in societies dependent on agriculture, also holds. Unfortunately, work time is a variable that is rarely recorded in official statistics and for which only fragmentary estimates exist for pre-industrial times (Voth, 1998). Some early systematic estimates of hours worked come from Huberman (2004), who reports average weekly work hours in 1870 for a small number of European and North American countries. Though these do not refer specifically to agricultural labor, Figure 3.2 shows that they are positively correlated with potential labor intensity at the country level. Despite

the small number of observations, the positive correlation lends credit to our hypothesis, particularly because it is documented for a time period when no welfare regulation or restrictions on work time were yet in place in developed nations.

Figure 3.2: Potential labor intensity and historical hours worked




**Notes:** Estimates of average hours of work per week are from Huberman (2004). They are largely based on historical reports of the U.S. Department of Labor and are averaged across genders and five economic sectors: Mining and Construction, Iron and Steel, Textile, Manufacturing and Services.

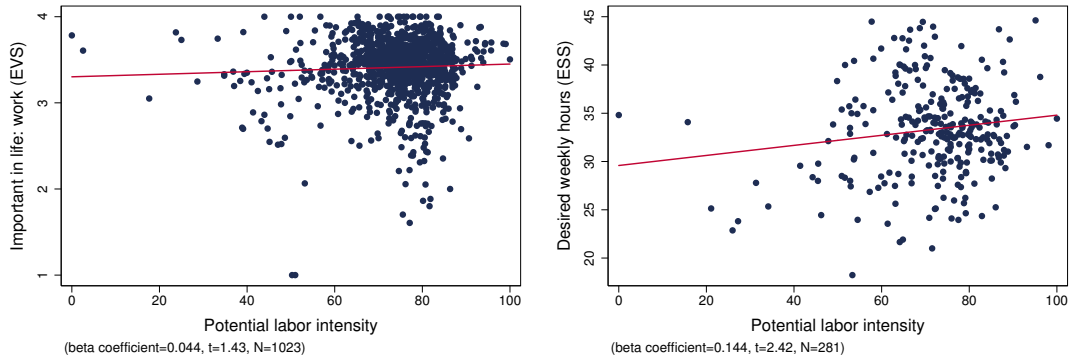
Our main analysis uses data from two European surveys that provide information on attitudes at the regional (NUTS) level. The European Values Study (EVS) asks interviewed subjects "Please say how important is work in your life". Answers take on one of four values: 1 "Very important" 2 "Quite important" 3 "Not important" 4 "Not at all important". We use information from 4 waves of EVS (1981-1984, 1990-1993, 1999-2001 and 2008-2010) and recode the variable so that higher numbers are associated with a higher work ethic. The 5th round of

the European Social Survey (ESS), conducted in 2010, asks individuals to report the number of hours they would ideally choose to work weekly. The question is phrased "How many hours a week, if any, would you choose to work, bearing in mind that your earnings would go up or down according to how many hours you work?". Tables 3.1 and 3.2 in the Appendix report summary statistics for these measures and for the rest of the variables included in the empirical analysis. Figure 3.3 shows that there is a positive correlation between the regional averages of these two variables and potential labor intensity.
Our main specification is

$$Y_{irc} = \alpha + \beta \text{Potential labor intensity}_{rc} + X_{irc}\gamma_1 + Z_{rc}\gamma_2 + \theta_c + \epsilon_{irc}$$

where $Y_{irc}$ is the outcome variable for individual $i$ living in region $r$ of country $c$, $X_{irc}$ is a vector of individual socioeconomic controls, $Z_{rc}$ a vector of regional geographic and economic controls and $\theta_c$ is a country fixed effect.

Figure 3.3: Bivariate correlation plots




We run our baseline regressions at the individual level, to exploit the extra precision added by the inclusion of individual controls that are important in determining the importance of work and the chosen number of weekly work hours for study subjects. Both in the EVS and in the ESS sample, we control for gender, age, age squared, indicators for the level of completed education, for marital status, employment status and religious affiliation. The ESS also provides information on whether respondents live with their children, as well as on respondents'

family income, classified in ten categories, which we include as additional controls. Table 3.2 gives the baseline estimate of the effect of potential labor intensity on the importance of work (Column [1]) and desired work hours (Column [4]), controlling only for these individual factors. This effect is small in magnitude, but statistically significant. In the case of desired work hours, a standard deviation increase in potential labor intensity increases desired work time by 20 minutes per week.

Since potential labor intensity is a measure constructed on the basis of relative suitabilities for different crops, there is a concern that it captures some of the geographic and climatic factors that determine these suitabilities. To address this concern we control in columns [2] and [5] for a number of potentially important geoclimatic variables. Temperature, precipitation, the slope of the terrain and altitude, are all determinants of crop suitability considered in the FAO models. We control for these variables, as well as for a general index of suitability for rainfed agriculture from FAO. We also include controls for latitude and longitude, as well as a dummy for landlocked regions, to capture other spatial patterns that potentially affect work ethics, but are not related to labor intensity. Including these controls increases the size of the estimated coefficient without affecting its significance.

Country fixed effects capture factors affecting attitudes towards work that differ at the country level, such as labor laws and collective agreements, unemployment and welfare provision, as well as GDP, a variable strongly negatively correlated with the number of actual worked hours at the country level. In columns [3] and [6], we include a number of additional economic controls at the regional level: the log of regional income measured in 2007 and its square, unemployment in 2007 and the share of the labor force employed in agriculture, industry and services measured in 2008.[8] These controls affect little the magnitude of the estimated coefficients.

[8]Data for these variables come from the ESS and the chosen years are the ones for which we have the fewest missing values.

Table 3.2: Individual-level OLS estimates using data from EVS and ESS

| Dep. variable | Work important | | | Desired weekly work hours | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Potential | 0.00173*** | 0.00187*** | 0.00179** | 0.0247* | 0.0298* | 0.0343** |
| Labor Intensity | (0.000607) | (0.000678) | (0.000659) | (0.0123) | (0.0164) | (0.0146) |
| | [0.000564] | [0.000589] | [0.000612] | [0.0121] | [0.0141] | [0.0120] |
| Observations | 71037 | 71037 | 44607 | 13561 | 13561 | 12331 |
| R-squared | 0.0888 | 0.0890 | 0.0997 | 0.217 | 0.218 | 0.237 |
| Ind. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Geo. controls | No | Yes | Yes | No | Yes | Yes |
| Econ. controls | No | No | Yes | No | No | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. All regressions include country fixed effects and control for gender, age and age squared, two dummies for secondary and tertiary education, a dummy for married individuals, a dummy for employed (ESS) or unemployed (EVS) individuals and dummies for recorded religious groups (EVS: catholic, protestant, orthodox, jewish, muslim, hindu. ESS: catholic, protestant, orthodox, jewish, muslim, other Christian religions and other non-Christian religions). The ESS sample includes a control for whether children are present in the household and additional indicators for 10 income categories. Regressions with the EVS sample include survey year fixed effects. Columns [2], [3], [5] and [6] control for regional mean temperature, mean precipitation, mean terrain slope index, mean altitude, latitude, longitude, suitability for rainfed agriculture and a dummy for landlocked regions. Columns [4] and [6] control for log GDP per capita and log GDP per capita squared in 2007, unemployment in 2007, share employed in agriculture, industry and services in 2008. Standard errors in parentheses are clustered at the country level. Conley standard errors adjusted for spatial correlation are reported in brackets.

### 3.4.2 Persistence and cultural transmission

Cultural transmission is an important part of our story. Part of the work ethic is transmitted from parents to children and this vertical socialization mechanism is important both in the past, when returns to labor in agriculture determined optimal effort, but also potentially today, when work attitudes persist because of interaction with institutions or similar mechanisms. This suggests that our baseline estimates should become more precise if we remove from the sample immigrants, whose place of origin has potentially very different labor intensity from that of the region in which they currently live. We do this in Table 3.3. Columns [1] and

[4] report our baseline regression with individual and regional controls. Columns [2] and [5] restrict the sample to individuals born in the country in which they are interviewed. This increases the magnitude of the coefficients. In columns [3] and [6] we further exclude second generation immigrants by restricting the sample to native-born individuals, whose parents are also native-born. This makes the coefficient of the EVS sample smaller — though still larger than in the baseline — but further increases the coefficient in the ESS sample.

Table 3.3: Individual-level OLS estimates for natives using EVS and ESS data

| Dep. variable | Work important | | | Desired weekly work hours | | |
|---|---|---|---|---|---|---|
| | Entire sample | Native-born | Native-born of native parents | Entire sample | Native-born | Native-born of native parents |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Potential | 0.00179** | 0.00254** | 0.00224** | 0.0343** | 0.0409** | 0.0438** |
| Labor Intensity | (0.000659) | (0.000904) | (0.000841) | (0.0146) | (0.0171) | (0.0178) |
| | [0.000612] | [0.000953] | [0.000946] | [0.0120] | [0.0140] | [0.0135] |
| Observations | 44607 | 27930 | 25988 | 12331 | 10982 | 10164 |
| R-squared | 0.0997 | 0.106 | 0.110 | 0.237 | 0.240 | 0.245 |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. All regressions include country fixed effects and control for gender, age and age squared, two dummies for secondary and tertiary education, a dummy for married individuals, a dummy for employed (ESS) or unemployed (EVS) individuals and dummies for recorded religious groups (EVS: catholic, protestant, orthodox, jewish, muslim, hindu. ESS: catholic, protestant, orthodox, jewish, muslim, other Christian religions and other non-Christian religions). The ESS sample includes a control for whether children are present in the household and additional indicators for 10 income categories. Regressions with the EVS sample include survey year fixed effects. We also control in all columns for regional mean temperature, mean precipitation, mean terrain slope index, mean altitude, latitude, longitude, suitability for rainfed agriculture, a dummy for landlocked regions, log GDP per capita and log GDP per capita squared in 2007, unemployment in 2007, share employed in agriculture, industry and services in 2008. Standard errors in parentheses are clustered at the country level. Conley standard errors adjusted for serial correlation are reported in brackets.

To further assess the role of cultural transmission, we look at the children of immigrants in the US (Fernández and Fogli, 2006, 2009). Our measure of potential labor intensity is computed with European data and ignores a large number of crops that have for centuries constituted important staples for many societies outside of Europe, such as rice or corn. For this reason, we restrict our analysis to

individuals whose parents migrated to the US from Europe. We use information from the Current Population Survey for the years 2008–2010 and estimate the effect of potential labor intensity in the parental country on average weekly hours worked in the main and secondary occupation for a sample of employed second generation immigrants.[9]

Table 3.4: Weekly hours worked for children of European immigrants in the US

| Dep. variable: | Father's country | | Mother's country | |
|---|---|---|---|---|
| | Average weekly hours worked | | | |
| | [1] | [2] | [3] | [4] |
| Potential Labor Intensity | 0.205** | 0.259** | 0.255*** | 0.286*** |
| | (0.0923) | (0.106) | (0.0918) | (0.101) |
| | [0.0963] | [0.0752] | [0.0881] | [0.0808] |
| Observations | 1753 | 1753 | 1780 | 1780 |
| R-squared | 0.223 | 0.226 | 0.205 | 0.209 |
| Individual controls | Yes | Yes | Yes | Yes |
| Origin country economic controls | Yes | Yes | Yes | Yes |
| Origin country geographic controls | No | Yes | No | Yes |
| State of residence fixed effects | Yes | Yes | Yes | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. The sample includes only employed individuals. All regressions control for gender, age and age squared, two dummies for secondary and tertiary education, a dummy for married individuals, a dummy for metropolitan status, dummies for 16 family income categories, state of residence fixed effects, survey year fixed effects, country of origin log GDP per capita and log GDP per capita squared, unemployment rate and share of the labor force employed in agriculture (all measured in 2005). Columns [2] and [4] include country of origin controls for mean temperature, mean precipitation, mean terrain slope index, mean altitude, latitude and suitability for rainfed agriculture from FAO. Standard errors in parentheses are clustered at the country of origin level. Conley standard errors adjusted for spatial correlation are reported in brackets.

[9]The General Social Survey, the main US attitudinal survey, keeps only a general record of ethnicity, as the country where an individual's ancestors came from. In this way, second and higher generation immigrants are pooled together. Potential labor intensity in the origin country of individuals with European ancestors is weakly positively correlated with their reported agreement with the statement "Work is a person's most important activity", but this correlation is not statistically significant.

Table 3.4 reports the results. Columns [1] and [2] consider labor intensity in the father's country of origin and columns [3] and [4] repeat the estimations for the country of the mother. As before, we control for a number of individual characteristics affecting work hours, such as gender, age, age squared, dummies for secondary and higher education, a dummy for metropolitan status, a dummy for married individuals and indicators for categories of total family income. We also include fixed effects for the state of residence of individuals within the US. Columns [1] and [3] control for log GDP per capita in the country of origin and its square, as well as for the unemployment rate and the share of the labor force employed in agriculture. Columns [2] and [4] add the host of geographic and climatic controls included in our baseline regressions computed at the country level. The estimated effect of the mother's country is larger than that of the father. An increase of one standard deviation in the potential labor intensity of the father's country increases weekly worktime by 34 minutes; the same increase for the mother's country leads to an increase of 43 minutes, a large and significant effect.

## 3.5 Robustness

Our results are robust to using alternative estimation methods. An ordered logit regression of the Work Important variable of EVS including all available controls yields a coefficient of 0.00518, significant at the 1 percent level. Since our variable of interest, potential labor intensity, varies at the regional level, we repeat our baseline estimation with European regions instead of individuals as the unit of observation. Results are presented in Table 3.5 and are qualitatively similar to the baseline estimates. The estimated magnitudes are slightly larger. In the specification with all controls, a standard deviation increase in potential labor intensity increases desired worktime by approximately 41 minutes per week.

A potential concern with our baseline measure of potential labor intensity is that the Prussian data used to compute it are not representative of optimal factor allocations to different crops. Furthermore, we use only one year of data, 1886, and though our estimation amounts to computing the average labor share across Prussian counties and thus removes some idiosyncratic variation, it is still possible that 1886 was a special year for Prussia in terms of average yields or crop prices.

More generally, it would be desirable to check whether our ranking of crops in terms of labor intensity holds when computed with different data.

Table 3.5: Regional-level estimates using data from EVS and ESS

| Dep. variable | Work important | | | Desired weekly work hours | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Potential | 0.00236* | 0.00231** | 0.00359*** | 0.0269* | 0.0483*** | 0.0512*** |
| Labor Intensity | (0.00135) | (0.00106) | (0.000926) | (0.0141) | (0.0168) | (0.0164) |
| | [0.00121] | [0.00107] | [0.00121] | [0.0141] | [0.0158] | [0.0145] |
| Observations | 1023 | 1023 | 799 | 281 | 281 | 268 |
| R-squared | 0.331 | 0.336 | 0.300 | 0.609 | 0.629 | 0.675 |
| Geo. controls | No | Yes | Yes | No | Yes | Yes |
| Econ. controls | No | No | Yes | No | No | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. Columns [2], [3], [5] and [6] control for regional mean temperature, mean precipitation, mean terrain slope index, mean altitude, latitude, longitude, suitability for rainfed agriculture and a dummy for landlocked regions. Columns [4] and [6] control for log GDP per capita and log GDP per capita squared in 2007, unemployment in 2007, share employed in agriculture, industry and services in 2008. Standard errors in parentheses are clustered at the country level. Conley standard errors adjusted for spatial correlation are reported in brackets.

To address these concerns, we turn to the US Census of Agriculture, which provides information on crop yields by unit of land at the county level, from 1880 onwards. We use three census-years: 1880, 1890 and 1900 and repeat the estimation of labor shares for each crop described in Section 3, this time including census-year fixed effects. This alternative measure is not perfect: the US Census does not list information for all crops available in the Prussian one, but only for potato, wheat, rye, oats and barley. There is also the concern that US agriculture in the period 1880-1900 was more mechanized than that of Prussia in 1886, so that capital might play a bigger role in the production of some crops and confound our results. Nevertheless, the US data yield a very similar ranking of crops as the Prussian ones. With the exception of barley, that is now more labor intensive than all other three cereals, the remaining crops retain their ranking. What is

important, the potato is again significantly more labor intensive than cereals.

An additional advantage of using US data is that we can directly compare the resulting ranking of crops to estimates of crop-specific labor requirements from available agronomic studies. Cooper (1916) reports man-hours per acre of land for various field crops in the US for the period 1902–1912. He finds the highest labor requirement for potatoes, followed by corn. Oats, barley and wheat require a very similar, generally low, number of average man-hours, while rye is the least labor intensive crop; the ordering in terms of labor intensity is practically identical to that produced by our estimation.

Table 3.6: Potential labor intensity estimated with US data

| Dep. variable | Work important | | | Desired weekly work hours | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Potential | 0.00137* | 0.00123* | 0.00103* | 0.0328*** | 0.0298** | 0.0403*** |
| Labor Intensity | (0.000755) | (0.000703) | (0.000575) | (0.00987) | (0.0135) | (0.0125) |
| | [0.000569] | [0.000565] | [0.000555] | [0.0139] | [0.0160] | [0.0132] |
| Observations | 71037 | 71037 | 44607 | 13561 | 13561 | 12331 |
| R-squared | 0.0888 | 0.0890 | 0.0996 | 0.217 | 0.218 | 0.237 |
| Ind. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Geo. controls | No | Yes | Yes | No | Yes | Yes |
| Econ. controls | No | No | Yes | No | No | Yes |

**Notes:** Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1. All regressions include country fixed effects and control for gender, age and age squared, two dummies for secondary and tertiary education, a dummy for married individuals, a dummy for employed (ESS) or unemployed (EVS) individuals and dummies for recorded religious groups (EVS: catholic, protestant, orthodox, jewish, muslim, hindu. ESS: catholic, protestant, orthodox, jewish, muslim, other Christian religions and other non-Christian religions). The ESS sample includes a control for whether children are present in the household and additional indicators for 10 income categories. Regressions with the EVS sample include survey year fixed effects. Columns [2], [3], [5] and [6] control for regional mean temperature, mean precipitation, mean terrain slope index, mean altitude, latitude, longitude, suitability for rainfed agriculture and a dummy for landlocked regions. Columns [4] and [6] control for log GDP per capita and log GDP per capita squared in 2007, unemployment in 2007, share employed in agriculture, industry and services in 2008. Standard errors in parentheses are clustered at the country level. Conley standard errors adjusted for spatial correlation are reported in brackets.

We use the US-based estimates of crop-specific labor intensity to recompute our measure of potential labor intensity at the regional level in Europe. Repeating the baseline estimation with the new measure yields coefficients that are both qualitatively similar and surprisignly close in magnitude to the baseline estimates. Column [4] of Table 3.6 shows that a standard deviation increase in potential labor intensity increases the number of desired hours by 0.448, or approximately 27 minutes (20 minutes in baseline). These results are reassuring and suggest that the measure of potential labor intensity is not an artifact of the data we use to compute it.

## 3.6 Discussion and conclusion

We have investigated the hypothesis that a high work ethic arises and persists in societies for which labor has historically been an important input in agricultural production. We have shown how a preference for work depends positively on the share of labor in production in the context of a model of endogenous preference formation. We have then attempted to quantify the relative labor input required in different crops using production data from 19th century Prussia and combined this information with agricultural suitability in an index of potential labor intensity. In our empirical analysis we show that this measure of potential labor intensity positively correlates with various proxies of a work ethic. Individuals from European regions that are relatively more suitable for labor intensive crops consider work more important in their lives and report higher desired hours of work, controlling for country fixed effects, individual factors and regional economic and geographic characteristics. This effect is generally stronger for individuals native to their region of residence. US natives with European-born parents also work more hours when their parents come from countries with a higher potential labor intensity, a result that offers some support to a cultural transmission mechanism.

There are a number of caveats in the way we measure labor intensity. We derive the share of labor in each crop using a structural estimation of the first order condition of a farmer's optimization problem. To the extent that the farmer's objective function is not a Cobb-Douglas with constant returns to scale, misspecification will be an issue for our estimates. The presence of capital and the differential

possibility of mechanization across crops are also important concerns. In practice, our estimation backs out the share of labor through a crop fixed effect, which is taken to proxy for the share of labor in the total value of production after the contribution of land has been controlled for. This will be a good proxy for the labor share if crop-specific capital inputs matter relatively little. This is not very unlikely in the context of traditional agriculture, as it was practiced for centuries in Europe, before the introduction of mechanization and agronomic improvements. In the context of modern agriculture, crop-specific capital usage will be more relevant, but not necessarily problematic for our estimates. Since mechanization has been a far more important labor-saving factor for land-intensive cereals than for labor-intensive tubers such as the potato (Knowlton et al., 1938; Elwood et al., 1939), it is likely that, by abstracting from capital, we overestimate the labor intensity of cereals and thus compress the true difference in labor intensity between them and the potato. Controlling for capital would show e.g. wheat to be even less labor-intensive than we now find it to be. In any case, it is reassuring that at least our ordering of crops in terms of labor intensity seems to be confirmed by existing estimates of labor requirements, expressed as man-hours per unit of land.

In the same way that crop-specific capital inputs might bias our labor share estimates, any crop-specific unobserved factor will have a similar effect. Volatility and risk, to the extent that they are more important for some crops than for others, are an example of such a factor. Furthermore, we would expect the effect of labor intensity on the work ethic to be affected not just by the crop-specific, but also by the overall volatility of production. Returns to labor are lower when farmers are more uncertain of their total output, and so is the incentive to invest in a preference for work. Studies of peasant culture suggest indeed that fatalism and the belief that no amount of hard work can improve the peasants' situation decrease significantly when production becomes more predictable, for example through the introduction of irrigation that reduces dependence on rainfall (Arkush, 1984; Ortiz, 1971).

We presently consider only a small number of staples, for which historical production data was available. Variation in the measure of potential labor intensity is therefore driven mainly by variation in suitability for potato and beans, relatively to suitability for cereals. The difference in labor intensity between cereals is relatively small and the degree of substitutability of those cereals in production

is relatively high, so that areas that are e.g. suitable for wheat can easily switch to barley. Despite the limited number of crops and sources of variation, we capture the most important staples in the European diet since the introduction of the potato to the Old World (Weiss Adamson, 1995). To extend our analysis outside Europe, we would need data on other crops that constituted important staples for different regions of the world, such as rice or maize.

Throughout history, different forms of ownership structures and farm labor relationships, such as feudal serfdom and slavery, have been important. To the extent that farmers under serfdom are forced to work longer hours than they otherwise optimally choose for themselves, without benefiting from the extra consumption, the incentive of parents to transmit a work ethic to their children will be lowered. On the other hand, longer demanded work hours offer parents a direct incentive for making their children hard-working and reducing their future disutility, so that the total effect of serfdom or slavery on work ethics will be ambiguous. In any case, regional differences in labor intensity within serfdom should still lead to differences in work attitudes. Labor intensive crops demand a higher labor input, even if that is chosen by the feudal lord and not the serf himself. If the nature of production forces children of serfs to work hard, then a higher work ethic will be beneficial for them.

Our theoretical framework was simple and used to demonstrate how the formation of a work ethic depends on the equilibrium labor share in an agricultural economy. We have not investigated theoretically how the work ethic persists once agriculture stops being the most important economic activity. One way in which this persistence can be explained is through the interaction of the work ethic with institutions, such as redistribution. If redistributive policies are chosen through majority voting, a society with high work norms will be more likely to choose low tax rates; individuals will then rely more on their own labor than on welfare, thus having an incentive to maintain a high work ethic. Such models of multiple steady states, in which institutions interact with work culture have been proposed by Bisin and Verdier (2004), Alesina and Angeletos (2005) and Bénabou and Tirole (2006).

Finally, an interesting note concerns the relationship between actual hours worked and attitudes towards work. In our data, average actual worked hours

are uncorrelated with the reported importance individuals place on work. This seems to cast doubt on the usefulness of this reported measure as a proxy for work values. On the other hand, reverse causality is an issue, with work values both affecting and being affected by employment status and actual work hours. Our theory suggests that potential labor intensity is a valid instrument for variables measuring work attitudes, since it only affects contemporary outcomes through the formation of a work ethic. An IV regression of actual hours worked on the reported importance of work, instrumented by potential labor intensity, yields a large positive and significant coefficient; this is the effect we would expect with an instrument that addresses reverse causality and is reassuring for the validity of our hypothesis and attitudinal measures.

## 3.7 Appendix A: Model extensions

### 3.7.1 Work ethics and fertility choice

To investigate how population growth affects the results of our baseline model, we introduce an endogenous fertility choice following the literature on Malthusian growth (see for example Barro and Becker (1989)). The number of children is denoted by $n$, and they can be raised at a cost $\theta(n)$, $\theta_n > 0, \theta_{nn} \leq 0$, paid in consumption goods. The family holding of land is denoted with $t$ and is distributed equally among children. Finally, $N$, $H = hN$ and $T = tN$ denote total population, aggregate labor and land supply, respectively.

A representative parent solves the program

$$
\begin{aligned}
V(\gamma, t; N) = \max_{c,h,n,I} & \left\{ \log(c) - \frac{1}{\gamma}\frac{h^{1+\phi}}{1+\phi} - nI + a(n)V(\gamma', t'; N') \right\} \\
& s.t. \\
& c = wh + r_T t - \theta(n) \\
& \gamma' = \rho\gamma + \Psi(I) \\
& t' = \frac{t}{n}
\end{aligned}
$$

The first order and envelope conditions of this problem are

$$\begin{aligned}
\frac{w}{c} &= \frac{1}{\gamma}h^{\phi} \\
n &= a(n)V'_{\gamma}\Psi_I \\
\frac{\theta_n}{c} + I &= a_n V' - a(n)V'_t\frac{t}{n^2} \\
V_{\gamma} &= \frac{1}{\gamma^2}\frac{h^{1+\phi}}{1+\phi} + a(n)V'_{\gamma}\rho \\
V_t &= \frac{r_T}{c} + a(n)V'_t\frac{1}{n}
\end{aligned}$$

which together with the aggregate conditions

$$\begin{aligned}
H &= hN \\
T &= tN \\
Y &= AT^{1-\beta}H^{\beta} \\
w &= \beta\frac{Y}{H} \\
r_T &= (1-\beta)\frac{Y}{T} \\
c &= \frac{Y}{N} - \theta(n) \\
N' &= nN
\end{aligned}$$

define the equilibrium outcome. Notice that again the first order condition for labor, together with the wage equation and the resource constraint, implies that all else equal, a high labor intensity $\beta$ will induce a higher equilibrium labor supply.

In the steady state with zero population growth, we have $n = 1$ and the

dynamic equations hence become

$$\gamma = \frac{\Psi(I)}{1-\rho} \tag{3.8}$$

$$V_\gamma = \left(\frac{1}{1-a(1)\rho}\right)\frac{1}{\gamma^2}\frac{h^{1+\phi}}{1+\phi} \tag{3.9}$$

$$V_t = \frac{1}{1-a(1)}\frac{r_T}{c} \tag{3.10}$$

$$\frac{\theta_n(1)}{c} + I = a_n(1)V - a(1)V_t t \tag{3.11}$$

$$1 = a(1)V_\gamma \Psi_I \tag{3.12}$$

$$\frac{w}{c} = \frac{1}{\gamma}h^\phi \tag{3.13}$$

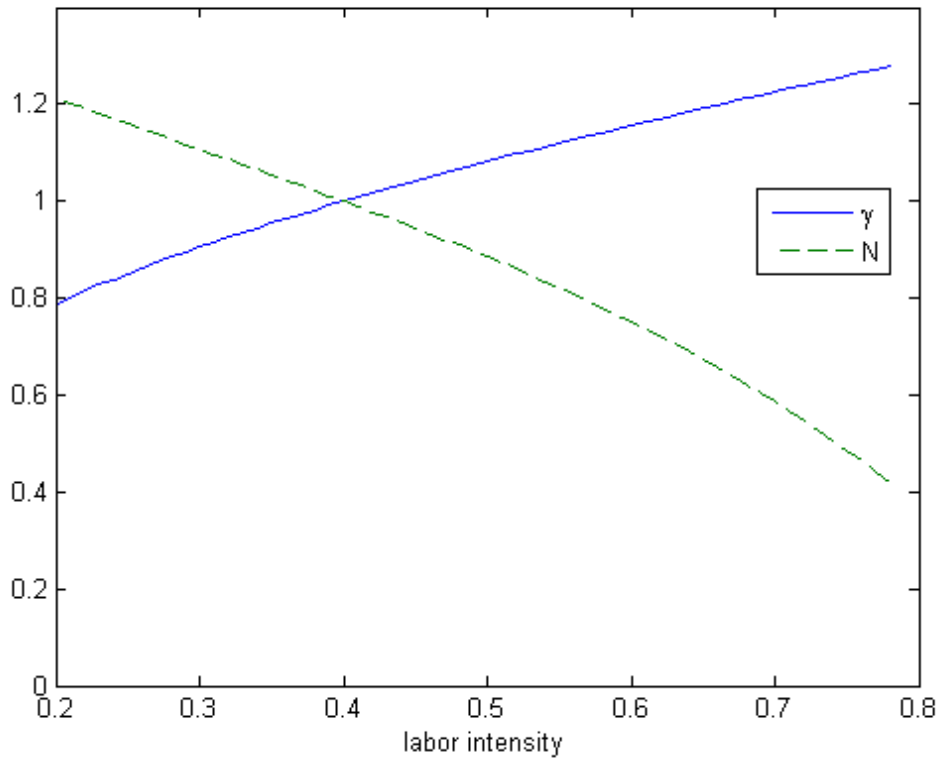
While this model does not have a general analytical solution, we can analyze for illustration purposes the special case in which $\theta(1) = 0$, i.e. the theoretical case in which the first child contributes as much to income as it costs. Under this assumption, labor supply collapses to $h = (\beta\gamma)^{\frac{1}{1+\phi}}$. Together with (3.8), (3.9) and (3.12), this implies that work ethics behave exactly the same as in the basic model and are hence increasing in the labor intensity of production.

We obtain numerical solutions for the steady state using Dynare for the general case. We investigate a large region of parameter values and functional form, and robustly get the result that the work ethics are a strictly increasing function of labor intensity. Figure 3.4 illustrates how work ethics and population size depend on $\beta$, where the shown specification assumes a linear cost function of children $\theta(n) = 0.4n$. Interestingly, and the flip side of the result for work ethics, population is decreasing in labor intensity. As a work ethic becomes more valuable with high $\beta$, parents invest more in their children but reduce the quantity of children they have.

### 3.7.2 Work ethics in subsistence agriculture

In the theoretical results up to now, labor productivity had no effect on hours worked and the development of work ethics, since the income and substitution effect cancel out. Instead, labor intensity was the sole determinant of attitudes

Figure 3.4: Work ethics and population size




**Notes:** Steady state values as a function of $\beta$. Both series are normalized such that they equal one at $\beta = 0.4$.

towards work.[10] In this section, we briefly review the case of a subsistence agriculture by introducing a minimum consumption constraint into the basic model, and discuss how this affects the role of productivity and labor intensity.

Consider again the basic model, extended with a minimum consumption requirement $c \geq \underline{c}$. We define a subsistence economy as an economy in which the

[10] An interesting example of this, within the context of a single crop, has been documented by Barker et al. (1985). Due to differences in the geographic and technological structure across rice farms in selected villages of Indonesia, Taiwan and the Philippines around 1970, the average labor productivity was lowest in Indonesia, followed by the Philippines and Taiwan. The authors estimate however that, for a given amount of labor input, the marginal product is higher in Indonesia and Taiwan than it is in the Philippines. They then use village level data to show that rice farmers work significantly less hours in the Philippines than in the other two regions. Returns to labor seemed to dominate labor productivity as a determinant of effort, as in the baseline version of our model.

hours optimally worked in the steady state of the unconstrained model are not sufficient to satisfy the minimum consumption requirement, i.e. $AT^{1-\beta}h_{ss}^{\beta} < \underline{c}$, $h_{ss} = (\beta\gamma_{ss})^{\frac{1}{1+\phi}}$. In this case, the equilibrium labor supply is given by

$$h = \frac{1}{T}\left(\frac{\underline{c}}{AT}\right)^{\frac{1}{\beta}} \tag{3.14}$$

Hours worked now depend negatively on the aggregate productivity $A$. If productivity is low in a subsistence economy, individuals will need to work more. By the same logic as discussed in the previous section, this increases the return to having a high work ethic and hence to parental investment. It follows that low productivity leads to a high steady state work ethic.

The effect of labor intensity is now ambiguous. Notice that $\beta$ only has a positive effect on hours worked if the term in brackets of (3.14) is less then one, i.e. $AT > \underline{c}$. This result is similar to the one in Vollrath (2011), and indicates that in subsistence agriculture, labor intensity only has a positive effect on hours and hence on work ethics once productivity is already sufficiently developed. Finally notice that the minimum consumption requirement can lead to a multiplicity of steady states when $\beta$ is sufficiently high, with one steady-state in the constrained region with low work ethics and one in the unconstrained region with high work ethics. This results from the fact that work ethics endogenously determine whether the consumption constraint is binding or not. We leave a deeper investigation of this topic, as well as potential interactions with an endogenous fertility choice, for further research.

## 3.8 Appendix B: Summary statistics

Table 3.1: EVS Sample

| Variable | Mean | Std. Dev. | Min | Max | Obs. |
|---|---|---|---|---|---|
| Work Important | 3.452 | 0.764 | 1 | 4 | 94055 |
| Potential labor intensity (Prussia) | 74.039 | 9.401 | 0 | 100 | 95804 |
| Potential labor intensity (US) | 56.086 | 11.121 | 0 | 100 | 95804 |
| Individual controls | | | | | |
| Female | 0.542 | 0.498 | 0 | 1 | 98717 |
| Age | 46.039 | 17.638 | 15 | 108 | 98400 |
| Secondary education | 0.439 | 0.496 | 0 | 1 | 74082 |
| Tertiary education | 0.211 | 0.408 | 0 | 1 | 74082 |
| Married | 0.583 | 0.493 | 0 | 1 | 98749 |
| Unemployed | 0.059 | 0.235 | 0 | 1 | 98749 |
| Catholic | 0.430 | 0.495 | 0 | 1 | 98749 |
| Protestant | 0.165 | 0.371 | 0 | 1 | 98749 |
| Orthodox | 0.098 | 0.297 | 0 | 1 | 98749 |
| Jewish | 0.0013 | 0.036 | 0 | 1 | 98749 |
| Muslim | 0.0284 | 0.165 | 0 | 1 | 98749 |
| Hindu | 0.0006 | 0.024 | 0 | 1 | 98749 |
| Geographic controls | | | | | |
| Temperature | 9.513 | 3.740 | −1.74 | 18.49 | 98749 |
| Precipitation | 784.166 | 246.496 | 0 | 2005.04 | 98749 |
| Terrain slope index | 7298.121 | 1928.62 | 0 | 9953.9 | 98749 |
| Altitude | 304.297 | 315.607 | 0.133 | 2307.261 | 98749 |
| Absolute latitude | 48.742 | 7.059 | 28.291 | 69.951 | 98749 |
| Longitude | 11.227 | 11.382 | −21.674 | 43.650 | 98749 |
| Suitability for rainfed agriculture | 4.231 | 1.539 | 0 | 8.17 | 98749 |
| Landlocked | 0.568 | 0.495 | 0 | 1 | 98749 |
| Economic controls | | | | | |
| Log GDP per capita 2007 | 9.902 | 0.690 | 7.650 | 11.037 | 63256 |
| Unemployment rate 2007 | 7.138 | 3.702 | 2.1 | 23.5 | 63256 |

Table 3.1: EVS Sample (cont.)

| Variable | Mean | Std. Dev. | Min | Max | Obs. |
|---|---|---|---|---|---|
| Share labor force in agriculture 2008 | 0.045 | 0.046 | 0 | 0.296 | 60888 |
| Share labor force in industry 2008 | 0.282 | 0.079 | 0.113 | 0.474 | 60888 |
| Share labor force in services 2008 | 0.665 | 0.100 | 0.436 | 0.879 | 60888 |

Table 3.2: ESS Sample

| Variable | Mean | Std. Dev. | Min | Max | Obs. |
|---|---|---|---|---|---|
| Desired weekly work hours | 32.335 | 15.587 | 0 | 144 | 32980 |
| Potential labor intensity (Prussia) | 72.095 | 13.592 | 0 | 100 | 43819 |
| Potential labor intensity (US) | 52.124 | 12.685 | 0 | 100 | 43819 |
| Individual controls | | | | | |
| Female | 0.537 | 0.499 | 0 | 1 | 43940 |
| Age | 48.594 | 18.704 | 14 | 101 | 43874 |
| Secondary education | 0.562 | 0.496 | 0 | 1 | 43855 |
| Tertiary education | 0.294 | 0.456 | 0 | 1 | 43855 |
| Married | 0.525 | 0.450 | 0 | 1 | 43855 |
| Children at home | 0.368 | 0.482 | 0 | 1 | 41724 |
| Paid work | 0.481 | 0.500 | 0 | 1 | 43961 |
| Income category | 5.118 | 2.807 | 1 | 10 | 33179 |
| Catholic | 0.508 | 0.500 | 0 | 1 | 27003 |
| Protestant | 0.225 | 0.417 | 0 | 1 | 27003 |
| Orthodox | 0.204 | 0.403 | 0 | 1 | 27003 |
| Jewish | 0.001 | 0.032 | 0 | 1 | 27003 |
| Muslim | 0.033 | 0.178 | 0 | 1 | 27003 |
| Other Christian | 0.208 | 0.143 | 0 | 1 | 27003 |
| Other non-Christian | 0.0054 | 0.074 | 0 | 1 | 27003 |
| Geographic controls | | | | | |
| Temperature | 9.292 | 3.603 | −1.74 | 18.45 | 43961 |
| Precipitation | 788.491 | 263.898 | 275.82 | 1739.55 | 43961 |
| Terrain slope index | 7460.282 | 1786.619 | 1471.55 | 9953.90 | 43961 |
| Altitude | 284.612 | 291.886 | 2.74 | 1673.7 | 43961 |
| Latitude | 49.326 | 6.979 | 28.342 | 68.851 | 43961 |

Table 3.2: ESS Sample (cont.)

| Variable | Mean | Std. Dev. | Min | Max | Obs. |
|---|---|---|---|---|---|
| Longitude | 11.619 | 11.244 | −15.669 | 33.227 | 43961 |
| Suitability for rainfed agriculture | 4.281 | 1.363 | 1.32 | 8.01 | 43961 |
| Landlocked | 0.498 | 0.500 | 0 | 1 | 43961 |
| Economic controls | | | | | |
| Log GDP per capita 2007 | 9.895 | 0.777 | 7.650 | 11.315 | 43961 |
| Unemployment rate 2007 | 6.779 | 3.479 | 1.9 | 23.5 | 43961 |
| Share labor force in agriculture 2008 | 0.049 | 0.046 | 0 | 0.296 | 41246 |
| Share labor force in industry 2008 | 0.286 | 0.082 | 0 | 0.474 | 41246 |
| Share labor force in services 2008 | 0.656 | 0.101 | 0.436 | 0.879 | 41246 |

Table 3.3: CPS Sample of second-generation immigrants

| Variable | Mean | Std. Dev. | Min | Max | Obs. |
|---|---|---|---|---|---|
| Weekly worked hours | 39.507 | 12.204 | 0 | 99 | 1876 |
| Potential labor intensity (Prussia) | 13.080 | 9.205 | 4.539 | 100 | 1819 |
| Individual controls | | | | | |
| Female | 0.495 | 0.498 | 0 | 1 | 1876 |
| Age | 44.591 | 13.280 | 15 | 85 | 1876 |
| Secondary | 0.952 | 0.214 | 0 | 1 | 1876 |
| Tertiary | 0.753 | 0.432 | 0 | 1 | 1876 |
| Married | 0.694 | 0.461 | 0 | 1 | 1876 |
| Metropolitan status | 0.946 | 0.227 | 0 | 1 | 1876 |
| Family income category | 12.606 | 3.200 | 1 | 16 | 1876 |
| Geographic controls | | | | | |
| Temperature | 9.153 | 4.808 | −6.16 | 25.29 | 1819 |
| Precipitation | 784.191 | 206.979 | 354.13 | 1460 | 1819 |
| Slope | 7279.616 | 1415.673 | 3662.53 | 9762.68 | 1819 |
| Altitude | 322.643 | 245.012 | 30.32 | 1292.25 | 1819 |
| Absolute latitude | 48.251 | 7.468 | 12.118 | 64.504 | 1819 |
| Suitability for rainfed agriculture | 4.466 | 1.151 | 1.9 | 7.53 | 1819 |
| Economic controls | | | | | |
| Log GDP per capita 2005 | 9.781 | 0.928 | 6.723 | 11.094 | 1819 |
| Unemployment rate 2005 | 8.767 | 4.023 | 4.3 | 37.3 | 1819 |

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