What Works for Immigrant Integration? Lessons from the Americanization Movement*

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Abstract

Which types of policies promote the social and political incorporation of immigrants? I address this question in the context of the Americanization movement, the concerted effort of state and non-state actors to culturally assimilate the large numbers of immigrants arriving to the US in the early 20th century. I offer a framework for conceptualizing the effects of integration policy packages, based on the relative role of incentives they offer and prescriptions they set for immigrant behavior. I illustrate the framework's insights through the causal evaluation of different types of Americanization initiatives, using linked census records on the universe of the foreign-born between 1910 and 1930, and samples of the second generation between 1930 and 1960. Initiatives that increase the benefits of integration are successful in promoting citizenship acquisition and increasing language proficiency and rates of intermarriage with the native-born. Prescription-based policies instead are either ineffective or counterproductive in promoting integration.

Keywords: Immigration, integration, policy, Americanization.

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During the second half of the 20th century, immigration and rising diversity emerged as fundamental challenges for Western societies. Alongside controls on immigrant inflows, integration policy is the main tool used by governments to manage both the economic effects of immigration and the social challenges that it poses for migrant receiving countries. Policy objectives may range from immigrants' labor market or political incorporation to full cultural assimilation. At a minimum, all countries intend to promote knowledge of the language and some degree of social integration of immigrant minorities. To achieve such integration objectives, a large range of policies have been employed historically and contemporaneously, with varying degrees of success.

Political scientists and sociologists have long taken interest in integration policies and their effects. A rich literature has developed indices to classify and quantify policies, focusing on various policy dimensions such as multiculturalism (Banting et al. 2006), rights granted to immigrants (Koopmans et al. 2005), or civic integration requirements (Goodman 2014). Studies making use of such indices to examine the effects of different policy packages on immigrant outcomes have reached mixed conclusions (Helbling and Michalowski 2017). Part of the difficulty in interpreting their results owes to their reliance on cross-country comparisons of immigrant outcomes, with differences attributed to differences in policy, making it hard to account for other country-level confounders (Koopmans 2013). Studies that place more emphasis on causal identification instead do so by narrowing the focus to single policy initiatives, in disparate country contexts and time periods (Avitabile, Clots-Figueras, and Masella 2013; Hainmueller, Hangartner, and Pietrantuono 2015, 2017). Their conclusions are not necessarily guided by an overarching framework of policy types, and are thus harder to generalize beyond their specific context.

This paper builds on both these bodies of work on immigrant integration, attempting to synthesize their approaches. I aim at making progress in identifying the types of initiatives that contribute to or hinder immigrant incorporation by causally evaluating the impact of different integration programs within a single unified context, that of the United States during the Americanization movement. The Americanization movement was a massive set of efforts undertaken by both state and societal actors to "Americanize" or assimilate the large numbers of – primarily European – immigrants who arrived to the US during the Age of Mass Migration (1850-1924). Though assimilationist in its objectives, the movement involved a great variety of approaches and mobilized multiple actors, from federal and state institutions, civil society clubs and organizations, to industrial employers and labor unions. This variation makes this period of US history a unique setup within which to examine how different approaches to immigrant integration fared in terms of their observed outcomes.

Focusing on this historical setup has two additional advantages. First, during the period examined, the US experienced the largest immigrant inflows in its entire history – rivaled only by those of the present day – with the share of the foreign-born reaching 14% between 1870 and 1920 (Abramitzky and Boustan 2017). Then, as today, nativism and concerns about the integration of culturally distant immigrants were at the forefront of political debate (Spiro 2008). As such, the Age of Mass Migration is perhaps the historical context with most parallels to today's immigration-related challenges in the US and Europe. Second, the period under focus allows for the use of rich data from the full universe of foreign-born residents of the US, as well as census linking methods, in order to track integration outcomes of immigrants in response to Americanization initiatives in both the short and the long-run.

To analyze the impact of different Americanization initiatives I propose a formal analytical framework based on rational choice, in which immigrants optimally choose the amount of effort to exert in order to integrate in the host society. Their choice is the result of a comparison of costs of effort and benefits to integration. I distinguish between *integration effort*, which is chosen by the immigrants, and *successful integration*, which also depends on acceptance by the native society. The former need not guarantee the latter, and policy may impact integration effort and success in different ways.

I conceptualize policy as a package, consisting of *prescriptions* and *incentives*. Prescriptions are target levels of effort required by immigrants in exchange for a reward. The reward constitutes the incentive side of the policy. For instance, learning the language of the host country is the prescriptive component of citizenship policy, which then rewards compliance with naturalization. Incentives can be negative, and take the form of punishment for non-compliance with prescriptions.

Altering either component of policy changes immigrants' optimal choice of effort – and subsequent chances of successful integration – but in different ways. Because immigrants differ in their cost of effort provision, both incentives and prescriptions have heterogeneous effects. Increasing the benefits of integration (or the costs of non-compliance), other things equal, increases integration effort and integration success for immigrants of intermediate costs, but does not change the behavior of those already exerting the requisite effort, or those who find effort provision very costly. Increasing the prescriptive component of policy instead may result in lower integration. On the one hand, prescriptions promote higher effort, as immigrants attempt to comply with higher requirements. On the other hand, effort can also be discouraged if prescriptions seem unattainable to some. Immigrants who struggle most to meet the new requirements may "give up" and reduce the integration effort they provide. While the average effects of incentives on effort and successful integration are thus positive or zero, average effects of prescriptions can be negative, if prescriptions are too high relative to the costs of effort of a large part of the immigrant population.

I illustrate these insights by empirically evaluating two policy examples, each relying relatively more on one of two policy components – incentives or prescriptions – in the context of the Americanization movement. I focus on outcomes that proxy for integration efforts (such as English proficiency), and successful integration (such as rates of intermarriage with the native-born).

To examine the effects of incentives, I analyze a characteristic instance of incentive-based Americanization program: the Five-Dollar Day plan of the Ford Motor Company introduced in the Highland Park Ford Plant in 1914. The plan was a profit-sharing scheme offered to workers conditional on requirements that included the attendance of English classes and the adoption of a lifestyle compatible with American middle-class values. Incentives offered by the program were steep relative to requirements, as the plan paid significantly higher wages than other auto manufacturers in Detroit or elsewhere. Using a linked sample of over two million foreign-born men and a triple-differences strategy, I compare within-person changes in the outcomes of Detroit and Highland Park auto workers to those of other industrial workers in cities with a Ford assembly plant before and after the introduction of the Five-Dollar Day plan.

Ford's program increased both immigrant effort and rates of political and social integration. The introduction of the plan is associated with an approximate 2 percentage points (differential) increase in the likelihood of speaking English for affected workers compared to the control group, which amounts to close to a quarter of the average increase in English proficiency among industrial workers between 1910 and 1920. Effects are even larger for naturalization rates, with Detroit auto workers experiencing a differential increase of 9 percentage points, equal to half of the average increase in naturalization rates experienced by industrial workers between 1910 and 1920. Rates of intermarriage with native-born women of native-born parents for men in marriageable age increased by 90% of their pre-period mean.

Consistent with the theoretical framework, the effects of the plan were heterogeneous. The largest increase in integration effort and success was experienced by workers whose native language was relatively distant from English. English-speaking immigrants experienced smaller or zero changes in their outcomes, consistent with the effect of incentives being decreasing in the cost of integration effort.

To illustrate the role of prescriptions, I evaluate the effects of English-only laws and foreign language bans enacted by US states in the period between 1890 and 1920.

These laws made the use of English a requirement for obtaining an education in the country's public schools. The increase in language requirements was not accompanied by additional incentives for school attendance. Using the 1930 5% and 1960 1% samples of census microdata, I examine the adult outcomes of US-born children of foreign-born parents. I identify those exposed to an English-only law based on their year of birth and the compulsory schooling age range in their state of birth. I first restrict attention to states that introduced English-only laws and compare cohorts exposed to such laws to untreated cohorts. English-only laws had no effect on English proficiency, and negatively impacted the probability of marriage to a native-born person of native-born parents. This result holds in a difference in differences comparison, and even within narrow control groups of adjacent states.

As with incentives, also in the case of prescriptions the average null effect on integration effort (proxied by English proficiency) masks heterogeneity consistent with theoretical predictions. Immigrants with lower costs of effort for learning English, such as those with an English-speaking or native-born mother, showed improvements in their use of English, and experienced smaller drops in intermarriage rates.

This paper makes five main contributions. First, it provides a new analytical framework to conceptualize the effects of different types of integration policies and policy-like initiatives. While there is no dearth of studies constructing typologies of integration policies[1], substantially less work exists on *how* policies affect immigrants and through what pathways they change their behavior. As highlighted by Goodman 2015, without theorizing on causal mechanisms behind policy effects, any estimated impact, even if causally identified, becomes hard to interpret. This paper highlights one set of causal mechanisms driving the effects of policy, by focusing on individual immigrant decision-making. I formalize the insight that integration can be understood as the outcome of rational choice (Laitin 1998; Adida 2014), and consider how policy enters immigrants'

[1]See Goodman 2015 for a complete survey of integration policy indices.

decision-making and the comparison of costs and benefits to integration effort.

Second, the study contributes to the empirical literature evaluating the effects of integration policy. A number of studies examine the correlation between immigrant outcomes and policy bundles (Koopmans 2010; Ersanilli and Koopmans 2011; Ersanilli 2012; Wright and Bloemraad 2012; Bloemraad and Wright 2014; Goodman and Wright 2015), or causally assess the effects of specific policies (Hainmueller, Hangartner, and Pietrantuono 2017, 2015). This paper explicitly contrasts the causal effects of different types of policies within the same temporal, geographic and institutional context, and one that is highly relevant for contemporary immigration debates.

Third, this study broadly contributes to a multidisciplinary literature on immigrant integration and assimilation. Traditionally the object of study of sociologists (Waters and Jiménez 2005), integration and assimilation have been extensively studied by political scientists (Hochschild et al. 2013; Goodman 2014; Harder et al. 2018) and economists (Abramitzky, Boustan, and Eriksson 2014). I build on work that treats integration as the outcome of rational choice (Laitin 1998, 1995; Alba and Nee 2009; Lazear 1999; Vigdor 2010), but add to the existing literature by explicitly modeling the role of integration policy and its interaction with individual decision-making.

Fourth, the paper contributes to the historical analysis of the effects of the Americanization movement. A large sociological and historical literature has analyzed the Americanization period (Hill 1919; Hartmann 1948), but few studies have provided causal evidence on the extent to which Americanization efforts were successful. Most of them have focused on the effects of compulsory schooling or language laws (Lleras-Muney and Shertzer 2015; Bandiera et al. 2019; Fouka 2020). The present paper extends the focus to other aspects of the Americanization movement, such as the role of industrial employers, with findings that contradict the received wisdom that all Americanization attempts were ineffective.

Finally, the study broadly contributes to the literature on nation-building. Most scholarship has focused on the historical creation of nation-states (Weber 1976; Hobsbawm 1990) and the politics of ethnic homogenization in multiethnic states (Mylonas 2012; Wimmer 2018). A smaller set of studies examine nation-building as it relates to the management of immigration flows (Shevel 2011; Kymlicka 2012). The paper adds to that literature by empirically studying the effects of different nation-building policies and private initiatives in the context of one of the large drives of the 20th century to manage ethnocultural diversity.

Conceptual framework

To understand the effects of Americanization initiatives, and of integration policy more generally, I sketch a simple formal framework that illustrates how different components of a policy bundle can have different effects on immigrant decisions and outcomes. Proofs of formal statements are provided in Section A of the Online Appendix.

An immigrant faces the decision of whether to provide *integration effort* e, at a cost $c(e)$. Effort refers to actions that are under the immigrant's control, such as learning the host country's language or adopting the mode of dress and behavior of the native-born. Costs of integration effort can be tangible – for example money and time spent on language study – or intangible – for example psychological costs sustained by immigrants who abandon certain elements of their culture in order to better fit into the host country's culture.

Effort does not guarantee acceptance by the host society. I thus distinguish between integration effort and *successful integration*.[2] The latter is the outcome of both effort and acceptance. The distinction between effort and successful integration is analytically useful because policies may affect effort and success differently. Successful integration

[2] I sometimes also refer to successful integration as integration success or simply integration. The outcomes I examine empirically (language proficiency, citizenship, rates of intermarriage with the native-born) capture various aspects of participation in the majority society. I thus refer to *integration*, as opposed to *assimilation*, throughout. However, the same framework can be applied to the study of assimilation decisions, when choices and behaviors under study capture immigrants' abandonment of their cultural practices in favor of practices used by the native population.

is achieved with probability $P(e)$, with $0 \leq P \leq 1$, $P_e > 0$, and $P_{ee} < 0$, and brings payoff $f > 0$. As with costs of effort, benefits to integration can be tangible (for example access to jobs) or intangible (for example satisfaction associated with social acceptance). Often, the benefit of integration is the avoidance of the cost of non-integration, such as in cases when non-integrated immigrants are discriminated or harassed.

I conceptualize integration policies as a package, consisting of *prescriptions* and *incentives*. Prescriptions set a minimum level of effort $\underline{e}$, for example by requiring that immigrants only use the language of the host society at school. Meeting the target entails a reward, which constitutes the incentive part of the policy package, and is denoted by h. Rewards can be positive (such as access to certain privileges) or negative (such as avoidance of penalties). For instance, in the case of civic integration policies (Goodman 2010, 2014), achieving a minimum target of effort by demonstrating knowledge of the host country's language and history, is rewarded with access to citizenship.

I assume that failing to meet the target of minimum integration effort required by a prescriptive policy substantively reduces the chances of successful integration in society more broadly. For instance, non-compliance with English-only laws (by not attending school) has severe downstream negative consequences for integration in the labor market and in society at large.[3] For simplicity, I normalize the probability of integration when immigrants exert effort $e < \underline{e}$ to zero. An immigrant's probability of successful integration then becomes

$$\Pi(e) = \begin{cases} P(e) & \text{if } e \geq \underline{e} \\ 0 & \text{if } e < \underline{e} \end{cases}$$

and the problem of the immigrant is given by

[3]This interaction between direct effects of a policy and effects in other domains of integration echoes the "increments of achievement" framework introduced by Goodman and Wright 2015. In the example above, obtaining an education in English is the first-order achievement, and later socioeconomic integration the second- or higher-order achievement, of an English-only school policy. Without the first-order achievement, higher-order achievements become significantly harder to attain.

$$\max_{e} \quad U(e) = \Pi(e)f - c(e) + h\mathbb{1}_{(e \geq \underline{e})}$$

where $\mathbb{1}_{(e \geq \underline{e})}$ is an indicator for effort that exceeds the prescribed level. Costs of integration effort vary across immigrants, depending on factors such as facility in learning foreign languages, or navigating foreign cultures. In what follows, I assume that the cost of effort of an immigrant i increases linearly in the amount of effort provided at a rate c_i, so that $c_i(e) = c_i e$.

The following proposition establishes that the presence of prescriptions induces heterogeneous effort provision within the immigrant group. Specifically, prescriptions lead some immigrants – those who find the provision of minimum required effort too costly – to "give up" altogether on attempting to integrate.

Proposition 1. *There exists a cutoff $\hat{c}$ given by $\frac{P(\underline{e})f+h}{\underline{e}}$, such that the effort provided by immigrants with $c_i < \hat{c}$ is positive and the effort provided by immigrants with $c_i > \hat{c}$ is zero.*

The key intuition behind this result is that high effort is only profitable to immigrants as long as they meet the prescription target. If they have no hope of doing so, and given that non-compliance implies diminished chances of integration anyway, no effort is better (less costly) than any amount of effort lower than what the prescription requires.

It is straightforward to show that, for a given level of prescriptions, optimal effort provision and the rate of successful integration are non-decreasing in incentives. The result is given by the following proposition:

Proposition 2. *Consider an increase in incentives from h to h^n and denote the resulting new cutoff by $\hat{c}^n$. Then $\hat{c}^n > \hat{c}$, and both optimal effort and the probability of successful integration increase if $\hat{c} < c_i < \hat{c}^n$ and are constant otherwise.*

The effect of incentives is illustrated in the left panel of Figure 1. Higher incentives increase the cost cutoff that determines whether the provision of minimum effort $\underline{e}$

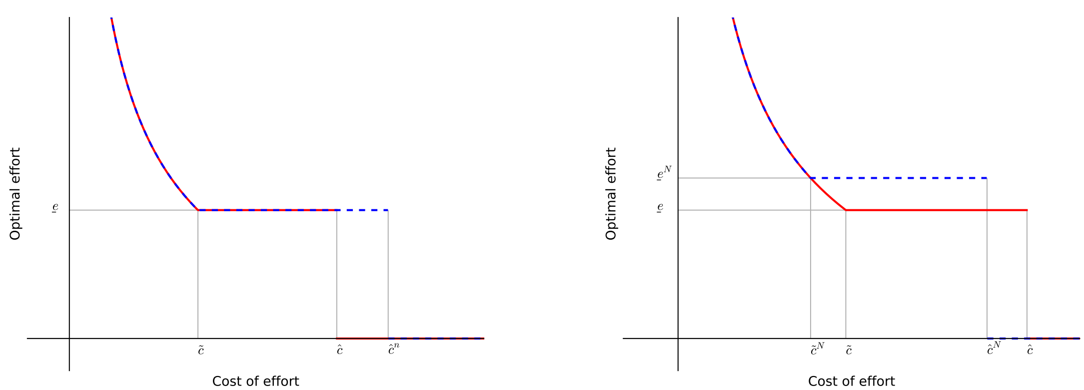
is worthwhile, leading some immigrants to increase their efforts to meet the target effort set by the prescription. Those who found effort provision worthwhile with lower incentives (those to the left of $\hat{c}$) are not affected. The same is true for those with high costs of effort ($c > \hat{c}^n$), for whom the increase in incentives is not large enough to make positive effort provision optimal. Depending on the distribution of costs of integration effort, incentives can thus have a positive or zero effect on integration effort and success on average.

Unlike with incentives, an increase in prescriptions can lead to a *decrease* in optimal effort and successful integration for some immigrants. The effects of a change in prescriptions are summarized in the following proposition.

Proposition 3. *Consider an increase in prescriptions from* $\underline{e}$ *to* $\underline{e}^N$ *and denote the resulting new cutoff by* $\hat{c}^N$*. Denote by* $\tilde{c}^N$ *the cost of immigrant with* $e^* = \underline{e}^N$*. Then* $\hat{c}^N < \hat{c}$*, and optimal effort is decreasing if* $\hat{c}^N < c_i < \hat{c}$*, increasing if* $\tilde{c}^N < c_i < \hat{c}^N$ *and constant otherwise.*

Figure 1. Optimal effort in response to changes in incentives and prescriptions

(a) An increase in incentives (b) An increase in prescriptions



Notes: The red line depicts optimal integration effort under incentives h and prescriptions $\underline{e}$. The dashed blue line depicts optimal integration effort for increased incentives h^n (left) or increased prescriptions $\underline{e}^N$ (right).

On the one hand, higher prescriptions promote higher effort provision, as immigrants try harder to comply with target effort levels. On the other hand, non-compliance with prescriptions substantively reduces the chances of broader integration. Compliance itself crucially depends on immigrants' ability and willingness to reach prescribed behavioral targets. Those with sufficiently low costs of effort will try harder to meet the prescriptions. Those with high costs will instead lower their efforts to levels below those they exerted in the absence of the policy.

The right panel of Figure 1 illustrates the effect of an increase in prescriptions from $\underline{e}$ to $\underline{e}^N$. There are now four groups of immigrants. The first group (those with costs $\tilde{c}^N < c_i < \hat{c}^N$) has costs sufficiently low, so as to be able to provide additional effort and meet the higher prescription target. A second group (those with costs $\hat{c}^N < c_i < \hat{c}$) find the new prescription target too costly to meet. Since non-compliance now implies a lower (zero) probability of integration, this group finds it more profitable to abandon effort altogether. There is no effect on immigrants who were either providing no effort under the old prescription target (those with $c_i > \hat{c}$) or had low enough costs to be in the unconstrained optimization region under both old and new prescriptions (those with $c_i < \tilde{c}^N$).

The implications of a change in prescriptions for successful integration are straightforward and are given by the following proposition.

Proposition 4. *Consider an increase in prescriptions from $\underline{e}$ to $\underline{e}^N$ and denote the resulting new cutoff by $\hat{c}^N$. Denote by $\tilde{c}^N$ the cost of immigrant with $e^* = \underline{e}^N$. Then $\hat{c}^N < \hat{c}$, and the probability of successful integration is decreasing if $\hat{c}^N < c_i < \hat{c}$, increasing if $\tilde{c}^N < c_i < \hat{c}^N$ and constant otherwise.*

Taken together, Propositions 3 and 4 imply that increases in prescriptions can either increase or decrease integration efforts and successful integration. As with incentives, their effects *on average* are determined by the distribution of costs of effort in the immigrant population.[4]

[4]This result implies that the same change in prescriptions may have different effects in different contexts, that vary in terms of the characteristics of the immigrant population and of existing integra-

The average effects of prescriptions also depend on the extent to which non-compliance reduces the chances of integration in domains beyond the one governed by the policy. Exclusion from education or the labor market (because immigrants failed to comply with target behaviors such as learning the language) substantively reduces the probability of broader integration. When non-compliance implies restriction of more limited privileges, chances of broader integration suffer less. For example, restricting certain facets of political participation, such as voting in national elections, hinder an immigrant's political integration, but may have limited impact on their economic or social integration at large. Prescriptions are thus more likely to negatively impact integration the greater are their spillovers on other domains of the immigrants' life.

In sum, what determines the average effects of policy in this simplified setup is the mix of incentives and prescriptions relative to immigrants' costs of effort. Higher incentives increase integration efforts and successful integration, unless they are set at levels too low relative to most immigrants' costs, in which case they are ineffective. Higher prescriptions instead can lower integration rates, particularly when non-compliance implies negative spillovers on domains of integration beyond the one directly governed by the policy. Conditional on the distribution of immigrant characteristics, policies relatively more reliant on incentives are therefore more likely to succeed in increasing immigrant effort and integration rates, than policies reliant on prescriptions.

Historical background

Ford's Five-Dollar Day

Henry Ford introduced his Five-Dollar Day Plan in 1914 in the Highland Park Plant which produced the Model T. At that time, the Ford Motor Company employed approx-

tion requirements (the level of the original cutoff $\hat{c}$). Thus, an identical policy that proves restrictive in one context, may promote integration in another (Goodman 2012).

imately 75% foreign-born workers, over half of whom were from Southern and Eastern Europe. The assembly line system, introduced by Ford in 1908, had substantially reduced the complexity of tasks to be performed and consequently the level of skill required of workers. At the same time, it had increased worker dissatisfaction and rates of absenteeism (Raff and Summers 1987). Most accounts point to Ford introducing his new plan as an attempt to deal with these phenomena and pay efficiency wages (Conot 1974).

The plan guaranteed a shorter work day and divided pay into wages and profits. Profit-sharing with workers was subject to strict conditionality. To qualify for the Five-Dollar Day plan, workers had to be vetted by Ford's Sociological Department, which visited them in their homes and ensured that their lifestyle met Ford standards. This meant adopting middle-class American habits and values. Workers were not only required to abstain from drinking and gambling, but to consume wholesome foods, display cleanliness and tidiness and regularly deposit money into a savings account. In describing the work of his Sociological Department, Ford argued that "these men of many nations must be taught American ways, the English language and the right way to live" (Barrett 1992).

Instruction in the English language was undertaken by Ford's English School. The principle of strict conditionality was also applied to school attendance. In the words of S.S. Marquis, the head of the Sociological Department, "If a man declines to go, the advantages of the training are carefully explained to him. If he still hesitates, he is laid off and given uninterrupted meditation and reconsideration. When it comes to promotion, naturally preference is given to men who have cooperated with us in our work." From 1915 to 1920 the company reported 16,000 workers graduating from the Ford English school. Company statistics indicated that the share of workers who did not speak English dropped from 35.5% in 1914 to 11.7% in 1917 (Meyer 1980).

English-only laws

Teaching English to the foreign-born was one of the primary goals of the Americanization movement. Night schools and evening adult classes sprung up across the country to educate immigrants in the use of the English language, especially in the states with the largest immigrant population, such as New York (Ziegler-McPherson 2009). During the Age of Mass Migration, from 1850 to 1915, many states also enacted compulsory schooling laws. Such laws were often motivated by the desire to assimilate immigrants, especially those coming from countries without compulsory schooling (Bandiera et al. 2019).

At the same time, state legislation regarding language was for the most part permissive. With the exception of California, which had introduced an English law in schools since 1874, most states had no provision on the language of instruction and some explicitly allowed for the use of foreign languages. 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 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). Such permissiveness did not imply lack of concern on the language issue at school. In fact, nativist pressures had advocated for English-only laws since the late 19th century. The 1889 Wisconsin Bennett Law required English instruction in public and private schools, but was repealed two years after its enactment following pressure from the state's German and Polish communities.

The first English-only laws were passed in 1909 in New York and Rhode Island, followed by a 1913 law in Arizona. Yet a true wave of change in legislation only occurred after WWI, spurred by the patriotism brought about by the war and the concern that foreign-language education was a hotbed for disloyalty. Between 1919 and 1923 multiple states enacted English-only laws or banned foreign languages, even as a separate instruction course. Anti-German sentiment specifically motivated many of the laws enacted in the years after WWI. Ohio and Indiana explicitly prohibited the use of German in all schools during this period (Luebke 1974).

In 1923 Meyer vs Nebraska led to the repeal of a 1919 Nebraska law that banned the use of foreign languages either as a medium of instruction or as a separate subject in private schools and ultimately led to the repeal of all related legislation. Yet English-only laws in public schools remained in place in most states and the wartime sentiment that motivated the enactment of language bans led to the demise of foreign-language schooling in the post war era (Schlossman 1983; Wüstenbecker 2007).

Incentives and prescriptions for Americanization

I argue that, while different in several respects, Ford's Five-Dollar Day plan and English-only school policies are good examples of, respectively, incentive-based and prescription-based Americanization initiatives.

While the introduction of Ford's plan changed both prescriptions and incentives for affected workers, emphasis on the incentive component was stronger. Incentives were offered in the form of direct, monetary compensation. Ford's wages were, at least initially, markedly higher than those prevalent in the rest of the industry (Raff and Summers 1987). They were paid out immediately and were directly conditioned on workers' effort, which consisted in attendance of English classes and behavior compatible with the standards set by the Sociological Department. These conditions constitute the prescriptive component of Ford's policy. Non-compliance with Ford's standards excluded the worker from profit-sharing, and potentially even from work in the company. However, as most workers would have been able to move on to other jobs, with lower wages but also less strict behavioral standards, the impact on their broader economic and social integration would not have been dramatic. Thus, any negative spillover effect of the plan's prescriptive component that could counterbalance the effect of incentives was muted.

The enactment of English-only policies introduced new prescriptions, but did not change incentives. Immigrant children were required to attend schools in which English was the only medium of instruction. The reward for compliance was access to education and its returns in adulthood. English-only laws thus changed requirements for immigrant pupils, without offering any additional incentives for compliance (compared to those already present in the absence of the policy). Furthermore, existing incentives were hard to quantify, uncertain, and materialized later in life. To the extent that decisions about school attendance were made by parents, incentives were also less direct than in the case of Ford, as they accrued to students and were likely not fully internalized by the relevant decision-makers. All of these features imply a relatively less central role for incentives – and a more prominent one for prescriptions – in English-only policies. Finally, non-compliance with the laws implied exclusion from education, with potentially substantive negative spillovers on later labor market and social integration. This implies an even higher likelihood of negative effects of English-only laws on the integration of immigrants with high costs of effort.

Empirical analysis

Outcomes

I focus on four outcomes that constitute the most reasonable individual-level proxies of social and civic integration effort and successful integration available in the historical census.[5] These are English proficiency, the filing of Declarations of Intention to naturalize (known as "first papers"), rates of citizenship acquisition, and rates of intermarriage with the native-born.

[5]Integration is a multidimensional concept that is hard to measure, even with modern-day data (Harder et al. 2018), and integration in one domain of life does not necessarily imply integration in others (Aleksynska and Algan 2010). Yet the outcomes examined here all arguably capture important dimensions of participation in the host society. The individual-level identification strategy does not allow me to examine other, potentially equally important facets of integration, like ethnic segregation (Koopmans 2010) or church and club membership, all of which are available only at an aggregate level.

English proficiency is measured as a binary indicator for immigrants who could speak English, as decided by the census enumerator. First papers could be taken out by immigrants upon arrival to the country, and they constituted the first step in the naturalization process. Eligible immigrants, i.e. those who had lived in the country for at least five years and had access to two witnesses, could file a Petition for Naturalization ("second papers") which, if approved by the court, granted them US citizenship. Knowledge of English and first papers thus constitute proxies of integration effort.

Naturalization instead required court approval and is thus, at least in theory, a measure of successful integration. In practice, rejection rates were likely low,[6] though courts could and did exercise discretion in processing petitions (Fouka 2019). Neither the intention to naturalize, nor citizenship acquisition are necessarily synonymous with integration. Yet they are both good proxies, since citizenship can be both the end product of integration, as well as its catalyst (Bauböck et al. 2013; Hainmueller, Hangartner, and Pietrantuono 2017).

Finally, I examine intermarriage as a measure of successful social integration. This outcome is dependent on both willingness to integrate and acceptance on the part of the host society. According to Gordon 1964, intermarriage is "the final stage of assimilation" for immigrants in the US. As with citizenship, marriage to a native-born person can be both the outcome of a desire to integrate (Bisin, Topa, and Verdier 2004), as well as a pathway to integration (Meng and Gregory 2005). Rates of intermarriage among foreign-born industrial workers in cities with a Ford assembly plant in 1910 were low (lower than 10%) and thus, particularly for this sample, any changes in this measure set a rather high bar for successful incorporation into American society. I measure intermarriage as an indicator for being married to a native-born spouse of

[6] In a sample of approximately 3,300 naturalization petitions filed in New York City in 1930, Biavaschi, Giulietti and Siddique (2017) find that only 2.6% were rejected.

native-born parents.[7]

The effects of Ford's Five-Dollar Day

To investigate the impact of Ford's Five-Dollar Day plan on the outcomes of foreign-born workers I use data from the full count of the 1910 and 1920 US censuses (Minnesota Population Center and Ancestry.com 2013). I compare outcomes of Detroit and Highland Park residents before and after the introduction of the plan in 1914. The opening of the plant may have changed the composition of the foreign-born living in the area, by attracting more or less Americanized foreigners. To account for this potential compositional change, I estimate changes in the outcomes of the same workers over time in a within-person specification.

Following Abramitzky, Boustan, and Eriksson (2014), I link all foreign-born men aged 15-65 and in the labor force in 1910 to the 1920 census, using information on their first and last name, country of birth and year of birth.[8] Details on the linking procedure, as well as on linking diagnostics and summary statistics on the resulting linked dataset can be found in the Online Appendix. Summary statistics for all outcome variables are provided in Table B.2. I restrict attention to cities in which Ford had an assembly plant between 1910 and 1920.[9]

For three of the four outcome variables (English knowledge, first papers, citizenship), I exploit the panel nature of the linked dataset and estimate the following specification:

$$Y_{icst} = \alpha_1 + \lambda_i + \mu_t + \theta_s \times \mu_t + \delta_c \times \mu_t + \beta_1 T_{icst} + u_{icst}$$

[7] A native-born spouse of native-born parents could still be a third-generation immigrant. Census data does not allow me to observe the birthplace of grandparents. This does not invalidate the measure, as third-generation immigrants are expected to be more (or at least not less) integrated than second- or first-generation immigrants, thus having a positive contribution to the integration of their spouse.

[8] As is common practice in the census-linking literature, I focus on men. Women changed their maiden names after marriage, making it harder to track them in consecutive census schedules.

[9] Table B.1 reports the full list of cities.

where subscript i denotes individuals, subscript c denotes cities, subscript s denotes industries and subscript t denotes census decades. λ_i are individual fixed effects. They allow us to keep constant fixed unobservable characteristics of individuals, as well as time-variant unobservable confounders that affect everyone in the same way.[10] θ_s and δ_c are, respectively, industry and city fixed effects. Including their interactions with census decade fixed effects μ_t ($\theta_s \times \mu_t$ and $\delta_c \times \mu_t$) allows us to control for time variant unobservable confounders that may differentially affect the outcomes of individuals in different industries and cities. T_{icst} is an indicator for workers in the auto industry who lived in Detroit or Highland Park in 1910. The coefficient of interest is β_1. It captures how the change in outcomes between 1910 and 1920 differs for auto workers in Detroit and Highland Park compared to workers of other industries living in other cities with a Ford assembly plant. Since treatment varies at the city-industry level, I follow Abadie et al. (2017) and conservatively compute two way clustered standard errors at the city and industry level.

Crucially, the effect identified by β_1 is not biased by selection of workers into Ford's plan on the basis of their prior integration or desire to Americanize. The treatment is defined based on workers' characteristics (location and industry) in 1910, prior to the introduction of the Five-Dollar Day plan. Workers who left Ford after 1910 are classified as treated. If these individuals changed jobs in order to avoid Americanization, the estimated effect of exposure to Ford's plan will be an underestimate of the true effect.[11] There is also no evidence of differential selection of more integrated Detroit auto workers into the linked sample. While more integrated individuals are on average more likely to be matched across census years (Table C.1), this difference is the same for Detroit auto workers and workers in comparison cities and industries (Table C.3).

[10] Individual fixed effects also account for any time-invariant confounders at the industry-city level. For instance, this estimation strategy accounts for any differences in integration outcomes (that do not vary over time) between auto workers in Detroit and e.g. iron workers in Pittsburgh.

[11] A similar downward bias will be present if other industrial employers undertook Americanization efforts between 1910 and 1920.

Table 1 displays the results. For every outcome I report two coefficients, one estimated among all men in the labor force, and one in the restricted set of workers in the manufacturing sector, that is potentially a better comparison group for auto workers. Detroit auto workers experienced an improvement along all measured outcomes between 1910 and 1920. The likelihood of speaking English for this group differentially increased by 1.5 to 1.9 percentage points. The average increase in English knowledge among foreign-born workers in cities with a Ford assembly plant between 1910 and 1920 was 8.3 percentage points. The effect of Ford's plan estimated in column 1 amounts to almost 25% of that increase. Even larger effects are estimated for first papers (equivalent to over 100% of the average increase between 1910 and 1920) and naturalization rates (equivalent to 50% of the average increase between 1910 and 1920).

Table 1. Change in outcomes of Detroit auto workers

Dependent variable	Speaks English		First papers		Naturalized	
	(1)	(2)	(3)	(4)	(5)	(6)
Detroit × Auto industry × 1920	0.0191 (0.00293)	0.0156 (0.0118)	0.0382 (0.00355)	0.0302 (0.00720)	0.0478 (0.00405)	0.0660 (0.00807)
Mean dep. variable in 1910	0.850	0.790	0.116	0.119	0.580	0.534
Observations	219428	71962	210632	67600	210632	67600
R-squared	0.614	0.624	0.545	0.540	0.707	0.715
In Manufacturing		✓		✓		✓

Notes: Data restricted to foreign-born men aged 15-65 and in the labor force in 1910. All regressions control for individual fixed effects, city (in 1910) × year and industry (in 1910) × year interactions. Two-way clustered standard errors at the city and industry (in 1910) level reported in parentheses.

Estimates change only modestly when focusing on the manufacturing sector. Table B.3 in the Appendix replicates the results of Table 1 by restricting the control group to men in Midwestern cities with a Ford factory, who are arguably more comparable to workers in Detroit and Highland Park. Estimated effects are largely comparable in magnitude for all outcomes.

The analysis of intermarriage rates requires a different approach. To begin with, marriage outcomes are slower moving than language proficiency and naturalization. To allow enough time for any effects of Ford's plan to manifest in marriage decisions, I link individuals between 1910 and 1930, thus observing them 16, rather than 6 years after the plan's introduction. I restrict attention to foreign-born workers who were not married in 1910. Since I cannot estimate a specification with individual fixed effects as before, I focus on outcomes of these workers in 1930, and estimate the following equation

$$Y_{icsn} = \alpha_2 + \delta_c + \theta_s + \kappa_n + \beta_2 T_{icsn} + \gamma \mathbf{X}_{cs} + \varepsilon_{icsn}$$

Here, i indexes individuals, c indexes cities, s indexes industries and n indexes countries of birth. δ_c, θ_s and κ_n are city, industry and country of birth fixed effects, respectively. In more parsimonious specifications I also include additional fixed effects (indicators for city by country of birth, industry by country of birth, and age in 1910). The vector $\mathbf{X}_{cs}$ represents city-industry-level controls that capture characteristics of the marriage market faced by foreign-born industrial workers. I control, specifically, for the share of first and second generation immigrants in the city and industry in 1910; this share captures the availability of immigrant spouses and should thus be negatively associated with the likelihood of intermarriage with natives. T_{icsn} is an indicator for auto workers in Detroit and Highland Park. The coefficient of interest is β_2, capturing the difference in intermarriage rates for Detroit and Highland Park auto workers compared to workers in other cities and industries.

Table 2. Effects of Ford's program on rates of intermarriage

Dependent variable	Married to native			
	(1)	(2)	(3)	(4)
Detroit × Auto industry	0.00698	-0.000677	0.0704	0.0953
	(0.00763)	(0.00978)	(0.0184)	(0.00218)
Mean dep. variable in 1910	0.126	0.126	0.131	0.123
Observations	24534	23045	10938	4216
R-squared	0.109	0.190	0.240	0.243
Controls		✓	✓	✓
Aged 15-25 in 1910			✓	✓
In manufacturing				✓

Notes: Census year 1930. Data restricted to foreign-born men aged 15-65 (in columns 1 and 2) or 15-25 (in columns 3 and 4), in the labor force and not married in 1910. All regressions control for city (in 1910), industry (in 1910) and country of birth fixed effects. Additional controls in columns 2–4 include city (in 1910) by country of birth, industry (in 1910) by country of birth, and age in 1910 fixed effects as well as the share of first and second generation immigrants in the city and industry in 1910. Two-way clustered standard errors at the city and industry (in 1910) level reported in parentheses.

Table 2 presents the results. There is no effect on rates of intermarriage with the native-born for the sample at large (columns 1 and 2). Since the dataset comprises individuals of ages up to 65 years in 1910, who would likely not get married within the next twenty years, these estimates are likely to be downward biased. Indeed, when restricting attention to the subset of men most likely to enter the pool of married individuals between 1910 and 1930, i.e. those aged 15-25 in 1910, Detroit auto workers experience a higher increase in the likelihood of being married to a native-born spouse of native-born parents (column 3). This effect is larger when restricting comparisons to workers in the manufacturing sector (column 4). The estimated magnitude in column 3 amounts to over 90% of the baseline mean of intermarriage rates among foreign-born industrial workers in the same age group (aged 35-45 in 1910). Figure B.1 in the Appendix plots coefficient estimates from regressions like the one in column 4, restricting attention to different age groups in 1910. As expected, the effect of Ford's plan on the likelihood of being married to a native increases as the data is restricted to younger ages, who are more likely to marry between 1910 and 1930.[12]

[12]Effects on intermarriage rates of men aged 15-25 in 1910 are even larger when the comparison group is restricted to Midwestern cities (columns 1 and 2 of Table B.4). Coefficients attenuate when

All in all, the data indicates that Detroit and Highland Park auto workers experienced increases in English proficiency, intention to naturalize and naturalization outcomes, as well as rates of marriage to native spouses. By providing strong incentives to exert integration effort, the Five-Dollar Day Ford plan was effective in increasing both effort and successful integration, as measured by all proxies available in the census.

One alternative explanation is that the Five-Dollar Day plan changed integration outcomes not because it incentivized immigrant effort, but because the wage increase it implied improved workers' economic outcomes. Upwards economic mobility could have then indirectly favored English proficiency, naturalization and social integration.[13] Table B.5 in the Appendix examines the effect of exposure to the plan on a number of economic outcomes. Income and wage data is not collected by the US census until 1940, so I use instead the logarithm of the occupational earnings score, an indicator for homeownership and an indicator for reliance on mortgage loans among homeowners. The values of the occupational earnings score represent the median total income (in hundreds of 1950 dollars) of all persons with that particular occupation in 1950.

Ford's plan did indeed improve workers' economic outcomes. The effect on log occupational earnings scores, though statistically significant, is small (less than 1% of the 1910 mean). The effect on rates of homeownership is more substantial, amounting to 6% of the 1910 mean and thus smaller than the effect on either first papers or naturalization rates, but larger than that on English proficiency.[14]

Yet there is little reason to believe that homeownership alone can explain a signif-

examining marriage outcomes in 1920 (columns 3 and 4 of Table B.4), consistent with effects of Ford's plan on intermarriage extending over a longer time horizon.

[13]To be sure, upwards economic mobility would also constitute successful (economic) integration. But the mechanism leading to better integration outcomes would be mechanical and not work through changed incentives of immigrants to exert effort, as proposed by the conceptual framework. It is of course plausible that both channels were simultaneously at work.

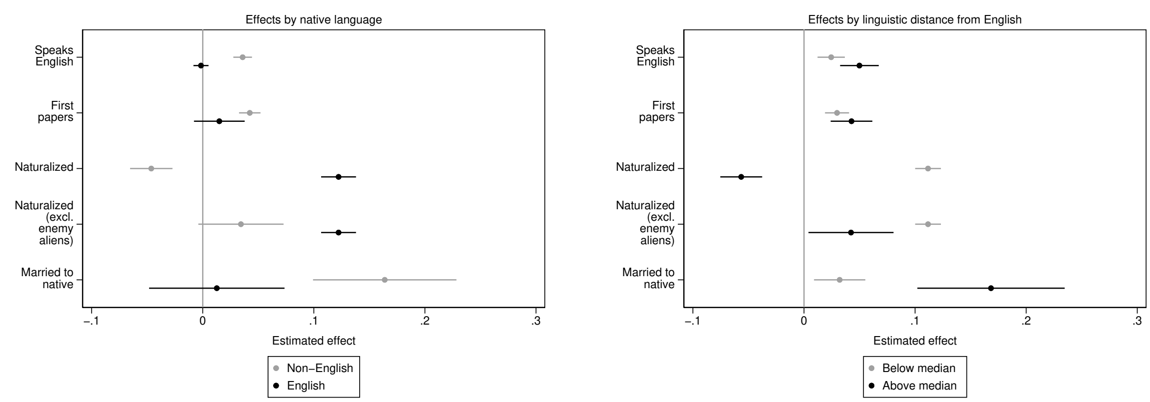
[14]One reason why exposure to the Five-Dollar Day plan was not associated with even higher improvements in economic outcomes may have been rising wages offered by other automobile manufacturers in Detroit. Raff (1988) suggests this was not in direct response to Ford's efficiency wages, but rather the result of orders connected with World War I.

icant share of the improvements in the social integration outcomes that we observe. Two pieces of evidence support this conclusion. First, a simple correlation analysis of changes over time suggests that the increase in rates of homeownership is too small to account for the estimated effects on integration variables. Workers outside the auto industry and outside of Detroit and Highland Park experience a 16% increase of homeownership from 1910 to 1920, which was associated with a 2% increase in filing of first papers and a 9% increase in rates of naturalization. Assuming a similar correlation among auto-workers in Detroit and Highland Park, this would indicate that the estimated increase in homeownership as a result of Ford's plan can account for only one quarter of the estimated increase in naturalization rates and less than one tenth of the estimated increase in filing of first papers.

Second, I use sequential g-estimation (Acharya, Blackwell, and Sen 2016) to estimate the average controlled direct effect of Ford's plan on integration outcomes, treating homeownership as a mediator. This strategy yields effects identical to the baseline ones up to the third decimal (Table B.6). This is not surprising, given the modest (and, in the case of first papers, negative) correlation of homeownership with measures of integration and integration effort. Taken together, these analyses speak against the idea that Ford's plan mainly affected integration outcomes mechanically through economic channels.

It was shown theoretically that incentives can have heterogeneous effects, with little or no impact on the efforts of immigrants with low costs of integration effort. Figure 2 provides evidence for such heterogeneity in the case of Ford's plan. I proxy for immigrants' costs of effort using their native language and its distance from English. The left subfigure of Figure 2 plots the effect of Ford's programs on all outcomes, separately for immigrants with English as their native language (Canadians, Irish, English) and all others. The right subfigure plots heterogeneous effects by a continuous measure of a language's distance from English, developed by Chiswick and Miller 2005. The measure assigns higher scores to languages that native English speakers take a longer time to master. I recode the measure, so that higher values represent a higher linguistic distance from English, and re-estimate effects for all outcome variables, separately for immigrant nationalities with above and below median linguistic distance from English. Consistent with the theory, the effects of Ford's plan are never negative. For all outcomes but one they are larger for immigrants with languages distant to English.[15] This analysis confirms the theoretical insight that the strength of incentives is decreasing in immigrants' costs of effort (provided that those costs are not prohibitively high). Underlying regressions are shown in Table B.7.

Figure 2. Heterogeneous effects of Ford's plan



Notes: The figure plots coefficient estimates and 95% confidence intervals from regressions like the ones specified in columns 1, 3 and 5 of Table 1 and column 4 of Table 2.

The effects of English-only laws

To analyze the effects of language policies enacted in education during the Americanization period I focus on the US-born children of immigrants and link their adult outcomes to their potential exposure to English-only laws in school based on their state of birth. For comparability with the analysis of Ford's Five-Dollar Day plan, I restrict attention

[15]Naturalization rates are the exception to this pattern, but even in that case, the effect of Ford's program is not statistically different from zero. Furthermore, estimates are sensitive to the exclusion of Germans and nationals of the Austro-Hungarian empire who faced barriers to naturalization during the period under study. The Alien and Sedition Acts invoked after the US's entry into WWI prohibited "enemy aliens" – nationals of countries at war with the US – from acquiring the US citizenship.

to men. Unlike with the foreign-born, I cannot use first papers and naturalization rates as outcomes. I thus restrict attention to English proficiency and marriage to a native spouse.

I use data on individuals with a foreign-born father, from the 5% 1930 and 1% 1960 samples of the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2010). Since almost all English-only laws were enacted between the late 19th century and the early 1920s, these census decades allow me to observe English proficiency and marriage outcomes for cohorts affected by such laws at a time when they were no longer at school. As before, I measure intermarriage rates as marriage to a native-born spouse of native-born parents. This outcome is available in 1930 and 1960, but not in 1940 and 1950. In these two census decades information on parental birthplace was recorded only for a random sample of the universe, which makes the question unavailable in the 1% sample. Information on English knowledge instead is only available in 1930.

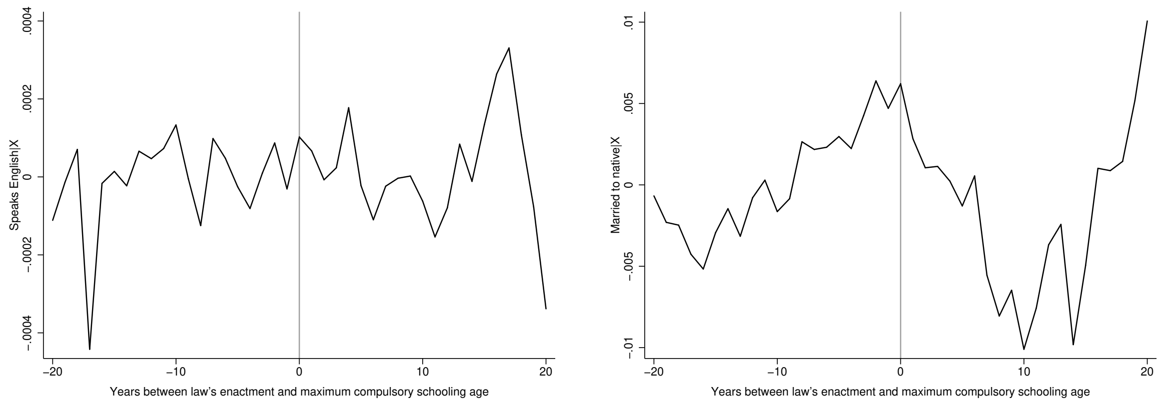
Data on English-only laws is from Edwards (1923) and Knowlton Flanders (1925). Table B.8 in the Appendix lists all state laws, their scope of application and year of enactment. I create an indicator for exposure to an English-only law for individuals who should have been at school at the time the law was enacted according to compulsory school age requirements. Information on the state-specific age range for compulsory schooling comes from Goldin and Katz (2008).

Figure 3 offers visual evidence on the effect of English-only laws. The figure plots average outcomes by cohort. Cohorts are grouped according to the difference between the year when an individual reached the end of compulsory schooling age and the year of the law's enactment in the individual's state of residence. Cohort 0 then represents all individuals who could leave school in the year when the law was introduced, while cohort 1 includes all individuals who were exposed to the law for at most one year. Outcomes are residualized by controlling for state of birth, father and mother's birthplace (country in the case of the foreign-born and US state in the case of the US-born), as well as age and census decade fixed effects. For visualization purposes, I restrict attention to the

20 birth cohorts before or after the date of the law's enactment.

The graph is revealing. There is no indication of a trend break for English proficiency. Instead, there is an impressive trend reversal in the rates of intermarriage for treated cohorts. Intermarriage rates steadily increase among cohorts too old to have been at school during the enactment of English laws. This increase stops right around the first cohort treated by an English law and turns into a steep decline thereafter.

Figure 3. Effect of language bans on English proficiency and intermarriage rates



Notes: The figure plots residuals of the variable in the y-axis from a regression on indicators for state of birth, father and mother's birthplace, age and census decade. The sample consists of US-born men with a foreign-born father. Data is from the 1930 5% IPUMS sample (left subfigure) and the pooled 1930 5% and 1960 1% samples (right subfigure).

Since treatment assignment is based on cohort and state of birth, comparisons in Figure 3 correspond to an intention-to-treat effect. Treatment intensity varies in the degree to which different immigrant groups had access to non-English-speaking education prior to the enactment of English-only laws. Northern and Western Europeans had private and parochial school networks that used foreign languages either as a medium of instruction in certain subjects or as a course (Olneck 2009). It is thus more likely that English-only laws had a measurable effect on that group of immigrants than on Southern Europeans, for whom English was the only language available at school in the first place.

Figure B.2 in the Appendix displays patterns consistent with these facts, by disaggregating the effects of laws by immigrant origin (based on the nationality of the father). Effects are uniformly null for English proficiency. For intermarriage, a trend break for cohorts affected by English-only laws is apparent primarily for immigrants from Northwestern Europe, Scandinavia and Germany. Italians and other Southern Europeans display a dip either directly upon enactment or immediately afterwards, but this is not large or readily distinguishable from general volatility in the intermarriage time series. Interestingly, the most pronounced effect is observed among Germans. Not only did that group have one of the largest networks of parochial schools in the US (Schlossman 1983), but they were also singled out by English-only legislation that explicitly banned the use of their language (Luebke 1974). They were thus more likely to be affected by school laws.

Figure 3 corresponds to an event study analysis, that compares treated and non-treated cohorts in states that ever enacted an English-only law. I next generalize this comparison to a difference-in-differences framework, using states that never enacted English-only laws as a control group. I estimate

$$Y_{icst} = \alpha_3 + \beta_3 T_{cst} + \pi_c + \zeta_s + \eta_t + \epsilon_{icst}$$

where subscript i denotes individuals, subscript c denotes cohorts, subscript s denotes states of birth, and subscript t denotes census decades. π_c, ζ_s and η_t are birth cohort, state of birth and census decade fixed effects, respectively. T_{cst} is an indicator for treated cohorts (based on each state's compulsory schooling age range at the time of the English-only law's enactment) in states with an English-only law. The coefficient of interest is β_3, capturing the differential change on outcomes for school-age cohorts in treated states.

Table 3. English-only laws and long-run outcomes of the second generation

Dep. variable	Speaks English		Married to native	
	(1)	(2)	(3)	(4)
English-law × Treated cohort	0.00300	0.000195	-0.0468	-0.0210
	(0.00316)	(0.000386)	(0.0176)	(0.00942)
Mean dep. variable	0.995	0.995	0.396	0.396
Observations	5796993	5796983	3025951	3025943
R-squared	0.0666	0.147	0.0387	0.121

Notes: The sample consists of US-born men with a foreign-born father. Data is from the 1930 5% IPUMS sample (columns 1 and 2) and from the pooled 1930 5% and 1960 1% samples (columns 3 and 4). *Married to native* is an indicator for individuals with a native-born spouse of native-born parents. All columns include indicators for year and state of birth. Columns 2 and 4 additionally include indicators for age and father and mother's birthplace. Standard errors are clustered at the state of birth level.

Table 3 presents the results. Consistent with the visual evidence in the graphs, there is no effect on English knowledge. This finding confirms the results of Lleras-Muney and Shertzer (2015) who find no effect of English-only laws on contemporaneous English proficiency of foreign-born children.[16] There is, instead, a negative and significant effect on the likelihood of native intermarriage for exposed cohorts, that ranges between 2.1 and 4.7 percentage points.

Table B.9 in the Appendix replicates the analysis in Table 3, by restricting the comparison to adjacent states. To increase comparability across states that did and did not enact language legislation, I assign to each treated state a different control group, composed by all its neighboring states without a law. Each of the control neighbors is then assigned an artificial treatment year, based on the year in which its first treated neighbor enacted language legislation. Results are very similar in direction and magnitude as in Table 3.

The difference in differences estimation relies on the assumption that cohorts in states that did and did not enact language legislation were on parallel trends prior to the introduction of the English-only laws. To corroborate the validity of this assumption,

[16]A possible explanation for this is that there is little margin for improvement of this outcome, because knowledge of English is near universal among US-born adults in the sample. Still, as will be shown, there is heterogeneity in effects depending on language background of the family.

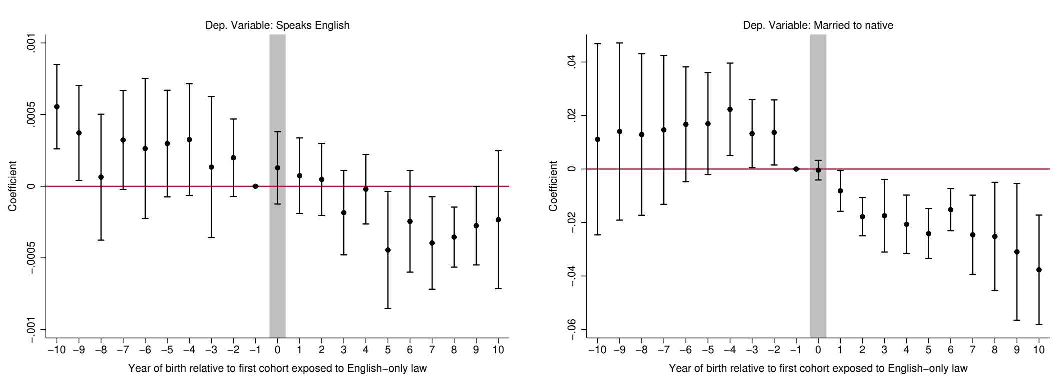
I plot estimates of β_τ's from the following specification including leads and lags:

$$Y_{icst} = \sum_{\tau} \beta_\tau T^{\tau}_{cst} + \gamma_c + \zeta_s + \eta_t + \epsilon_{icst} \tag{1}$$

where T^{τ}_{cst} is a dummy equal to 1 if cohort c reached the age of compulsory schooling age τ years after the law was enacted. The β_τ's then measure the difference in outcomes between treated and controls states for cohorts leaving school by compulsory law before ($\tau < 0$) or after ($\tau > 0$) the introduction of the law. I normalize coefficients relative to the last cohort to finish compulsory schooling prior to the law's introduction ($\tau = -1$).

Figure 4 presents evidence supportive of the identification strategy. No significant difference in outcomes is observed between treated and control states for cohorts that left school prior to the introduction of the law. Consistent with estimated effects in Table 3, no significant effect of laws is found on English proficiency for treated cohorts, though there is a general tendency for individual cohort coefficients to become smaller, and even significantly negative, with increased exposure to a law. For intermarriage instead, differences between treated and control states appear for the first cohorts to be affected by language bans (based on compulsory schooling age in their states) and grow larger for subsequent cohorts, proportionally to a cohort's exposure to legislation.

Figure 4. Event-study graph of the effects of language bans

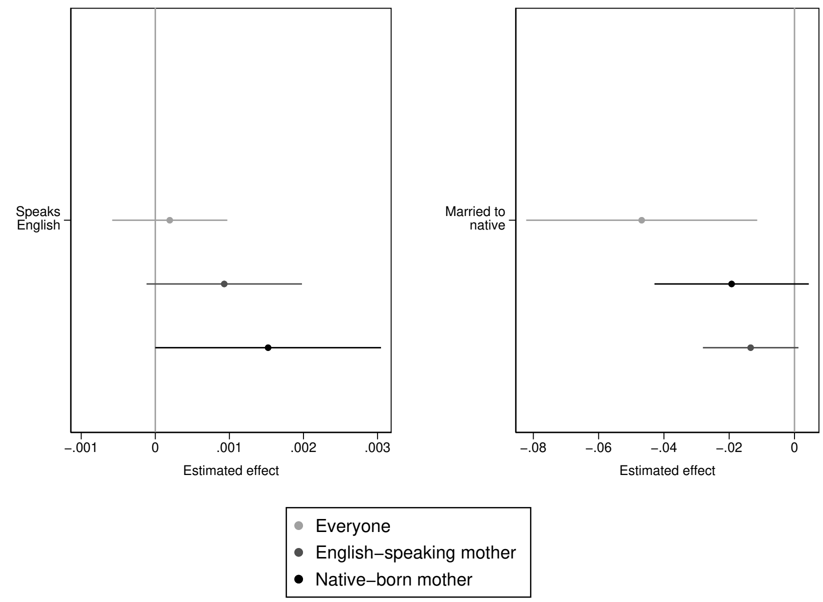


Notes: The figure plots coefficient estimates and 95% confidence intervals for β_τ from the regression in 1 for english proficiency (left) and intermarriage (right).

The results are consistent with the hypothesized effect of prescriptions outlined earlier. On the one hand, English-only laws incentivize effort to learn English for a subset of immigrant children. At the same time, costs of compliance may be substantial, especially for immigrant children with little baseline knowledge of English. This echoes one of the central arguments of proponents of bilingual education: allowing immigrant children to use their language alongside English may facilitate the transition into a fully English-speaking environment. Such transition is difficult for children with no access to English in their surroundings. Those who fail to comply face the penalty of lower educational attainment, with potential downstream negative effects on integration later in life.

To further substantiate the theoretical mechanism behind the effects of prescriptive policies, I examine heterogeneous effects of English-only laws. Compliance with the law was easier for children who were already exposed to English in their family environment. I use the ethnic background of the mother to proxy for the children's costs of integration effort. Figure 5 shows that English-only laws indeed incentivize effort (in the form of language proficiency) for children with lower effort costs. The graph plots the effect of laws for different subgroups of immigrants. For those with English-speaking mothers (those born in the US, the UK, Canada or Ireland), English proficiency displays a larger increase and the negative effect of laws on intermarriage becomes smaller. In the case of children of native-born mothers, the effect on English proficiency is even larger.

Figure 5. Heterogeneous effects of English-only laws



Notes: The figure plots coefficient estimates and 95% confidence intervals from a regression like the ones reported in columns 1 and 3 of Table 3, for different immigrant subgroups. Underlying regressions are reported in Table B.10.

Taken together, the results illustrate the ambiguous effects of prescriptions. While effort is incentivized for those with low effort costs, the reduced probability of integration for non-compliers lowers effort within this group. The average effect depends on the composition of the immigrant population and the extent to which non-compliance impacts broader integration. For a policy like school laws, where non-compliance hinders educational attainment with severe downstream consequences for other socioeconomic outcomes, broader integration is more likely to be negatively affected.

Discussion and conclusion

This paper presents a framework for evaluating the effects of integration policy, introducing an analytical distinction between incentives and prescriptions as separate components of a policy package. I illustrate the insights of this framework, by causally evaluating the effects of different types of initiatives on immigrant integration in the context of the Americanization movement. Initiatives with a strong emphasis on incentives to exert integration effort, such as Ford's Five-Dollar Day plan, were effective in increasing immigrants' efforts, measured by English proficiency and filing of first papers, and eventual successful integration, measured by naturalization rates and intermarriage with the native-born. Instead, prescription-based policies with muted incentives for effort provision, such as language bans and English-only laws in education, had null effects on effort and negatively impacted social integration. Negative effects were most felt by immigrants with high costs of effort; instead, those who found it easier to adjust in an exclusively English school environment, such as those growing up using English in the home, were more likely to respond to prescriptions by increasing their efforts, and less likely to suffer in terms of social integration outcomes.

Clearly, Ford's Five-Dollar Day plan and English-only policies differ in more respects than just their relative emphasis on incentives versus prescriptions. Yet many of the differences either do not affect, or strengthen the conclusions drawn. Two of the most pronounced differences concern the actors introducing each initiative and the population of affected immigrants. Ford's plan was a private initiative while English-only policies were enacted by US states. As such, the two initiatives may have differed in their goals. An advantage of the identification strategy is that it allows us to treat either initiative as quasi-exogenous and thus abstract from the goals of the policy and the identity of the policymaker and focus instead on immigrant behavior. In both cases, immigrants faced similar outside options. Auto workers disaffected with Ford's plan could have left the company, and immigrants reacting to state-level school laws could have moved to states without a law. Such compositional changes do not affect estimated effects, as exposure to treatment is always defined on the basis of individuals' pre-treatment characteristics (industry and location in 1910 in the case of Ford, and state of birth in the case of English laws).

Immigrant populations also differ across the two examples. Auto workers are first-generation immigrants and, as revealed by Table B.2 they are less integrated by any observable measure than the second-generation immigrant students affected by school-laws. Yet this difference should make a negative effect of prescriptions less and not more likely. Prescriptions are more likely to backfire the higher is the cost of integration effort. Such a cost would have been even higher among first generation immigrant pupils than second generation ones. In other words, if the sample of second-generation immigrants resembled the Ford sample more closely, the effects of prescriptions would have been more negative. This conclusion is also corroborated by the within-sample heterogeneity of Figure 5 and Table B.10.

Additional mechanisms could interact with prescriptions and incentives to produce the observed effects. Both Ford's Americanization program and English-only laws constituted a threat to immigrants' ethnic and cultural identity. Such a threat could have produced resentment, with backlash more likely to ensue among the second generation as parents resisted the ban on ethnic schooling by doubling-down on other forms of identity investment (Greif and Tadelis 2010; Bisin et al. 2011). This mechanism has been highlighted by Carvalho and Koyama 2016, who examine theoretically how assimilationist schooling can cause resistance to education and lower educational investment among minorities. Identity-based mechanisms are complementary to the framework presented here. Viewed through the lens of social identity theory (Tajfel and Turner 1979), investment in integration effort on the one hand and in minority identity on the other are substitute group enhancement strategies (Ellemers and Haslam 2012) that can help minority members achieve positive self-distinction. Policies that promote integration effort can thus soften aggressive forms of investment in minority identity. Policies that instead discourage immigrant effort – as is the case with too high prescriptions – are more likely to produce reactive or oppositional identities (Rumbaut 2005, 2008).

A central takeaway of this study is not that prescriptions invariably fail to promote immigrant integration, but that they are an ambiguous policy tool, that has the potential to backfire. This is more likely to happen when immigrants' costs of effort are high and non-compliance with prescriptions negatively affects chances of integration in domains other than the one targeted by the policy. Evidence from other contexts supports these conclusions. Abdelgadir and Fouka (Forthcoming) study the effects of the 2004 law prohibiting the use of the headscarf in French public schools. Consistent with the framework presented here, responses to the ban were heterogeneous. While most Muslim girls complied with the law and unveiled in school, some of them dropped out. Non-compliance and dropping out had a massive negative effect on the chances of broader economic and social integration and, on average, the educational attainment and long-run labor market outcomes of Muslim girls affected by the ban suffered. The negative impact of the law was mostly felt by girls with higher costs of effort, namely those who were initially less integrated linguistically and psychologically in the French society.

Specifically with respect to language laws in school, the findings of this study are consistent with the broader literature on bilingual education. Monolingualism has been found to have generally null effects on English knowledge and educational performance (Angrist, Chin, and Godoy 2008; Slavin et al. 2011; Chin, Daysal, and Imberman 2013), and negative effects on social and cultural integration (Fouka 2020). Proponents of bilingual education argue that, by allowing for the use of immigrant children's native language in school, bilingual policies ease the transition to the host country's language and have a positive impact on educational performance. This is consistent with the theoretical argument that lower prescriptions can incentivize more effort among those with higher costs – in this case, non-native English speakers growing up in a non-English speaking environment. This, in turn, can increase rates of successful integration.

I have attempted to present a simple and tractable framework for understanding the effects of integration policy. Rather than an all-encompassing theory of integration this framework highlights some mechanisms that determine policy outcomes using an individual-level rational choice framework that focuses on immigrant decisions. This individual-level focus does not diminish the importance of group-level processes, co-ethnic resources and institutions in explaining integration outcomes. One benefit of a framework like the one presented here is that it is flexible enough to allow for the explicit incorporation of such processes, by modeling strategic interactions between immigrants, their leaders (Adida 2014) or elected representatives (Dancygier 2010) and the state. More broadly, this study aims to highlight that rational choice and causal analysis can usefully complement sociological and historical institutionalist approaches to provide a more complete picture of immigrant integration.

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Online Appendix

Table of Contents

A Proofs of propositions

Proof of Proposition 1. Denote the optimal effort under $\underline{e} = 0$ as $e^*(c_i)$. This is implicitly defined by the first order condition for effort

$$P_e^* = \frac{c_i}{f}$$

and is decreasing in c_i. If $e^* > \underline{e}$, then $e^*(c_i)$ is implicitly defined by

$$P_e^* = \frac{\tilde{c}}{f}$$

where $\tilde{c}$ is the cost of effort for immigrant with $e^* = \underline{e}$. If $e^*(c_i) < \underline{e}$ then optimal effort is given by $\underline{e}$ as long as $U(e = \underline{e}) > U(e = 0)$, or as long as

$$P(\underline{e})f - c_i e + h > 0$$

$$c_i < \frac{P(\underline{e})f + h}{\underline{e}} \equiv \hat{c}_i$$

□

Proof of Proposition 2. Differentiating $\hat{c}$ with respect to h gives

$$\frac{\partial \hat{c}}{\partial h} = \frac{1}{\underline{e}}$$

which is positive, and so $\hat{c}^n > \hat{c}$. Immigrants with $\hat{c} < c_i < \hat{c}^n$ provided zero effort and now provide effort $\underline{e}$. Those with $c_i > \hat{c}^n$ continue to provide zero effort and those with $c_i < \hat{c}$ continue to provide effort $\underline{e}$ if $c_i > \tilde{c}$ and effort $e^*(c_i)$, given by the solution to the unconstrained problem, if $c_i < \tilde{c}$. $P_e > 0$, and so integration also increases with increasing effort and remains constant otherwise. □

Proof of Proposition 3. I first establish that $\hat{c}^N < \hat{c}$. The change in the cutoff in response to a change in prescriptions is given by

$$\frac{\partial \hat{c}}{\partial \underline{e}} = \frac{P'(\underline{e}) f \underline{e} - P(\underline{e}) f - h}{\underline{e}^2} \tag{A.1}$$

Optimality implies $P'(\underline{e}) < \frac{c_i}{f}$ for any $c_i > \tilde{c}$. Then also for $\hat{c} > \tilde{c}$:

$$\begin{aligned} P'(\underline{e}) f &< \hat{c} \\ P'(\underline{e}) f &< \frac{P(\underline{e}) f + h}{\underline{e}} \end{aligned} \tag{A.2}$$

Rearranging A.2 and combining with the numerator of A.1 yields

$$\frac{\partial \hat{c}}{\partial \underline{e}} < 0$$

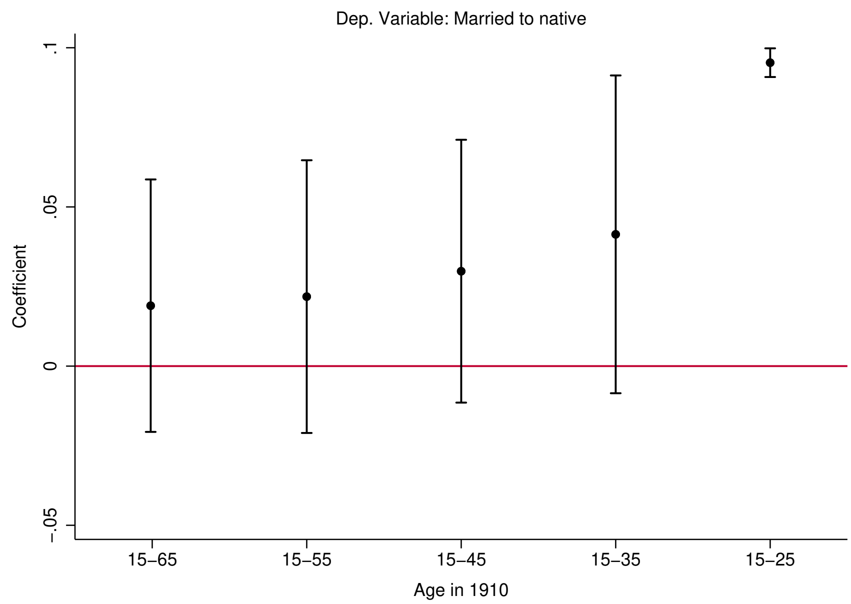
An increase in prescriptions from $\underline{e}$ to $\underline{e}^N$ lowers the cost cutoff for exerting positive integration effort. Immigrants with $\hat{c}^N < c_i < \hat{c}$, who provided effort $\underline{e}$ before the increase in prescriptions, now reduce their effort to zero. Immigrants with $\tilde{c}^N < c_i < \hat{c}^N$ who provided effort $\underline{e} < e^* < \underline{e}^N$ now increase their effort to $\underline{e}^N$. Immigrants with $c_i < \tilde{c}$ continue to provide effort $e^* > \underline{e}^N$ and immigrants with $c_i > \hat{c}$ continue to provide zero effort.

$\square$

Proof of Proposition 4. The result follows directly from Proposition 3 and the fact that $P_e > 0$. $\square$

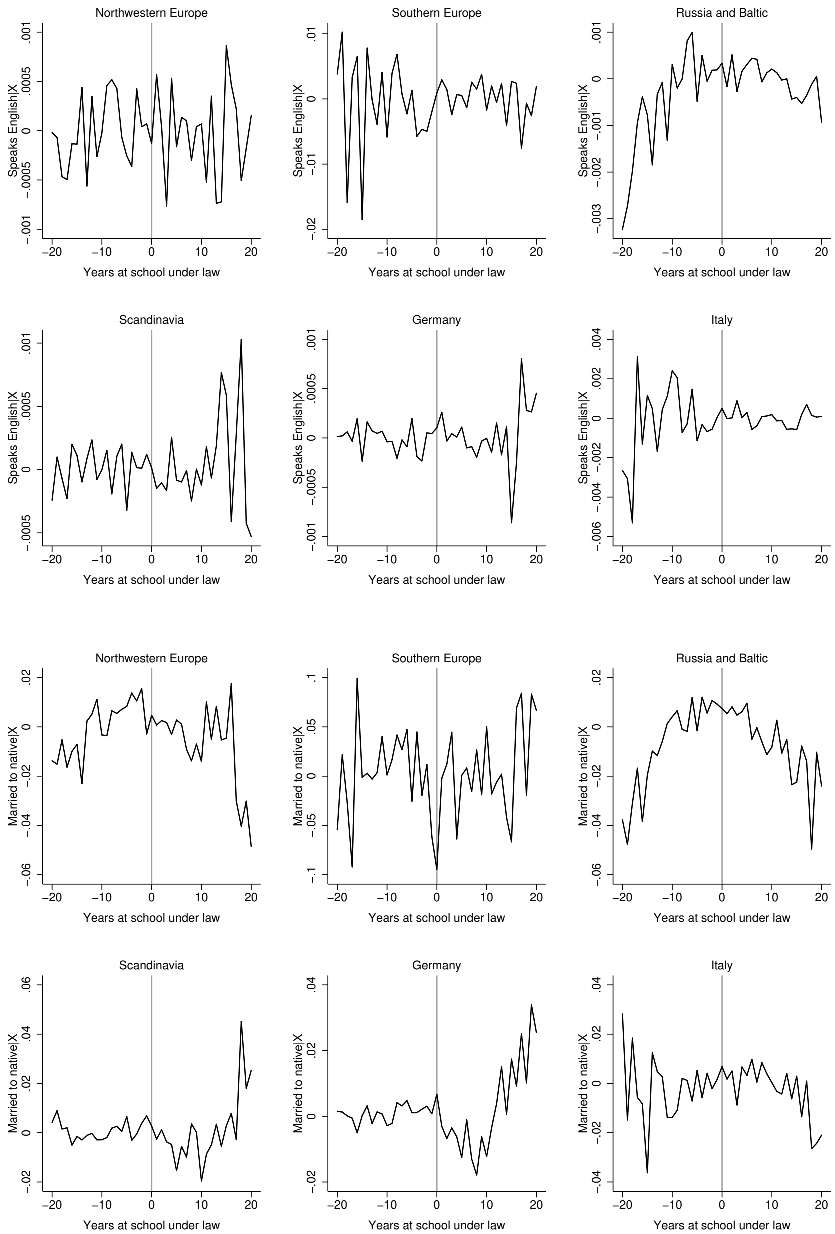
B Additional figures and tables

Figure B.1. Effect of Ford's program on rates of intermarriage by age in 1910



Notes: The figure plots coefficient estimates and 95% confidence intervals from a regression like the one specified in column 4 of Table 2, for the age groups indicated on the x-axis.

Figure B.2. Effect of language bans by nationality



Notes: The figure plots residuals of the variable in the y-axis from a regression on indicators for state of birth, father and mother's birthplace, age and census decade. The sample consists of US-born men with a foreign-born father. Data is from the pooled 1930 5% and 1960 1% samples.

Table B.1. Cities with a Ford manufacturing plant

Atlanta, GA	Kansas City, MO
Buffalo, NY	Kearney, NJ
Cambridge, MA	Los Angeles, CA
Charlotte, NC	Louisville, KY
Chicago, IL	Memphis, TN
Cincinnati, OH	Milwaukee, WI
Cleveland, OH	Minneapolis, MN
Columbus, OH	Oklahoma City, OK
Dallas, TX	Omaha, NE
Dearborn, MI	Philadelphia, PA
Denver, CO	Pittsburgh, PA
Des Moines, IA	Portland, OR
Detroit, MI	St. Louis, MO
Highland Park, MI	San Francisco, CA
Houston, TX	Seattle, WA
Indianapolis, IN	

Source: The Henry Ford.

Table B.2. Summary statistics

Variables	Mean	S.D.	Min	Max	N
Panel A: Linked dataset, 1910-1920					
Age	42.760	13.094	16	77	237092
Speaks English	0.895	0.306	0	1	234901
First papers	0.128	0.334	0	1	230516
Naturalized	2.424	0.812	0	4	237092
Married	0.696	0.460	0	1	237092
Married to native	0.091	0.288	0	1	165050
Panel B: Linked dataset, 1910-1930					
Age	47.136	15.281	16	87	201636
Speaks English	0.923	0.266	0	1	198536
First papers	0.091	0.287	0	1	195701
Naturalized	2.308	0.752	0	4	201636
Married	0.687	0.464	0	1	201636
Married to native	0.094	0.293	0	1	138578
Panel C: IPUMS samples					
Age	30.820	11.392	15	80	6549935
Speaks English	0.995	0.0704	0	1	5796993
Married	0.467	0.499	0	1	6549935
Married to native	0.3959	0.489	0	1	3026074

Notes: Data is from the complete count 1910-1930 censuses (Panels A and B) and the 5% 1930 and 1% 1960 IPUMS samples (Panel C). In Panels A-B, data is restricted to foreign-born men aged 15-65 and in the labor force in 1910. In Panel C, data is restricted to US-born men with a foreign-born father. Summary statistics for *Married to native* are reported for the subset who is married with a spouse present in the household.

Table B.3. Change in outcomes of Detroit auto workers, narrower control group

Dependent variable	Speaks English		First papers		Naturalized	
	(1)	(2)	(3)	(4)	(5)	(6)
Detroit × Auto industry × 1920	0.0178	0.0248	0.0421	0.0321	0.0422	0.0664
	(0.00562)	(0.0134)	(0.00580)	(0.0104)	(0.00443)	(0.0136)
Mean dep. variable in 1910	0.834	0.766	0.115	0.122	0.607	0.544
Observations	127756	44574	122412	41768	122412	41768
R-squared	0.616	0.630	0.547	0.544	0.703	0.714
In Manufacturing		✓		✓		✓

Notes: Data restricted to foreign-born men aged 15-65, in the labor force in 1910, living in cities with a Ford assembly plant that were located in a Midwestern state. All regressions control for individual fixed effects, city (in 1910) × year and industry (in 1910) × year interactions. Two-way clustered standard errors at the city and industry (in 1910) level reported in parentheses.

Table B.4. Effects of Ford's program on rates of intermarriage, robustness

Dependent variable	Married to native			
	Midwestern cities		Pooled linked samples 1920 and 1930	
	(1)	(2)	(3)	(4)
Detroit × Auto industry	0.104	0.109	0.00737	0.0106
	(0.0000258)	(0.0136)	(0.00717)	(0.0150)
Mean dep. variable in 1910	0.066	0.052	0.129	0.120
Observations	6348	2636	21721	8619
R-squared	0.238	0.223	0.248	0.264
In manufacturing		✓		✓

Notes: Census year 1930. Data restricted to foreign-born men aged 15-25, in the labor force in 1910 and not married in 1910. All regressions control for city (in 1910) by country or birth, industry (in 1910) by country of birth, and age in 1910 fixed effects as well as the share of first and second generation immigrants in the city and industry in 1910. Columns 3 and 4 additionally include an indicator for observations that belong to the 1910-1930 linked sample. Two-way clustered standard errors at the city and industry (in 1910) level reported in parentheses.

Table B.5. Change in economic outcomes of Detroit auto workers

Dependent variable	Log occupational earnings score		Owns home		Mortgage	
	(1)	(2)	(3)	(4)	(5)	(6)
Detroit × Auto industry × 1920	0.0223	0.0145	0.0202	0.0208	0.00721	0.0117
	(0.00425)	(0.00735)	(0.00515)	(0.00685)	(0.0136)	(0.0184)
Mean dep. variable in 1910	3.212	3.231	0.323	0.307	0.542	0.575
Observations	164678	54308	200026	67436	45744	14588
R-squared	0.704	0.640	0.658	0.657	0.640	0.639
In Manufacturing		✓		✓		✓

Notes: Data restricted to foreign-born men aged 15-65, in the labor force in 1910, living in cities with a Ford assembly plant that were located in a Midwestern state. In column 3, data is further restricted to homeowners. All regressions control for individual fixed effects, city (in 1910) × year and industry (in 1910) × year interactions. Two-way clustered standard errors at the city and industry (in 1910) level reported in parentheses.

Table B.6. Accounting for the mediating effect of economic outcomes

Dependent variable	Speaks English		First papers		Naturalized		Married to native	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Detroit × Auto industry × 1920	0.0191	0.0156	0.0382	0.0302	0.0478	0.0660		
	(0.005)	(0.008)	(0.005)	(0.008)	(0.004)	(0.009)		
Detroit × Auto industry							0.0704	0.0953
							(0.015)	(0.001)
Observations	219428	71962	210632	67600	210632	67600	10938	4216
R-squared	0.614	0.624	0.545	0.541	0.707	0.715	0.240	0.243
In Manufacturing		✓		✓		✓		✓

Notes: Coefficients represent the Average Controlled Direct Effect (ACDE) estimated using sequential g-estimation, treating an indicator for homeownership as mediator, as described in Acharya, Blackwell, and Sen (2016). Columns 1-6 replicate Table 1. Columns 7-8 replicate columns 3-4 of Table 2. Block bootstrapped standard errors (computed after 500 repetitions) clustered two-way at the city and industry (in 1910) level in parentheses.

Table B.7. Heterogeneous effects of Ford's program

Dep. variable	Speaks English		First papers		Naturalized		Naturalized (excl. enemy aliens)		Married to native	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: By native language									
	Non-English	English	Non-English	English	Non-English	English	Non-English	English	Non-English	English
Detroit × Auto industry × 1920	0.0359***	-0.00157	0.0423***	0.0149	-0.0463***	0.122***	0.0344*	0.122***		
	(0.00409)	(0.00331)	(0.00472)	(0.0111)	(0.00964)	(0.00767)	(0.0194)	(0.00767)		
Detroit × Auto industry									0.164***	0.0127
									(0.0311)	(0.0292)
Mean dep. variable	0.859	0.990	0.136	0.105	0.596	0.728	0.521	0.728	0.0839	0.282
Observations	163992	55428	156526	54098	156526	54098	79518	54098	3412	804
R-squared	0.616	0.507	0.543	0.554	0.718	0.654	0.711	0.654	0.194	0.207
	Panel B: By linguistic distance from English									
	< median	≥ median	< median	≥ median	< median	≥ median	< median	≥ median	< median	≥ median
Detroit × Auto industry × 1920	0.0245***	0.0499***	0.0297***	0.0428***	0.112***	-0.0565***	0.112***	0.0423**		
	(0.00621)	(0.00849)	(0.00529)	(0.00916)	(0.00566)	(0.00952)	(0.00566)	(0.0194)		
Detroit × Auto industry									0.0321***	0.168***
									(0.0111)	(0.0319)
Mean dep. variable	0.934	0.854	0.122	0.135	0.622	0.601	0.662	0.430	0.186	0.083
Observations	103776	114724	100482	109262	100482	109262	100482	32254	1542	2674
R-squared	0.614	0.617	0.550	0.543	0.687	0.724	0.687	0.705	0.253	0.195

Notes: Sample and specifications in columns 1-6 follow column 1 of Table 1. In columns 7-8 they follow column 4 of Table 2. ≥ *median* and < *median* refer to the median of distance of a language from English, following Chiswick and Miller 2005. Languages assigned to nationalities based on place of birth. In cases of countries with more than one official language (e.g. Switzerland), the average of official languages was used.

Table B.8. Summary of school laws

State	Year	Type of law
Alabama	1919	English-only, Foreign language ban
Arkansas	1919	English-only
Arizona	1913	English-only
Colorado	1919	English-only
Connecticut	1923	English-only
Delaware	1919	English-only, Foreign language ban
Iowa	1897	English-only
Iowa	1919	Foreign language ban
Idaho	1919	English-only
Illinois	1919	English-only
Indiana	1919	English-only, German language ban
Kansas	1919	English-only
Louisiana	1918	English-only, German language ban
Maine	1919	English-only
Minnesota	1919	English-only
North Dakota	1918	English-only
Nebraska	1919	English-only, Foreign language ban
New Hampshire	1919	English-only
Nevada	1919	English-only
New York	1909	English-only
Ohio	1919	English-only, German language ban
Oklahoma	1919	English-only, Foreign language ban
Oregon	1919	English-only
Pennsylvania	1919	English-only
Rhode Island	1909	English-only
South Dakota	1919	English-only, Foreign language ban
West Virginia	1919	English-only

Source: Edwards (1923) and Knowlton Flanders (1925).

Table B.9. English-only laws and long-run outcomes of the second generation, adjacent states

Dep. variable	Speaks English		Married to native	
	(1)	(2)	(3)	(4)
English-law × Treated cohort	0.00300	0.000195	-0.0468	-0.0210
	(0.00316)	(0.000386)	(0.0176)	(0.00942)
Mean dep. variable	0.995	0.995	0.396	0.396
Observations	5796993	5796983	3025951	3025943
R-squared	0.0666	0.147	0.0387	0.121

Notes: The sample consists of US-born men with a foreign-born father. Data is from the 1930 5% IPUMS sample (columns 1 and 2) and from the pooled 1930 5% and 1960 1% samples (columns 3 and 4). *Married to native* is an indicator for individuals with a native-born spouse of native-born parents. All columns include indicators for year and state of birth. Columns 2 and 4 additionally include indicators for age and father and mother's birthplace. Standard errors are clustered at the state of birth level.

Table B.10. Heterogeneous effects of English-only laws

Dep. variable	Speaks English			Married to native		
	Everyone	English-speaking mother	Native-born mother	Everyone	English-speaking mother	Native-born mother
	(1)	(2)	(3)	(4)	(5)	(6)
English-law × Treated cohort	0.000195	0.000931	0.00152	-0.0468	-0.0134	-0.0193
	(0.000386)	(0.000522)	(0.000758)	(0.0176)	(0.00728)	(0.0118)
Mean dep. variable	0.995	0.997	0.996	0.396	0.521	0.560
Observations	5796983	2084884	1372439	3025951	1164247	760463
R-squared	0.147	0.130	0.149	0.0387	0.0488	0.0442

Notes: The sample consists of US-born men with a foreign-born father (columns 1 and 4), a foreign-born father and a mother from an English-speaking country (columns 2 and 5) or a foreign-born father and a native-born mother (columns 3 and 6). Data is from the 1930 5% IPUMS sample (columns 1-3) and from the pooled 1930 5% and 1960 1% samples (columns 4-6). *Married to native* is an indicator for individuals with a native-born spouse of native-born parents. All columns include indicators for year, state of birth, age and father and mother's birthplace. Standard errors are clustered at the state of birth level.

C Linking procedure, diagnostics and robustness

I link census records forward in time using the procedure developed by Abramitzky, Boustan, and Eriksson (2012) and Abramitzky, Boustan, and Eriksson (2014).[17] I first clean first and last names by stripping them of special characters, occupational titles, and initials. I then generate the NYSIIS (New York State Identification and Intelligence System) phonetic equivalent of first and last names. The NYSIIS algorithm assigns the same code to words spelled differently, but pronounced in the same way (e.g. John and Jon). I keep observations that are unique by NYSIIS name and birth year (within a five year band) and match those observations forward using an iterative procedure, described in more detail in Abramitzky, Boustan, and Eriksson (2012). Observations that are linked to a unique record in the following census decade are considered matched. Those linked to multiple records are discarded. Remaining unmatched observations are re-matched forward allowing for a one-year band around the birth year (one year older or younger) in the first iteration and a two-year band in the second. Observations with multiple matches or no match within five years of age are dropped.

As can be seen in Table C.1, match rates range between 9.9% and 11.6%, which is comparable to the match rates reported by Abramitzky, Boustan, and Eriksson (2014) for foreign-born men. As expected, linked men have a more assimilated profile in the origin year. They are more likely to speak English and be US citizens, less likely to be illiterate, and have been in the US for longer. They also differ in the characteristics of their name strings, with longer first names and less common first and last names.

These differences do not compromise the internal validity of the empirical design, but may have implications for potential generalizations of the study's conclusions to the broader immigrant population. Following recent recommendations in the literature on historical census linking, summarized in Bailey et al. (2019), I examine the robustness

[17] I use the replication code provided by the authors at https://ranabr.people.stanford.edu/matching-codes.

Table C.1. Comparison of linked and non-linked records

	1910–1920			1910–1930		
	Not linked	Linked	Difference	Not linked	Linked	Difference
Age	36.414	37.474	-1.059	36.538	36.529	0.008
	(11.853)	(12.067)	(0.038)	(11.925)	(11.486)	(0.040)
Years in US	15.117	17.627	-2.510	15.214	17.178	-1.964
	(12.278)	(12.494)	(0.039)	(12.351)	(11.991)	(0.042)
Literate	0.885	0.93	-0.045	0.886	0.933	-0.047
	(0.319)	(0.255)	(0.001)	(0.318)	(0.251)	(0.001)
Speaks English	0.739	0.850	-0.111	0.740	0.865	-0.125
	(0.439)	(0.357)	(0.001)	(0.438)	(0.342)	(0.001)
First papers	0.106	0.116	-0.0105	0.105	0.122	-0.017
	(0.308)	(0.321)	(0.001)	(0.307)	(0.328)	(0.001)
Naturalized	0.469	0.580	-0.111	0.471	0.583	-0.113
	(0.499)	(0.493)	(0.0016)	(0.499)	(0.493)	(0.002)
First name length	5.784	6.048	-0.264	5.786	6.080	-0.294
	(1.722)	(1.672)	(0.0054)	(1.722)	(1.662)	(0.006)
Last name length	6.776	6.702	0.0738	6.777	6.674	0.104
	(1.873)	(1.758)	(0.0059)	(1.872)	(1.753)	(0.006)
First name commonness	0.007	0.006	0.0012	0.007	0.006	0.001
	(0.011)	(0.010)	(0.000)	(0.011)	(0.010)	(0.000)
Last name commonness	0.0003	0.0001	0.0001	0.0003	0.0002	0.0001
	(0.001)	(0.0007)	(0.000)	(0.001)	(0.001)	(0.000)
Observations	853,890	111,894	111,894	870,589	95,195	95,195
Match rate			11.6%			9.86%

Notes: The table reports means and standard deviations (in parentheses) for various characteristics of matched and unmatched records in 1910. The numbers in parentheses under the mean differences are standard errors on point estimates of differences.

of the paper's main conclusions to re-weighting the linked dataset to reduce selection and improve representativeness.

Following Bailey et al. (2019), I construct inverse propensity weights by running a probit model of linked status on first and last name length, first and last name commonness, and a quartic polynomial in age.[18] I then compute the predicted probability of being linked, denoted by P_i, and reweigh the data by $\frac{1-P_i}{P_i}\frac{q}{1-q}$, where q is the share of records that are linked. Table C.2 reports the results. Columns 1–6 replicate Table 1 and columns 7–8 replicate columns 3–4 of Table 2. With the exception of first papers, which seem sensitive to weighting when the sample is restricted to manufacturing workers, all results go through, and magnitudes of estimated coefficients increase.

This exercise limits concerns about low external validity. Additionally, the use of inverse probability weighting in combination with considering only linked records that are unique within a five-year band substantially reduces Type I error. As shown by Bailey et al. (2019), the combination of the two approaches delivers results statistically very similar to those produced using hand-linked data.

Table C.2. Robustness to inverse probability weighting

Dependent variable	Speaks English		First papers		Naturalized		Married to native	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Detroit × Auto industry × 1920	0.0346	0.0290	0.0114	0.00782	0.0514	0.0612		
	(0.00301)	(0.0127)	(0.00449)	(0.00826)	(0.00315)	(0.00841)		
Detroit × Auto industry							0.0960	0.112
							(0.0151)	(0.00229)
Observations	219406	71960	210612	67600	210612	67600	10938	4216
R-squared	0.614	0.625	0.542	0.537	0.702	0.709	0.245	0.248
In Manufacturing		✓		✓		✓		✓

Notes: Columns 1-6 replicate Table 1. Columns 7-8 replicate columns 3-4 of Table 2. All regressions are weighted by the inverse probability of being linked, estimated using a probit model of linked status on first and last name length and commonness, and a quartic polynomial in age.

Differences between linked and not linked records still allow for consistent estimation

[18]Commonness of a first or last name is computed as the share of all men in the 1910 census with that name.

of treatment effects. Concerns arise when those differences are also correlated with treatment status. Specifically, if more foreign-born men with a more integrated profile already in 1910 are more likely to be linked among Detroit and Highland Park auto workers, than among those in other cities and industries, one may be worried that estimated effects attributed to Ford's Five-Dollar Day plan are in fact due to differential sample composition.[19]

Table C.3 provides evidence against such concerns. I regress a number of outcomes in 1910 on an indicator for a linked record, an indicator for employment in the auto industry, an indicator for Detroit and Highland Park, as well as all three indicators' bilateral interactions. A significant triple interaction coefficient indicates that differences in baseline (1910) integration between linked and non-linked records are systematically correlated with treatment status. This is not the case for any of the outcomes considered, with the exception of English proficiency, which is statistically significant at the 10% level and only when the sample is not restricted to workers in the manufacturing sector. Furthermore, differences are not consistent in direction across outcomes. Linked records in the Detroit auto industry are more likely to speak English and have first papers, but are less likely to be naturalized and have arrived more recently to the US. This indicates no systematic pattern in linking more assimilated individuals in treated cities and industries.

[19]Individual fixed effects would still allow for consistent estimation, as long as trends in outcomes are the same for more and less integrated workers, but the data does not allow us to assess whether this identifying assumption holds.

Table C.3. Correlation of match rate with auto workers' characteristics in 1910, by city and industry

Dependent variable	Years in US		Literate		Speaks English		First papers		Naturalized	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel A: 1910 to 1920					
Detroit × Auto industry × Matched	-0.813	-0.848	0.00953	0.00861	0.0351	0.0206	0.0300	0.0257	-0.0439	-0.0488
	(0.697)	(0.743)	(0.0110)	(0.0126)	(0.0195)	(0.0214)	(0.0208)	(0.0217)	(0.0309)	(0.0326)
Observations	965784	340510	965784	340510	947082	335296	907663	314220	907663	314220
R-squared	0.00510	0.00738	0.00286	0.00511	0.00878	0.0146	0.000169	0.000397	0.00571	0.00822
					Panel B: 1910 to 1930					
Detroit × Auto industry × Matched	-0.100	0.0380	0.00975	0.00532	0.00842	0.00321	0.0237	0.0103	-0.00653	0.00916
	(0.724)	(0.769)	(0.0103)	(0.0119)	(0.0192)	(0.0212)	(0.0224)	(0.0235)	(0.0323)	(0.0342)
Observations	965784	340510	965784	340510	947082	335296	907663	314220	907663	314220
R-squared	0.00311	0.00606	0.00271	0.00558	0.00943	0.0167	0.000310	0.000585	0.00512	0.00873
In Manufacturing		✓		✓		✓		✓		✓

Notes: Data consists of foreign-born men aged 15-65 and in the labor force in 1910, who lived in cities with a Ford assembly plant. All regressions control for indicators for matched records, workers in the auto industry, workers in Detroit and Highland Park and their interactions. Robust standard errors in parenthesis.