# In or Out? Xenophobic Violence and Foreigner Integration. Evidence from 19th century France*

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May 27, 2024

How do foreigners respond to xenophobic violence? Do they move out, leaving their home, or do they buy in, assimilating further? We develop a stylized theoretical framework to explain why exposure to violence can yield both more exits and more assimilation. We use an exogenous spike in xenophobic violence in 19th-century France and fine-grained individual data to provide causal evidence of this dual effect. We also study how foreigners' response varies with the degree of exposure to violence and their prior level of integration in the host country, highlighting the importance of thinking of foreigners as a heterogeneous group.

*We thank conference and seminar participants at the EUI, KCL, Paris Migration Economics Seminar (PMES), the 2023 LSE-NYU conferences, and the 2023 Workshop on "Migration and Politics: Current and Future Challenges" in Caglio for excellent comments and suggestions. We thank Areen Dakessian, Sabrina Buratto, Anais Pintat, Romane Branthomme, César Pichon, Anaelle Thoreau, and Marit Pauwelyn for excellent research assistance. Funding for this project was provided by STICERD and the department of government at the LSE. All remaining errors are the authors' responsibility.

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In November 2023, leading members of the Alternative für Deutschland (AfD) as well as some neo-Nazis and a few conservative politicians met in a villa in Postdam near the capital city of Berlin. The alleged topic of the discussion: "remigration." The plan: the deportation to Africa of up to two million people, foreigners, naturalized, unassimilated immigrants, and, to be on the safe side, a few defenders of refugee rights (European Council on Refugees and Exiles, 2024; Oltermann, 2024). While not as extreme as the AfD, other politicians have also repeatedly targeted foreigners. Donald Trump stated that Mexicans illegally residing in the US are likely to be murderers and rapists (Washington Post, July 8, 2015), the British government has implemented a hostile environment against foreigners (The Guardian, November 28, 2017), the French legislature passed a bill promoting national preference in access to public goods (Le Monde, December 19, 2023).

Nor is it a new phenomenon. Throughout history, waves of migration have led to tensions and conflicts between natives and foreigners. The COVID-19 pandemic unleashed a spate of anti-Asian behaviors in the USA (Gover et al., 2020) and in Italy (Dipoppa et al., 2023). Anti-foreigner sentiments spiked in Europe during the 2015 Refugee crisis (Benček and Strasheim, 2016; Rigoni, 2016). Anti-Islamic hate crimes rose in the United States after the terrorist attacks of September 11, 2001 (Byers and Jones, 2007). Foreign populations were forcibly expelled from their residence after World War 2 (Becker and Ferrara, 2019; Becker et al., 2020). The entry of the US in World War I came along with violence against Germans (Nagler, 1993). Foreigners were repeatedly lynched in 19th century America (Seguin and Rigby, 2019) and 19th century France (Dornel, 2004).

As tension rises, foreigners react. But how do they do so? Recent studies exploring this question yield, what seems like, mixed findings. Some scholars find that foreigners disengage from their host community (Fouka, 2020; Gould and Klor, 2016; Grewal and Hamid, 2022; Steinhardt, 2018) or simply leave (Buggle et al., 2023; Ferrara and Fishback, 2022) as a result of xenophobic violence, discrimination or hostility. Others document an increase in assimilation efforts in the face of violence and discrimination (Fouka, 2019; Saavedra, 2021). This evidence, we argue, is not contradictory. Assuming that xenophobic violence increases the cost of remaining in the host country (either as a foreigner or as a national) and that foreigners are heterogeneous in their baseline integration level, we show that exposure to violence can lead less-integrated foreigners to exit and better-integrated ones to assimilate. On aggregate, exposure to xenophobic violence

can lead to both more disengagement and more assimilation.

Using the case of France in the 19th century, we present causal evidence consistent with this claim. To empirically document how xenophobic violence affects foreigners' choices, we take advantage of spontaneous violence against Italians that lasted a few days in 1894. These events were triggered by the assassination of the French President Sadi Carnot by an Italian anarchist in Lyon on June 24, 1894. As reported in local and national newspapers, anti-Italian xenophobia flared in various locations, especially big cities, in France, but was particularly violent and prolonged in Lyon (Rhône department) where the president was assassinated (Dornel, 2004). As Italians were almost exclusively the target of violence, we use a difference-in-differences design, zooming in on the Rhône department, using nationals from other countries as the control group and the year 1894 as the separation between the pre- and the post-treatment period.

Our information about foreigners comes from two sources: the nominative census records of the Rhône department, where Lyon is located, and official naturalization decrees of foreigners residing in the Rhône. We use the first data source, available for the years 1881, 1886, 1891, and 1896, to estimate the rate of exit of Italians and other foreigners from their municipality of residence; we use the second source, available every year, listing both successful and unsuccessful applications to measure Italians and other foreigners' efforts to assimilate into French society. Our main sample comprises 30,355 foreigners living in 251 municipalities of the Rhône department in 1886 and 1891, 55% of which are Italians and 28% are Swiss. Finally, we use a large corpus of 24,080 regional newspaper editions published in 1893 and 1894 to document the magnitude of the violence and identify neighborhood-level variation in exposure to violence.

Unlike previous works, we have access to the full population of foreigners over multiple periods of time. As a result, we can look both at emigration and assimilation when other studies have generally looked at those outcomes separately. Further, our research design estimates the *unconditional* causal effect of exposure to xenophobic violence on assimilation, when papers to date relying on aggregate level repeated cross-section have generally only been able to estimate the effect of xenophobic events *conditional* on foreigners still living in the municipality or country; that is, on a sample affected by the treatment they investigate. We can also leverage

rich pre-treatment characteristics to document foreigners' heterogeneous responses to being exposed to xenophobic violence by baseline integration levels. Finally, with our focus on a very local shock, we can look at foreigners' reactions as a function of their degree of exposure to xenophobic riots.

In line with our argument briefly described above, we document that after the event Italians were both more likely to leave their municipality of residence and more likely to assimilate as measured by applying for naturalization. We further show stronger exit rates and assimilation in municipalities with direct exposure to violence (where we have newspaper evidence that rioting occurred). We also explore whether Italians with different expected levels of integration react differently to the flares of xenophobia. We use three variables as proxies of integration: the nationality of the partner (French versus foreign), the position in the household (heads, spouses and children versus employees and servants), and occupation (shop owners, who have invested capital in France, versus workers, who are more mobile). Across all dimensions, we document that individuals who are more likely to be well integrated into French society (in mixed marriage, with family, as shop owner) are less likely to exit and more likely to naturalize following the riots of 1894.

Overall, our findings, both from a theoretical and empirical perspective, indicate the importance of looking at foreigners' varied responses to shocks to their mode of living. Like any other group, foreigners do not have a monolithic attitude towards their host society. We are certainly not the first ones to make this important point as we discuss in the next section. Yet, we think that fully embracing the idea that foreigners vary in several relevant dimensions would help better understand how foreigners react to pressure to assimilate or to leave, a topic that remains highly salient today.

# 1 Thinking about foreigners' response to violence

In this section, building on the literature, we develop a theoretical framework to better understand foreigners' responses when exposed to violence. We present a stylized decision-theoretic model in the main text and provide different ways to micro-found the payoff functions in Online Appendix A.2. Proofs of all results can be found in Online Appendix A.1.

Foreigners, we postulate, can make one of three choices. The first option is to exit. In its purest form, a foreigner returns to their home country. Exit, however, can also correspond to leaving one's commune or county of residence. This is the type of exit that Ferrara and Fishback (2022) study, this is also the outcome we can analyze in this paper. A second possible choice is to assimilate. Assimilation can take different forms: adopting natives' cultural norms (Jaschke et al., 2022), changing one's name or one's children's names (Fouka, 2019; Saavedra, 2021), marrying a native (Gould and Klor, 2016), or naturalizing (Ferrara and Fishback, 2022; Fouka, 2019). The third option consists basically of keeping one's situation as it is. It serves as the default option, relative to the other two (hence, what retaining one's status means is a function of the type of assimilation outcome researchers consider).[1]

A foreigner's choice depends on three factors. The first is what we call their level of integration: how well they are integrated into the host society before the choice they must make. Well-integrated individuals value assimilation the most, least integrated individuals see the highest benefit from exiting. Individuals also react to possible *exposure* to violence. From the onset, we insist that the phrase 'exposure to violence' should be understood broadly. It can be proper violence such as massacres or xenophobic attacks (Gould and Klor, 2016; Steinhardt, 2018), like the anti-foreigner riots we study here. Violence can also take less visible and more pernicious form, such as violent languages in newspapers (Ferrara and Fishback, 2022), a rise in discrimination following the onset of a conflict (Moser, 2012), or the prohibition of certain practices (Abdelgadir and Fouka, 2020; Fouka, 2020). The key assumption is that observing more violence changes a foreigner's perception of the community they live in. It makes keeping one's foreigner status more precarious. Exposure to violence can also make one rethink the benefits of assimilating. Lastly, the values of each option depends on some idiosyncratic factors, unobservable by the researchers.

Formally, we consider a mass one of foreigners who each make one of three choices: exit ($E$), assimilate ($A$), or keep their current status ($F$). Each individual $i$ is characterized by an underlying type $\tau^i$ distributed according to the continuous cumulative distribution function

[1]Building on Hirschman (1972), we could also have added the option of voicing concerns. Yet, it is not obvious how this option would materialize when it comes to foreigners. Non-nationals' ability to organize and influence issues is limited. Further, it is not directly clear how Hirschman's theory, which looks at the individuals' reactions to organization decline, applies to foreigners. Exit is a much more dramatic choice for foreigners than for organization members who often have close alternatives to choose from. These almost costless outside options are generally not available to foreigners.

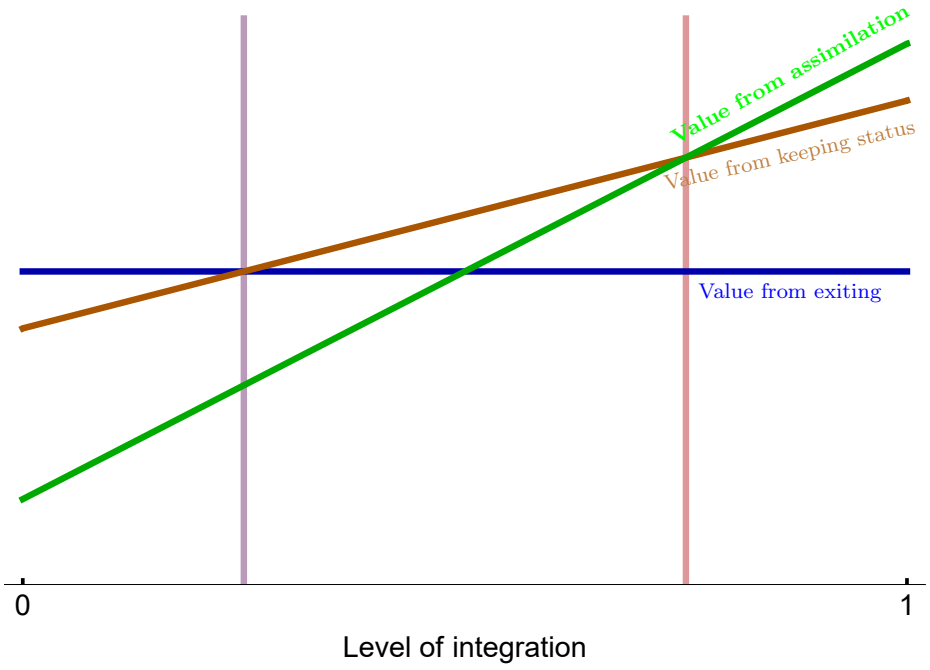
$G(\cdot)$ over the interval $[\overline{\tau}, \underline{\tau}]$. This type captures the underlying level of integration of individual $i$. A parameter $\kappa \geq 0$ represents an individual's exposure to violence, with $\kappa = 0$ meaning no violence and greater $\kappa$ greater degree of exposure (either in terms of the intensity of violence and/or how far violence occurred from a foreigner's home). Idiosyncratic factors are captured by random shocks for each option: $\epsilon_i^J$ ($J \in \{A, E, F\}$ i.i.d. for each individual and each option. As it is common in random utility choices, we impose that all shocks are distributed according to a type-1 Extreme Value distribution. Each foreigner observes their type and the various shocks before choosing one of the three options.

Individual $i$ gets $V^E + \epsilon_i^E$ if they exit. They receive a payoff of $V^F(\tau_i, \kappa) + \epsilon_i^F$ if they maintain their current status. They obtain $V^A(\tau_i, \kappa) + \epsilon_i^A$ if they assimilate. The function $V^J(\cdot, \cdot)$ captures continuation values. We assume that all functions and their derivatives are continuous and they satisfy the following conditions: $V_\tau^A(\tau_i, \kappa) > V_\tau^F(\tau_i, \kappa) > 0$ and $V_\kappa^F(\tau_i, \kappa) \leq V_\kappa^A(\tau_i, \kappa) \leq 0$ (with one strict inequality). Both the value of assimilating and the value of keeping the status quo increase with an individual's level of integration, with the gain for assimilation increasing faster. In other words, the incentives to assimilate relative to keeping the status quo are higher for well-integrated individuals than those with low levels of integration. Exposure to violence reduces the value of the status quo and assimilation with the incentives to assimilate decreasing at a lower rate than those of remaining foreigners. These assumptions are based on the idea that well-integrated foreigners value political rights more and better access to the labor market (e.g., Govind, 2021) that assimilation provides. Further, assimilation (especially naturalization) comes with a range of legal protection, including against deportation.[2] The assumption that the value of exit is unaffected by the level of integration or the degree of exposure to violence is without loss of generality (all that matters is the relative value of changes). Figure 1 offers an illustration of the continuation value for each function.

Each foreigner's choice is deterministic and reveals little on its own about the underlying parameter of interests. However, as is common for these decision-theoretic models, we can

[2]These assumptions are also motivated by the context of 19th-century France we study. On 10 August 1889, the French government issued a decree restricting the number of foreign workers who could be employed in public works (known as 'decret Millerand'). While this decree was not widely applied, it does represent a threat hanging over foreigners who do not naturalize. Further, since 1847, any legally residing foreigners could be expelled by decision of the French government.

Figure 1: Payoffs by level of integration




determine the probability that one individual chooses one of the three options:

$$P(E|\tau_i,\kappa) = \frac{\exp(V^E)}{\exp(V^E) + \exp(V^F(\tau_i,\kappa) + \exp(V^A(\tau_i,\kappa))} \tag{1}$$

$$P(F|\tau_i,\kappa) = \frac{\exp(V^F(\tau_i,\kappa))}{\exp(V^E) + \exp(V^F(\tau_i,\kappa)) + \exp(V^A(\tau_i,\kappa))} \tag{2}$$

$$P(A|\tau_i,\kappa) = \frac{\exp(V^A(\tau_i,\kappa))}{\exp(V^E) + \exp(V^F(\tau_i,\kappa)) + \exp(V^A(\tau_i,\kappa))} \tag{3}$$

It is quite direct that the probability a foreigner exits is strictly decreasing with their level of integration. The reverse holds true for the probability a foreigner assimilates. In what follows, we work under the assumption that maintaining the status quo is also increasing with the level of integration: $P_\tau(F|\tau_i,\kappa) > 0$.

Using Equations 1-3, we can also compute the share of foreigners who choose each of three

options:

$$\mathbb{P}(E|\kappa) = \int_{\underline{\tau}}^{\overline{\tau}} \frac{exp(V^E)}{exp(V^E) + exp(V^F(\widetilde{\tau}, \kappa)) + exp(V^A(\widetilde{\tau}, \kappa))} dG(\widetilde{\tau}) \tag{4}$$

$$\mathbb{P}(F|\kappa) = \int_{\underline{\tau}}^{\overline{\tau}} \frac{exp(V^F(\widetilde{\tau}, \kappa))}{exp(V^E) + exp(V^F(\widetilde{\tau}, \kappa)) + exp(V^A(\widetilde{\tau}, \kappa))} dG(\widetilde{\tau}) \tag{5}$$

$$\mathbb{P}(A|\kappa) = \int_{\underline{\tau}}^{\overline{\tau}} \frac{exp(V^A(\widetilde{\tau}, \kappa))}{exp(V^E) + exp(V^F(\widetilde{\tau}, \kappa)) + exp(V^A(\widetilde{\tau}, \kappa))} dG(\widetilde{\tau}) \tag{6}$$

With these equations, we can compute the theoretical equivalent to empirical estimates of the effect of exposure to violence, which are simply: $\mathbb{P}(E|\kappa) - \mathbb{P}(E|0)$ for exit and $\mathbb{P}(A|\kappa) - \mathbb{P}(A|0)$ for assimilation with $\kappa > 0$. Our model then yields the following first result:

**Proposition 1.** *Exposure to violence has*

- *a positive effect on exit:* $\mathbb{P}(E|\kappa) - \mathbb{P}(E|0)$ *is strictly positive and increasing with the degree of exposure* $\kappa$*;*
- *an ambiguous effect on assimilation: the sign* $\mathbb{P}(A|\kappa) - \mathbb{P}(A|0)$ *is indeterminate.*

Exposure to violence has two effects: (i) it reduces the value of maintaining the status quo and (ii) it makes assimilation less valuable. The combination of these two effects implies that the proportion of foreigners who choose to exit rises as $\kappa$ increases. When it comes to assimilation, if the first effect dominates, then exposure to violence decreases the probability of assimilation. If the second effect is stronger, the reverse occurs. Which of these two possibilities materialize is an empirical question, not a theoretical one.

Proposition 1 provides a way to organize empirical findings. Scholars have found that exposure to violence tends to increase exit intentions (Steinhardt, 2018) or actual exits (Ferrara and Fishback, 2022), as we do in this paper. In turn, the evidence on assimilation is more mixed. Exposure to violence reduces intermarriage rates (Fouka, 2020; Gould and Klor, 2016), knowledge of the host country's language (Steinhardt (2018), though see Chen and Xie (2020)), support for the host country's political system (Fouka, 2020; Grewal and Hamid, 2022). In contrast, others scholars have shown that individuals exposed to violence tend to pick for their newborns names common in the host country (Chen and Xie, 2020; Fouka, 2019; Saavedra, 2021), adopt the cultural practice of their host community (Jaschke et al., 2022), and apply more for naturalization (Fouka, 2019).

Proposition 1 looks at exposure to violence in general, but using our theoretical framework, we can also study how exposure to violence interacts with prior integration. We, like other scholars before us, do not observe individuals' level of integration. Yet, researchers have taken advantages of some proxies of integration. For example, Steinhardt (2018) show how foreigners' response depends on their skill levels, Fouka (2019) exploit both community-level data (the share of churches catering to the foreigners, in her case, Germans) and individual-level data (married to an in-group member or a native). In what follows, we will use intermarriage, family status in the host country, and occupation.

In all cases, these heterogeneous tests can be understood as researchers having access to some correlates $I_i \in \{0, 1\}$ (e.g., mixed households–$I_i = 1$—or not—$I_i = 0$) of integration. We label individuals with $I_i = 1$ as grounded individuals and those with $I_i = 0$ as unattached individuals. To study it formally, we assume that the distributions of types conditional on the variable $I_i$ are such that $G(\cdot|I_i = 1)$ first-order stochastically dominates $G(\cdot|\tau_i = 0)$ (i.e., high level of integrations are more likely under $I_i = 1$ than under $I_i = 0$). We also amend the notation in Equations 4-6 to denote $\mathbb{P}(J|\kappa, I_i)$ the proportion of foreigners who choose option $J \in \{A, E, F\}$ conditional on exposure to violence $\kappa$ and variable $I_i \in \{0, 1\}$.

We state two results regarding the differential effect of exposure to violence depending on the correlates for integration. For ease of exposure, the propositions are stated in a slightly informal way. We refer readers to Online Appendix A.1 for a complete formal statement. Our first result determines sufficient conditions under which individuals with value $I_i = 0$ are more likely to exit following exposure to violence.

**Proposition 2.** *If (i) $P(F|\tau_i, \kappa) > P(E|\tau_i, \kappa)$, (ii) $P(A|\tau_i, \kappa)$ is sufficiently small, and (iii) $V^F_{\tau\kappa}(\tau_i, \kappa)$ is not too negative for all $\tau_i$, then grounded individuals exits less on average following exposure to violence*

$$\mathbb{P}(E|\kappa, I = 1) - \mathbb{P}(E|0, I = 1) < \mathbb{P}(E|\kappa, I = 0) - \mathbb{P}(E|0, I = 0)$$

Let us first explain why the results cannot hold without additional assumptions. As noted above, individuals with a low level of integration are more likely to exit: $P(E|\tau_i, \kappa)$ is strictly decreasing with $\tau_i$, yielding $\mathbb{P}(E|\kappa, I = 1) < \mathbb{P}(E|\kappa, I = 0)$. Yet, this holds for all degrees of

exposure to violence. Researchers are not interested in comparing exit probabilities, but in studying the consequences of exposure to violence (i.e., $\mathbb{P}(E|\kappa, I) - \mathbb{P}(E|0, I)$). Hence, we need conditions so that individuals with low levels of integration react more after observing violence. This is exactly what the conditions in the text of the proposition ensure. Since unattached individuals have lower levels of integration on average (as implied by our first-order stochastic dominance assumption), we observe more exit in this population than in the population of grounded individuals.

Our second result looks at heterogeneity when it comes to assimilation decisions. It uncovers sufficient conditions under which unattached individuals are less likely to assimilate.

**Proposition 3.** *If (i)* $P_\kappa(A|\tau_i, \kappa) > 0$ *and (ii)* $P(A|\tau_i, \kappa)$ *is sufficiently small for all* $\tau_i$*, then grounded individuals assimilate more on average following exposure to violence*

$$\mathbb{P}(A|\kappa, I = 1) - \mathbb{P}(A|0, I = 1) > \mathbb{P}(A|\kappa, I = 0) - \mathbb{P}(A|0, I = 0)$$

The problem is quite similar as above. We know that well-integrated individuals are more likely to assimilate for all degrees of exposure to violence: $P(A|\tau_i, \kappa)$ is strictly increasing with $\tau_i$. What we need is conditions so that they react more following exposure to violence. The conditions in the proposition guarantee this. Using the first-order stochastic dominance assumption again, we obtain that the proportion of grounded individuals that assimilate following exposure to violence is greater than the proportion of unattached individuals.

Our theoretical framework provides a way to think about both the exit and assimilation of foreigners as local circumstances change. It also indicates how we can think of heterogeneous effects of integration on these two outcomes of interest. In what follows, we use our theoretical findings to organize our empirical results. We start by presenting the context and events we exploit.

# 2 Background

In the 19th century, Europe was a continent of emigration, with a staggering 55 million Europeans emigrating between 1820 and 1920 (Rygiel, 2007; Thistlethwaite, 1960). Not all of

Europe, though. France had a different experience as a country of positive migration (Noiriel, 2010). It saw its number of foreigners growing from 400,000 when they were first counted in the 1851 census to over 1.1 million at the start of World War I, principally coming from Belgium and Italy (INSEE, 2010). The presence and growth of foreign nationals were not always well accepted, far from it. Around 1880, immigration became a political issue (Noiriel, 2007). In 1885, the French authorities carried out a vast study to "measure" the impact of the presence of foreigners on the French economy, and especially natives' employment. Partly to counter international socialist arguments and partly to boost national sentiments, the theme of the foreign threat within became a common topic in public debates. It led to several laws restricting the opportunities of foreigners in France (in 1889, 1892, 1893, 1894, 1895, 1898, 1907, see Noiriel, 2007, 196).

The main target of legislators were Italians, both due to their fast growth, causing a feeling of invasion, and to the lack of skills of the new arrivals (Milza, 1986). Despite the cultural proximity, at least from our contemporary perspective, the integration of Italians into French society proved difficult. The economic conjecture was unfavorable and Italy was at the time a hostile foreign power. Multiple dramatic episodes of violence against Italians in the South of France, such as the death of three Italians during the *Vêpres Marseillaises* in 1881 (Liens, 1967) or the massacre at the Aigues-Mortes in 1893, with at least eight Italians killed, offer vivid examples of the latent conflict between this group and the natives.

The events that interest us occur in this tense context. On June 24, 1894, French President Sadi Carnot went to Lyon, the second largest city in France, located 288 miles southeast of Paris in the Rhône department, to visit the *Exposition universelle, internationale et coloniale*, a world's fair including a colonial exhibition held at the Parc de la Tête d'Or. After patronizing the exhibition, Carnot participated in the banquet organized in his honor by the local chamber of commerce, place des Cordeliers, and was on his way to the theater when around 9 pm, an Italian anarchist named Sante Geronimo Caserio struck and stabbed the President Carnot to death. Sante Geronimo Caserio was not motivated by the xenophobic violence committed against Italians. He wished to avenge the execution of the French anarchist Auguste Vaillantin in February 1894 (Zancarini-Fournel, 2016). Carnot's assassination was only one of the many terror attacks by anarchists in the years 1892-94, most of them carried out by French citizens.

Yet, the actions of Sante Geronimo Casiero became the pretext for a vast movement of violence against Italians in the days following the death of President Carnot. Restaurants, shops, and houses were ransacked in Lyon. A newspaper analysis reveals that damages were especially high in the neighborhoods of La Guillotière, Les Brotteaux, Saint-Fons, and Vaise. Individuals were also targeted with rioters demanding anyone suspected of being Italian to prove French citizenship. The violence was not limited to Lyon, it spread to nearby Grenoble. It even spread to Paris. Yet, the number of deaths due to the riots in June 1894 remained relatively low, with three deaths in Lyon, two rioters and one policeman, none of them Italian quite surprisingly.

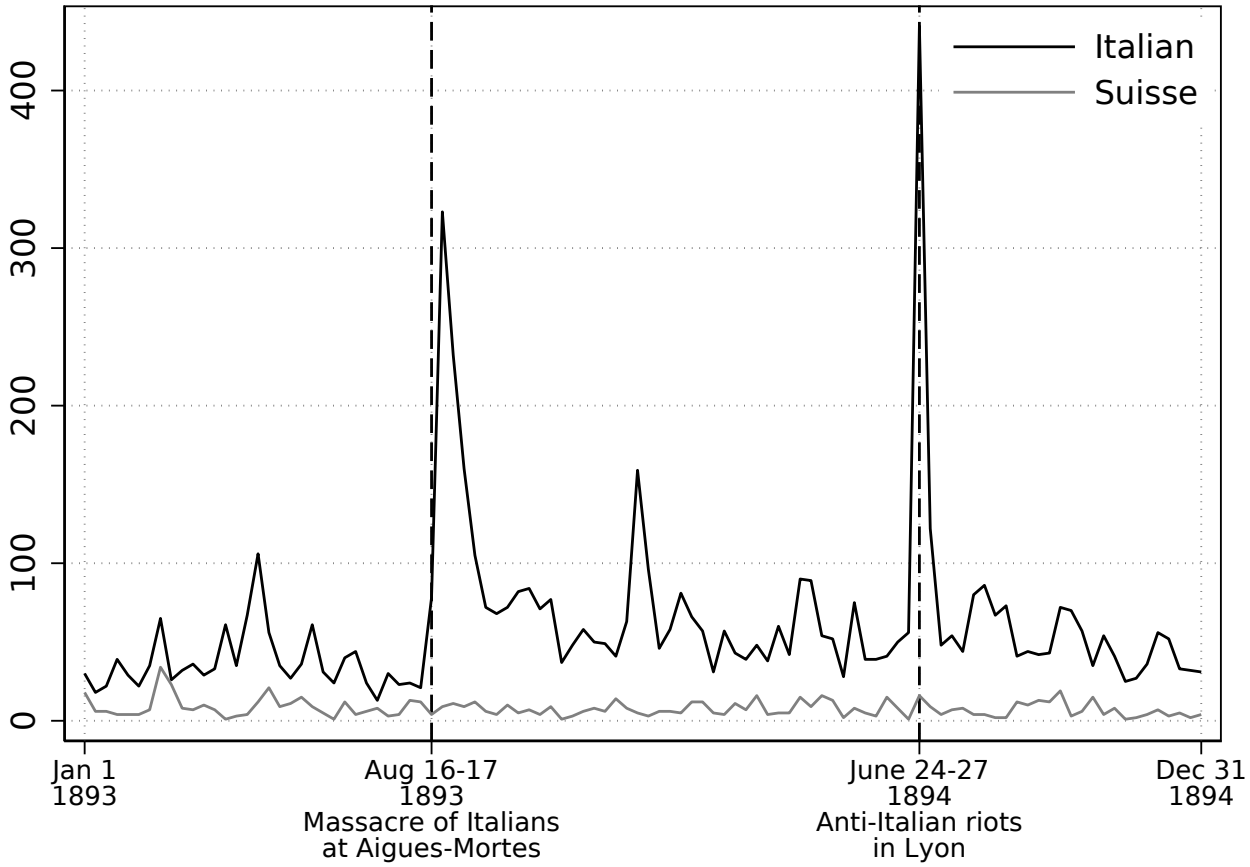
The riots were widely publicized in the local and national press. To document the coverage of the events, we collected the content of all regional newspaper articles published from January 1893 to December 1894 and digitized by the French National Library BnF (retronews.fr). We looked for the number of paragraphs each week mentioning Italians and acts of violence such as "rixe" (fight), "protestat." (protest), "tué" (murdered), etc.[3] We also performed the same search for Swiss who were the second largest group of foreigners in the Rhône department. The results of this data collection exercise are displayed in Figure 2.

Two main patterns are worth noting. First, we observe two peaks in Figure 2. The first in the second half of 1893 corresponds to the Aigues Mortes massacres already mentioned above; the second is for the riots in Lyon after the assassination of President Carnot. The two events receive more or less the same amount of attention, highlighting the importance of the riots we study. On top of it, we observe that the episodes of violence seem to have specifically targeted Italians. We do not see any variation in coverage of acts of violence against Swiss nationals, who constituted the second-largest group of foreigners in the Rhone department in 1894. We exploit this difference in exposure to violence in our empirical strategy described below.

We look at the reaction of Italians to this episode of targeted xenophobic violence across two dimensions: exit and assimilation. Historians have already documented that many Italian nationals left in the days after the violence (Dornel, 2004; Zancarini-Fournel, 2016) and our own archival search uncovered that 750 Italians were repatriated between June, 27th and June,

[3]The full set of keywords is: "rixe," "manifestat," "incendi," "protestat," "pourchasé," "exterminer," "armé," "armés de batons," "mis à sac," "mise au pillage," "bagarre," "à bas," "pillé," "saccagé," "saccage," "congédié," "incident," "licencié," "foule," "multitude," "pillé," "troubles," "tué," "démonstrations," "a mort," "querelle," "maltrait," "chasse à l'homme".

Figure 2: Weekly number of paragraphs mentioning violent keywords and nationality groups (1893 - 1894)




*Notes:* Weekly number of paragraphs mentioning Italian or Swiss with at least one violent keyword. The two vertical dashed lines indicate the timing of the Aigues-Mortes Massacre (August 1893) and the assassination of President Sadi Carnot (June 1894). Sample of 24,080 regional newspapers. Source: Retronews. List of violent keywords in Online Appendix B.

30th.[4] We are more interested in the long-lasting effect of the riots. On the one hand, some Italians may have returned. On the other end, others may have decided to leave later. They may have feared losing their jobs as native workers pressured their employers to fire Italians and it is not hard to imagine that these actions durably increased the latter's feeling of being unwelcome.[5] Observing violence may have changed the amount of trust that Italians had toward natives, and vice versa. They may also have feared future attacks.

When it comes to assimilation, we follow Fouka (2019) and look at petitions for naturalization,

[4]Archive Departmental (AD) du Rhone, 4M224

[5]AD Rhone 4M244. The archives contain letters sent to the prefecture by employers in La Prevotte, the Pyrite mines, the Saint Bel mines, Saint Gobain, and Patiaud Lagarde. Of course, many more (and maybe successful) pressures may have been left unreported.

which we take as the last step in the process of integration into the host society. To obtain French nationality at the time, other than through marriage or birth in France, a foreigner first had to petition for admission to legal residency, which, as far as we could tell, was granted without pre-requisite to foreigners who could demonstrate their intention to reside in France (according to the Article 13 of the Code Civil). Then after three years, a foreigner could petition for naturalization. Naturalization was granted or denied after a moral inquiry by the public administration (Article 1 of the law on naturalization from 3 December 1847). Naturalization came at a cost, the new national and their children were subject to up to three-year compulsory military service until they reach 30 years old. It also had benefits. Nationality was hereditary when legal residency was temporary (five years following the law on nationality of 26 June 1889), and foreigners were constrained to declare their residency within a week of moving to a new commune (following the decree on foreigners residing in France of 2 October 1888). Naturalization could potentially be socially beneficial as Italians may have felt they would be better protected against future acts of violence and discrimination by acquiring French nationality. We have already remarked that identity papers were asked of anyone suspected of being Italian by rioters.

# 3 Data

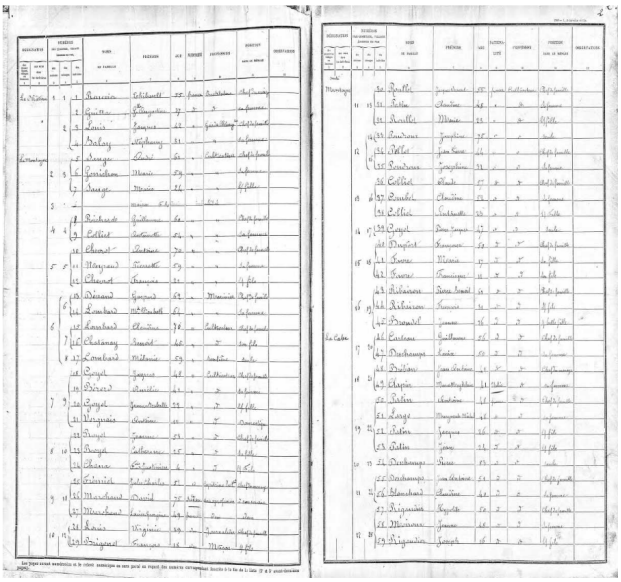
To investigate how exposure to violence (the events of 24-27 June 1894) shapes foreigners' choices, we make use of two sources of data: the French nominative census records and nominative naturalization decrees.

**Nominative census records.** We mostly use the French nominative census records from 1886, 1891 and 1896 for the Rhône department. These records list all inhabitants living in a given municipality, France's smallest administrative unit, every five years. In 1886, France counted 36,139 municipalities, with 266 located in the Rhône department (Gay, 2021). While the number of municipalities in the Rhône varies slightly over time—there are 268 municipalities in the Rhône department at the end of our period —, we use the 1886 municipality list for our analyses. We keep municipality boundaries fixed over time by merging the municipalities Saint Fons and Venissieux, which split in 1889, and the municipalities Le Perréon and Vaux-en-Beaujolais, which split in 1890.

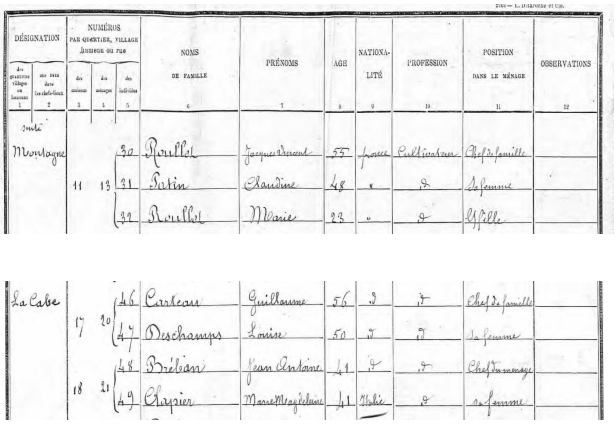
The nominative census contains the neighborhood of residence (and sometimes also the street), first and last names, nationality, age or year of birth, and occupation of all individuals living in the municipality. We can also identify members of the same household and how they relate to each other (head, spouse, child, household employees and other relatives). As an example of our raw data, Figure 3 displays a page from the 1886 nominative census of Albigny, one of the municipalities in our sample.

Figure 3: Example of a page of a nominative census record (Albigny, 1886)

(a) Full page



(b) Sample of rows

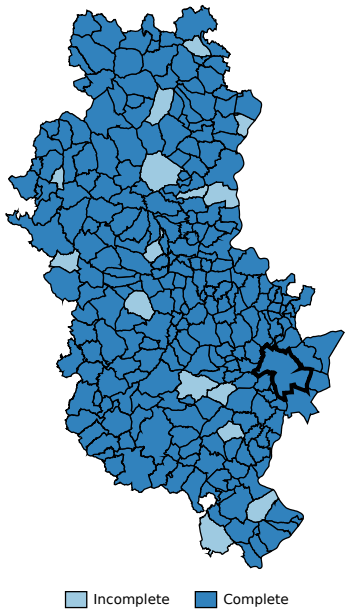


These hand-written records are available for all municipalities online on the websites of Rhône departmental archives. We hired a team of research assistants to extract indexed records of 39,742 pages amounting to 2,332,303 individuals. We perform two checks to ensure quality and correct mistakes. First, we compare the total number of individuals per municipality in our sample with the official municipality-level census counts published by INSEE for each of the census years in our sample.[6] This comparison reveals some discrepancies, most likely due to missing pages on the departmental archives website. While these pages are most likely missing at random, we restrict our sample to the municipalities for which we have a total of inhabitants equals to plus or minus 5% of the municipality-level official census counts. In practice, we drop 15 out of 798 (= 266 municipalities × 3 years) municipality-years from our sample, leaving us with 251 (out of 266) municipalities for which we have near complete individual-level data.

[6] Available at https://www.insee.fr/fr/statistiques/3698339.

In Figure 4, we display the coverage of municipalities in our sample using 1886 municipalities boundaries shapefile (obtained from Litvine et al., 2024). This map suggests that the 15 dropped municipalities are missing at random. Our sample comprises 2,277,497 individuals, French or foreign, in 251 municipalities over three census years, 1886, 1891 and 1896.

Figure 4: Coverage of municipalities in our sample




*Notes:* This Figure displays the 266 municipalities using the 1886 municipalities boundaries shapefile (Litvine et al., 2024). The city of Lyon is represented by a polygon with a thicker border. Municipalities are coded as complete if the total of inhabitants in our sample equals plus or minus 5% of the municipality-level official census counts, and as incomplete otherwise.

Second, we compare the total number of foreigners per municipality in our sample to the official municipality-level counts recorded on the last page of each municipality census record for the years 1886 and 1891 (we do not use information on the nationality of individuals listed in the 1896 census). In total for the 251 municipalities in our sample, we identified 32,985 of the 33,693 foreigners reported in official census counts in 1886 and 1891. The census enumerators recorded many non-French citizens as "foreigners" without specifying their nationality. This is the case for 2,650 foreigners out of the 32,985. Since we cannot tie these "foreigners" to a specific nationality to study their exit decisions and naturalization petitions, we exclude them from our analyses. Among the 30,335 foreigners with known nationality, 16,734 (55%) are Italian, 8,369 are Swiss (28%) and Germans make up for another 2,113 (7%). There are only 726 Belgians in our sample (2%), even though they represent 43% of foreigners nationwide in

1886.

In Table 1, we provide population summary statistics for 1886 and 1891. Excluding Lyon, municipalities counted on average 1,361 inhabitants, 7 Italians and 4 other known foreigners in 1886 (Panel B). In Lyon, there are roughly 400,000 inhabitants, close to 7,000 Italians and 5,550 other known foreigners in 1886 (see maximum values of Panel A).

Table 1: Municipality-level Summary Statistics

| | 1886 | | | | | 1891 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | S.d. | Min | Max | N | Mean | S.d. | Min | Max |
| **A. All municipalities** | | | | | | | | | | |
| Total population | 251 | 2,856 | 23,755 | 127 | 376,647 | 251 | 3,011 | 26,312 | 114 | 417,125 |
| Number of Italians | 251 | 34 | 434 | 0 | 6,868 | 251 | 33 | 424 | 0 | 6,707 |
| Number of other foreigners | 251 | 26 | 344 | 0 | 5,455 | 251 | 28 | 386 | 0 | 6,112 |
| Number of Swiss | 251 | 16 | 214 | 0 | 3,387 | 251 | 18 | 242 | 0 | 3,841 |
| Number of Belgians | 251 | 1 | 15 | 0 | 240 | 251 | 2 | 19 | 0 | 297 |
| Number of Germans | 251 | 2 | 25 | 0 | 391 | 251 | 2 | 24 | 0 | 384 |
| Number of 'étranger' (excluded) | 251 | 5 | 32 | 0 | 358 | 251 | 5 | 40 | 0 | 471 |
| **B. Excluding Lyon** | | | | | | | | | | |
| Total population | 250 | 1,361 | 1,791 | 127 | 14,051 | 250 | 1,354 | 1,901 | 114 | 17,063 |
| Number of Italians | 250 | 7 | 29 | 0 | 301 | 250 | 6 | 22 | 0 | 182 |
| Number of other foreigners | 250 | 4 | 17 | 0 | 172 | 250 | 4 | 14 | 0 | 144 |
| Number of Swiss | 250 | 2 | 10 | 0 | 122 | 250 | 2 | 9 | 0 | 105 |
| Number of Belgians | 250 | 0 | 2 | 0 | 19 | 250 | 0 | 3 | 0 | 38 |
| Number of Germans | 250 | 0 | 2 | 0 | 17 | 250 | 0 | 1 | 0 | 12 |
| Number of 'étranger' (excluded) | 250 | 4 | 24 | 0 | 345 | 250 | 5 | 38 | 0 | 471 |

On top of the three aforementioned censuses (1886 to 1896), we also make use of the 1881 census for which we collected the first and last names, year of birth, and municipality of residence of another 695,095 individuals. We do not use this census for our main analyses as it does not contain information about the nationality of individuals. We only reserve these data for the ancillary tests below.

**Nominative naturalization decrees.** Our second main data source are the official decisions regarding applications for naturalization, admission to legal residency, and reintégration into French citizenship (for those who had lost it previously, as was the case for women marrying a foreigner for instance). Until 1924, these decrees were published in the *Partie supplémentaire du Bulletin des lois de la République Française*. In 2017, decrees published between 1886 and 1898 were indexed through collaborative indexing organized by the French National Archives (project Natnum). Between 1886 and 1898, of the 94,749 published decrees, we uncover 1,805 decrees from applicants residing in the Rhône department. Among those, 888 individuals applied for naturalization, 367 applied for admission to legal residency, 435 for reintegration

into French nationality after marrying a foreigner, 111 for reintegration into French nationality of Alsatians, and 4 are missing information. In addition to the type of decrees and the day of the decision, each decree contains information regarding the applicant: first and last name, year and place of birth, occupation and place of residence. For decrees published after 1892, we can also observe whether the application was granted or denied. In the Rhône, we observe that 119 applications for naturalization were denied out of 646 (18%) decided between 1892 and 1898.[7]

Importantly, we can also recover the year an application was made using the application reference number (as described by Weil, 2002). Figures 5 and B.1 display one of these decrees, published on January 4, 1887, where the reference number indicates that the application was made in 1883 (recall that naturalization required an inquiry by public authority, hence the potentially long delay). Unfortunately, these data do not contain the month of an application. We collected this information separately for the years 1893 and 1894, around the Aigues-Mortes massacre and the assassination of President Carnot, by consulting naturalization books at the French National Archives (batch BB/29/829 to BB/29/837). An example of a page from the naturalization books is displayed in Figure B.2.

In our analyses, we focus on naturalization *applications*. That is, we do not look at whether a foreigner becomes French, but use the foreigner's intention to become French as this most closely matches our theoretical framework above (and avoid possible changes in behaviors from bureaucrats making decisions). We use these two sets of data—nominative censuses and naturalization applications—to understand how exposure to violence affects assimilation and exit.

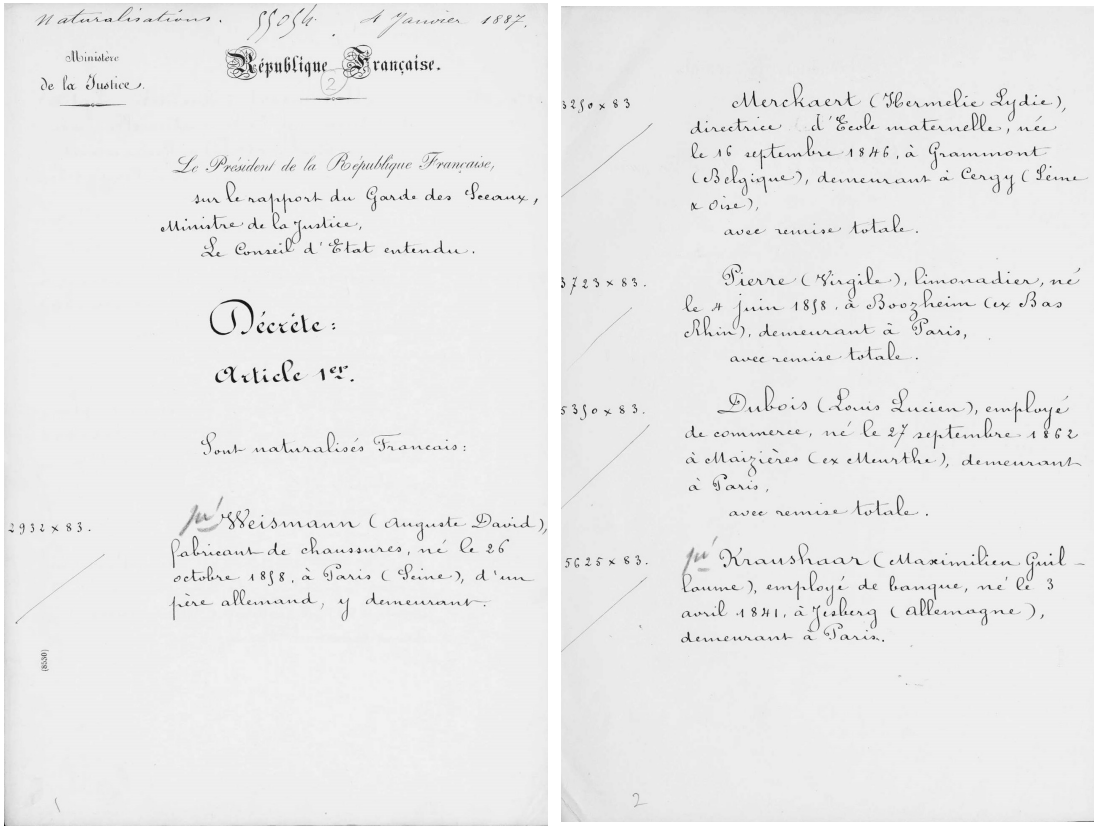
# 4 Main analysis

## 4.1 Exposure to violence and assimilation

We first look at the effect of exposure to violence on a commonly used measure of assimilation: the likelihood that a foreigner applies for naturalization. To do so, we link the 885 individuals who applied for French nationality to individuals in the 1886 and 1891 censuses (we drop 3 observations for which the year of birth was missing). In Supplemental Appendix B, we describe

[7] We also find that 12 applications for legal residence out of 110 (11%) were denied.

Figure 5: Example of naturalization decrees: original document (January 4, 1887)



Naturalisations. 55054. 4 Janvier 1887.

Ministère de la Justice.

République Française.

Le Président de la République Française,
sur le rapport du Garde des Sceaux, Ministre de la Justice,
Le Conseil d'État entendu,

Décrète :

Article 1er.

Sont naturalisés Français :

2932 × 83. Weismann (Auguste David), fabricant de chaussures, né le 26 octobre 1858, à Paris (Seine), d'un père allemand, y demeurant.

3250 × 83. Merckaert (Hermelie Lydie), directrice d'École maternelle, née le 16 septembre 1846, à Grammont (Belgique), demeurant à Cergy (Seine-et-Oise), avec remise totale.

3723 × 83. Pierre (Virgile), limonadier, né le 4 juin 1858, à Boozheim (ex Bas-Rhin), demeurant à Paris, avec remise totale.

5350 × 83. Dubois (Louis Lucien), employé de commerce, né le 27 septembre 1862 à Mairzières (ex Meurthe), demeurant à Paris, avec remise totale.

5625 × 83. Kraushaar (Maximilien Guillaume), employé de banque, né le 3 avril 1841, à Jeberg (Allemagne), demeurant à Paris.

our procedure at length. Here, we just summarize the basic idea behind our linking method. We match individuals based on (i) first and last names and (ii) year of birth using the `fastlink` algorithm developed by Enamorado et al. (2017) to facilitate probabilistic record linkages.

After keeping the best match (among those with a match probability greater than .85), we are left with 243 individuals from the 1886 census and 238 individuals from the 1891 census who matched one naturalization applicant.[8] Combining these two linkings, we match individuals from the census to 352 unique naturalization applicants (40% of the 885).

We then construct an indicator variable $Y_{ict}$ for all foreign individuals $i$ from municipality $c$ in the Rhône department and known nationality in the 1886 or 1891 census, $t \in \{1886, 1891\}$. This dummy variable takes value one if an individual from the 1886 (1891) census has applied for naturalization by the time of the next census in 1891 (1896); that is if individual $i$ has been linked according to the methodology above to a foreigner who applied for naturalization within

[8]We allow an applicant to naturalization to be matched to individuals in both the 1886 and 1891 census. This is to account for the case of individuals who are present in the census of both years and apply for French citizenship after the 1891 census.

5 years. Table 2 displays the summary for our main dependent variable $Y_{ict}$. We can see that .7% of foreigners present in the 1886 census in the Rhône department naturalized by 1891. The proportion rises to 1.4% in the next period (foreigners from the 1891 census naturalizing by 1896), mostly due to Italians naturalizing more after 1891 with other foreigners not displaying such a sharp increase.

Table 2: Summary statistics for naturalization

| | 1886 | | | 1891 | | |
|---|---|---|---|---|---|---|
| | N | Mean | S.d. | N | Mean | S.d. |
| **Applied for naturalisation by t+5** | | | | | | |
| Among all foreigners | 15,028 | 0.005 | 0.070 | 15,315 | 0.009 | 0.093 |
| Among Italians | 8,521 | 0.007 | 0.082 | 8,224 | 0.014 | 0.117 |
| Among other foreigners | 6,507 | 0.002 | 0.048 | 7,091 | 0.003 | 0.052 |
| Among Swiss | 3,963 | 0.002 | 0.039 | 4,412 | 0.002 | 0.043 |

We can make two observations based on these descriptive statistics. First, the likelihood that a foreigner applies for naturalization is very small (the raw proportions, assuming that all 888 applicants listed as coming from the department of the Rhône actually lived there are 1.7% for 1886-1890 and 2.7% for 1891-1895). Yet, these figures are not outliers. In aggregate, our small odds of a single individual applying corresponds to 39 applications by year for Italians and 21 by others over the period 1886-1898, not so different than the 56 applications by year by nationality group that Fouka (2019) documents in the United States in the 1910s. Even today, the rate of naturalization is relatively low, around 3% per year in the United Kingdom and in the United States.[9] The second important observation is that these descriptive statistics provide first-glance evidence of the impact of exposure to violence.

To more rigorously estimate the impact of xenophobic riots on Italians' assimilation decisions, we employ the following specification for all foreigners of known nationality found in the censuses.

$$Y_{ict} = \beta_1 1891\ \text{Census}_{ict} + \beta_2 \text{Italian}_i + \tau \text{Italian}_i \times 1891\ \text{Census}_{ict} + \lambda_c \text{Municipality}_c + \epsilon_{ict} \quad (7)$$

[9]For the UK, around 200,000 foreigners apply for naturalization according to the UK government relative to 4.9 millions non-UK passport holders living in the UK according to the office of national statistics. In the United States, around 800,000 individuals apply for naturalization according to Homeland Security compared to a foreign population of 25 million according to estimates from the Pew Research Center.

The explanatory factors we include on the left-hand side of Equation 7 are: municipality fixed effects—denoted $Municipality_c$—, a dummy variable equal to one if individual $i$ is present in the 1891 census—denoted 1891 $Census_{ict}$—, an indicator for whether individual $i$ is marked as Italian in one of the censuses— $Italian_i$ as well as the interaction of 1891 $Census_{ict}$ and $Italian_i$. $\epsilon_{ict}$ corresponds to the error term and we run all our regressions with robust standard errors clustered at the municipality level.

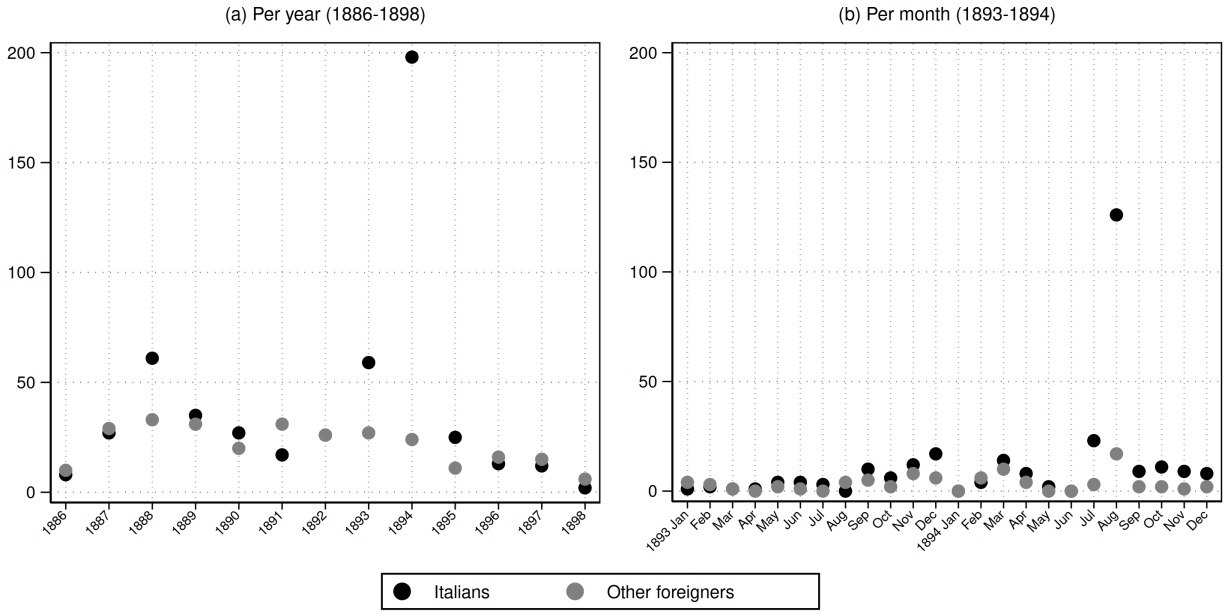
As such, our approach is akin to a difference-in-differences strategy with $\tau$ our main coefficient of interest. This coefficient captures within-municipality change in assimilation between 1886-1891 (pre-violence) and 1891-1896 (post-violence) for Italians net of the trend for other foreigners. Our approach combines the strengths of Fouka's (2019) and Ferrara and Fishback's (2022) research designs. Like Fouka (2019), we use foreigners from other nationality as our control group (Ferrara and Fishback, 2022, design is more akin to difference-in-differences design with intensity of treatment where the intensity is the possible exposure to violence). Like Ferrara and Fishback (2022), we look at individual level data, though they focus exclusively on naturalization by those who change residences following violence against their group (in Fouka, 2019, the unit of observation is at the ethnic group-year level). As such, Ferrara and Fishback (2022) studies naturalization conditional on moving (and being matched across censuses) rather than unconditionally as we do here.

Like all difference-in-differences research designs, Equation 7 recovers the causal effect of violence on the treated (Italian nationals). Nonetheless, as we discussed in our Background section, tensions between natives and foreigners were high throughout the period. Further, this particular episode of violence was triggered by an exogenous event, the assassination of President Carnot. Hence, we have good reasons to expect that our research design uncovers the average treatment effect of violence on foreigners' assimilation choices.

Our estimates are also causally identified if and only if three conditions are met. First, the parallel trends assumption should hold: absent exposure to violence, the probability that Italians would have applied for naturalization would have increased at the same rate as that of other foreigners. While this assumption cannot be tested, we follow best practices and display in Figure 6 the number of naturalization applications by Italians and foreigners by year (Panel a) and by month (Panel b). Looking at Figure 6(a), we observe no difference prior to 1894

between Italians and other foreigners except for the year 1893. When we focus on the months prior to the events of June 1894 (Figure 6(b)), we see that the 1893 difference in yearly figures is driven by small difference in the number of applications in December 1893. Italians and other foreigners appear to be applying for naturalization at the exact same rate for the six months prior to the assassination of President Carnot.

Figure 6: Number of naturalization applications in the Rhône department




The second assumption for causal identification is that the control group is not affected by the treatment (no interference). While non-testable again, we have seen above that the violence does not seem to have targeted Swiss nationals, the second largest foreign group in the Rhône department (see Figure 2). Figure 6 also indicates little changes in the naturalization pattern of other foreigners after June 1894 (though we observe a slight increase in naturalization applications by other foreigners in August 1894). Notice that if other foreigners were affected by the exposure to violence, to a lower extent than Italians, this would tend to bias downward our estimates.

The third assumption is that the reaction of Italians is not due to other concomitant events. One concern in particular is the massacre in Aigues-Mortes on 16 and 17 August 1893 in the department of the Gard, south of the Rhône department. Looking at Figure 6(b), we observe that (i) there is little differential change in naturalization in the few months following Aigues-Mortes and (ii) the peak in applications for Italians clearly happens post-treatment in July and

August 1894, which is reassuring. Another concern is a change in laws in 1889 that required all foreigners residing in France to renew their status every 5 years or naturalize. This policy change, however, affects *all* foreign nationals. Hence, it is taken into account by our census dummy.[10]

The results of our analyses are displayed in Table 3, where we decompose our difference-in-differences estimate. In the first column, we restrict the sample to Italians and look at their differential naturalization rates over the period 1886-1891 and the period 1891-1896. We see a clear increase in assimilation significant at the .01 level. In the second column, we perform the same analysis with all other foreigners and fail to detect an increase. The third column looks at all foreigners together and displays the result of our difference-in-differences regression (Equation 7). We observe that relative to other foreigners, Italians are 0.6 pp more likely to apply for French nationality in the period when the riots occur. In columns (4) and (5), we reproduce our analysis by looking at Swiss nationals, the second largest foreign group in the Rhône. The estimates are unchanged.

Table 3: Applied for naturalization: Main result

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | 0.007 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) |
| Italian | | | 0.005 | | 0.006 |
| | | | (0.001) | | (0.001) |
| 1891 × Italian | | | 0.006 | | 0.007 |
| | | | (0.001) | | (0.001) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table 3 indicates that an increase in exposure to violence is associated with an increase in assimilation. The effects appear to be small (around 0.7 pp). They are, however, quite large relative to the pre-treatment mean and standard deviation, see Table 2. Only using the treated

[10]The law from 1889 and its implication for residence permit in 1894 probably explain the small increase in naturalization we observe at the end of 1893.

(Italians), these estimates correspond to a 100% increase relative to the mean and 8.5% of a standard deviation As a point of comparison, Fouka (2019) documents an increase of 70% in application for naturalization relative to the mean for Germans in the United States following the US entry into World War 1, a much larger, but also more diffuse shock than the one we study. As such, observing a spike in violence had a non-negligible effect on Italians' assimilation choices.

## 4.2 Exposure to violence and exit

We now turn to exit, our rather our proxy for it. As before, we use Enamorado et al. (2017)'s `fastlink` algorithm to link individuals across censuses using first and last names and year of birth. We perform two separate matching procedures. In the first, we link individuals all over the Rhône department. In the second, we treat the municipality of residence as a blocking variable. We keep all matches with a match probability over .85 and when multiple matches are available for one individual, we keep the match with the highest match probability. For all foreigners $i$ residing in a municipality $c$ in census $t \in \{1886, 1891\}$, we generate a dummy $Y_{ict}$ equal to one if the individual is **not** found in the department or in the municipality at the time of the next census at $t + 5$. We associate the first outcome to having left the department, the second to having left the commune.

Table 4 presents summary statistics for our outcome variables. Our estimated exit rate is relatively high: we estimate that around 69% of foreigners in the 1886 census have left the municipality of residence by 1891 and that around 62% of foreigners left the department over the same period. Yet, it is on par with link rates in previous works. Abramitzky et al. (2021) compare the performance of various algorithms in linking the Union Army records to the census of 1900 in the USA and find link rates between 15% and 65% (an exit rate between 85% and 35%). For the particular technique we use, Enamorado et al. (2019) report a linking rate of above 90% when merging the 2015 and 2016 nationwide voter files in the USA.[11] On top of this, we confirm that in the pre-treatment period, the exit rate of individuals who declared their occupation as 'travellers' is substantially higher than for those who listed other occupations (Table B.1). We find that the exit rate out of the municipality was 8 percentage points higher for

[11] This linking rate for one year difference leads to a linking rate of 59% across 5 years, so an exit rate of 41%. Yet, the raw data Enamorado et al. (2019) use is digital, whereas we use hand written data that are indexed. Hence, we believe our linking rate is again within the right bounds.

travellers among French and 18 to 30 percent higher for Italians and Swiss, which is reassuring as this category tends to be more mobile.

Table 4: Summary Statistics on exit

| | 1886 | | | 1891 | | |
|---|---|---|---|---|---|---|
| | N | Mean | S.d. | N | Mean | S.d. |
| **Left the department by t+5** | | | | | | |
| Among all foreigners | 15,028 | 0.619 | 0.486 | 15,315 | 0.613 | 0.487 |
| Among Italians | 8,521 | 0.608 | 0.488 | 8,224 | 0.605 | 0.489 |
| Among other foreigners | 6,507 | 0.633 | 0.482 | 7,091 | 0.622 | 0.485 |
| Among Swiss | 3,963 | 0.597 | 0.491 | 4,412 | 0.583 | 0.493 |
| **Left the commune by t+5** | | | | | | |
| Among a sample of French | 197,592 | 0.545 | 0.498 | 195,353 | 0.540 | 0.498 |
| Among all foreigners | 15,028 | 0.686 | 0.464 | 15,315 | 0.678 | 0.467 |
| Among Italians | 8,521 | 0.681 | 0.466 | 8,224 | 0.679 | 0.467 |
| Among other foreigners | 6,507 | 0.693 | 0.461 | 7,091 | 0.676 | 0.468 |
| Among Swiss | 3,963 | 0.659 | 0.474 | 4,412 | 0.642 | 0.479 |

All the facts mentioned are reassuring and indicate that our dependent variables are likely to approximate our outcome of interest: exit. Yet, we recognize that our approach is not unproblematic. We only study local exits, not return to the home country. As nominative censuses are unavailable for the whole France, we can never be fully sure an individual has left the country. There are also many reasons why we still obtain a high exit rate. Names may be poorly transcribed by census enumerators (Abramitzky et al., 2021, suggest that this can account for 45% of link failures). Individuals may fail to be matched, and thus counted as exiting, because they die, change names, or marry (for women). All those are important problems, but they would tend to bias our estimates only if they affect Italians more than other nationalities. For now, we mention these issues, below, we describe a host of robustness tests we perform to alleviate these concerns.

We run the same specification as Equation 7. The assumptions for identification are, therefore, the same as before: parallel trends, non-interference, and no other treatment. Absent yearly (or even monthly) data and lacking information on the nationality of individuals in the 1881 census, we cannot apply best practices to increase confidence that these three assumptions hold. Yet, it is not immediately obvious why exit should be significantly different than assimilation. Further, we can perform a related analysis to test for the presence of pre-trend. We use the association between last name and nationality in the 1886 census to predict nationality based

on last names in the 1881, 1886 and 1891 censuses (as used by Fouka, 2019, 2020; Fryer Jr and Levitt, 2004). We then perform our difference-in-differences in strategies on this sample with predicted nationalities. Tables C.1 displayed in Online Appendix C (where we also provide more details on our approach) suggest that pre-trends are unlikely to be different between the treated and control groups.

Results for exiting the municipality are displayed in Table 5, where we repeat the same approach as in Table 3. We find that Italians were not more likely to leave their municipality of residence between 1891 and 1896 relative to the 1886-1891 period. In contrast to Italians, we find that the exit rate of other foreigners decreased slightly between the 1891 and the 1896 census. As a result, we find that the anti-Italian violence caused a 1.4 percentage point increase in the emigration rate of Italians relative to other foreign groups. The effect is similar in magnitude when compared to Swiss only, but not significant any more. Relative to the mean and standard deviation of the exit rate for Italians between 1886 and 1891 (respectively, 0.681 and 0.466 in Table 4), our results point to a statistically significant, yet relatively small impact of anti-Italian violence: an increase of 2% relative to the mean, corresponding to 4.4% of a standard deviation.

Table 5: Left the municipality: Main result

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | 0.003 | -0.010 | -0.010 | -0.008 | -0.011 |
| | (0.004) | (0.004) | (0.005) | (0.006) | (0.008) |
| Italian | | | -0.017 | | 0.015 |
| | | | (0.006) | | (0.011) |
| 1891 × Italian | | | 0.014 | | 0.015 |
| | | | (0.006) | | (0.009) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

In Tables C.2 and C.3, we decompose our estimate for whether the individual left the municipality into two components: whether the individual left the department and whether they moved to another municipality within the department. The results, though not statistically

significant, suggest that both sorts of moves played a role in Italian nationals following the xenophobic riots of June 1894.

## 4.3 Robustness

In the previous two subsections, we have shown that exogenous exposure to violence can lead to more assimilation and more exit. In this subsection, we build confidence in these findings by briefly describing a series of robustness tests we run (all results are collected in Supplemental Appendix D).

One concern is that our tests only measure changes over a relatively long period (five years) that is not exactly centered around our treatment, the sudden spike of violence against Italians in June 1894. As noted above, the time between censuses does not permit to do much more for exit. We have, however, yearly data for naturalization. We, thus, rerun Equation 7 at the individual-commune-year level (with year fixed effects instead of census fixed effects). Results displayed in Figure D.1 closely match the descriptive patterns in Figure 6.

We have also discussed several potential issues associated with linking names across censuses. To ensure that our results are not driven by our linking method, we rerun our results for both assimilation and exit (i) keeping all matches above .85 match probability and weighing outcomes by the match probability (as recommended by Enamorado et al., 2017), (ii) splitting the sample between men and women (as in Abramitzky et al., 2014; Ferrara and Fishback, 2022) to rule out that women who marry are driving our estimates (given that we only have gender for a subsample, we also show the results for individuals with known gender), (iii) keeping only individuals below 50 to reduce the risk that exit of an individual is due to death, and (iv) leaving out individuals with middle names who increase the risk of failed link. Our main results remain substantively unchanged across these six different alternative linkage strategies, see Table D.1.

We were also concerned that Italians may have changed their last names in greater numbers following the events of June 1894 and this may explain their differential exit rate. We collected all 643 name changes between 1884 and 1898 from the *Dictionaire des changements de nom* (Archiviste Jérôme, 1964). We estimate Equation 7 using as dependent variable a binary indicator for whether an individual changed their last name (coding individuals in the Census

has having changed their names if their last name matches at least one last name in the sample of name changes). Results reported in Table D.2 suggest changes of last names are unlikely to drive our exit result.

We also consider alternative measure of assimilation. First, we replace our naturalization measure by a close equivalent: whether a foreigner in a census at date $t$ is linked to a French national in a census at date $t+5$ ($t \in \{1886, 1891\}$). Our dependent variable is now an indicator equal to one if an individual present at time $t$ is (i) found in the municipality at $t+5$ and (ii) the individual he is matched with is French in $t+5$. We show both conditional estimates (coding as missing individuals who were not found) in Table D.3 and unconditional estimates (coding as 0 individuals who were not found) in Table D.4. Both results closely match our main findings in Table 3. We also look at another commonly used outcome: partnership with a French national. This is not our preferred outcome since we cannot compute pre-trends and it is unclear how to treat individuals we fail to match across censuses. As we show in Tables D.5 and D.6, results are sensitive to the way we code unmatched individuals. Conditional on remaining in the municipality, exposure to violence increases cross-national partnerships. Unconditionally, the exposure to violence decreases inter-partnership for single individuals. This last result is in line with the literature, which has generally found a negative effect of exposure to violence on such assimilation measure (Fouka, 2020; Gould and Klor, 2016). It also highlights the difficulty of making predictions on exposure to violence and assimilation as per our theoretical framework (recall Proposition 1).

One additional concern is that our estimates are not driven by the reactions of Italians to the exposure to violence, but rather by French authorities targeting anarchists who happen to be Italians. Indeed, as we described in our Background section, Sante Caserio was an anarchist first, Italian second. We take advantage of a list compiled by the Rhône prefecture of all anarchists, French or foreign present in the Rhone department in 1892 (see Figure B.3).[12]. In Table D.7, we split the sample by whether individuals in the Census had a last name matching the last name of an anarchist on the list. As Table D.7 indicates, Italian anarchists are more likely to exit the commune than non-anarchist, though the difference is not statistically significant and the coefficient on non-anarchists matches our main effect (Table 5). When it comes to naturalization, all results are driven by non-anarchists.

[12] AD Rhone 4M311

# 5 Heterogeneity

We build on our theoretical setup to perform two sets of heterogeneous tests. First, we look at the degree of exposure to violence separating between the locations where riots are known to have occurred and those where we have no evidence of such direct exposure. The basic idea is that direct exposure increases a sense of threat.[13] Based on Proposition 1, we expect greater exit rate in places with known exposure to violence. Note that we have no such theoretical expectation for assimilation. We then study assimilation and exit for grounded and unattached individuals using the language from our formal framework. Here, our theoretical results depended on a few conditions: greater likelihood to maintain the status quo than exit, low probability of assimilation, assimilation increasing with violence. While those sufficient conditions were stated at the individual level, our aggregate findings can be informative about their validity. Despite the noise associated with linkage, we observe an exit rate of "only" 68% (Table 4). Hence, it is not impossible to believe that exit is less likely than maintaining the status quo.[14] We also found a small percentage of foreigners naturalizing, our measure of assimilation (Table 2). Finally, Table 3 suggests that assimilation increases with violence. All these lead us to expect higher assimilation following exposure to violence for grounded individuals and higher exit for unattached individuals.

## 5.1 Exit and assimilation by threat of violence

As noted in our Background section, violence between French and Italians took place, namely la Guillotière, Vaise and Saint Fons. We identify these municipalities as having higher exposure to violence. We then rerun our difference-in-differences estimation (Equation 7) separately for the municipalities/neighborhoods with high exposure and municipalities/neighborhoods with low exposure.[15] Results for assimilation are displayed in Table 6 and findings for exit can be found in Table 7.

[13] While we cannot test this assumption in our setting, such a relationship has been shown in other contexts. For example, following a terrorist attack, affected citizens fear more the threat of future attacks (see, for example, Epifanio et al., 2023).

[14] Using a specific dataset (Enquete des 3000 familles), Daudin et al. (2016) estimate that only 17.3% of individuals left their department in a 50-year period (1861-1911). This also indicates that exit is less likely than maintaining the status quo.

[15] Since we identify places with high exposure based on mention in newspapers, we cannot exclude the possibility that other communes experienced some relatively high violence. It was not high enough, however, to be reported at the time.

Table 6: Applied for French citizenship: Heterogeneity by exposure to violence

| | High exposure | | | | Low exposure | |
|---|---|---|---|---|---|---|
| | All | Vaise | La Guillotière | Saint Fons | All | All |
| 1891 | 0.003 | 0.004 | -0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.004) | (0.000) | (.) | (0.000) | (0.000) |
| Italian | 0.009 | 0.007 | 0.013 | 0.007 | 0.004 | 0.004 |
| | (0.000) | (0.005) | (0.009) | (0.005) | (0.001) | (0.001) |
| 1891 × Italian | 0.010 | 0.023 | -0.013 | -0.007 | 0.006 | 0.006 |
| | (0.000) | (0.014) | (0.009) | (0.005) | (0.001) | (0.001) |
| High exposure | | | | | | -0.002 |
| | | | | | | (0.000) |
| 1891 × High exposure | | | | | | 0.002 |
| | | | | | | (0.000) |
| Italian × High exposure | | | | | | 0.005 |
| | | | | | | (0.000) |
| 1891 × Italian × High exposure | | | | | | 0.003 |
| | | | | | | (0.001) |
| Observations | 1,797 | 1,004 | 495 | 298 | 28,514 | 30,311 |
| # of municipalities | 4 | . | . | . | 132 | 132 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table 7: Left the municipality: Heterogeneity by exposure to violence

| | High exposure | | | | Low exposure | |
|---|---|---|---|---|---|---|
| | All | Vaise | La Guillotière | Saint Fons | All | All |
| 1891 | -0.116 | -0.297 | -0.033 | 0.095 | -0.005 | -0.005 |
| | (0.004) | (0.063) | (0.042) | (0.064) | (0.006) | (0.006) |
| Italian | -0.115 | -0.046 | -0.160 | -0.161 | -0.010 | -0.010 |
| | (0.007) | (0.055) | (0.043) | (0.070) | (0.006) | (0.006) |
| 1891 × Italian | 0.114 | 0.163 | 0.103 | 0.161 | 0.008 | 0.008 |
| | (0.007) | (0.084) | (0.062) | (0.070) | (0.006) | (0.006) |
| High exposure | | | | | | 0.033 |
| | | | | | | (0.004) |
| 1891 × High exposure | | | | | | -0.107 |
| | | | | | | (0.008) |
| Italian × High exposure | | | | | | -0.103 |
| | | | | | | (0.006) |
| 1891 × Italian × High exposure | | | | | | 0.102 |
| | | | | | | (0.007) |
| Observations | 1,797 | 495 | 1,004 | 298 | 28,514 | 30,311 |
| # of municipalities | 4 | . | . | . | 132 | 132 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table 6 indicates that assimilation slightly increases with the degree of exposure to violence, though the relationship is noisy when we look at the municipalities/neighborhoods one by one. In contrast, we uncover a clear larger effect for exit in places with high exposure to violence than with low exposure to violence. When exposure is high, we document an increase in exit rate by around 16.5% relative to the mean and around 24% of a standard deviation (see Table 4). Those estimates match the finding in Ferrara and Fishback (2022) who document that Germans living in the most hostile counties (as measured by the number of causalities in World War 1) were around 11% (around 19%) more likely to leave than Germans living in counties with median (lowest) level of hostility.[16]

## 5.2 Exit and assimilation by level of integration

We look at three proxies for the level of integration. First, we use the nationality of spouse to distinguish between household heads in a partnership with a French citizen (grounded individuals) from household heads in a partnership with a foreigner (unattached individuals). Second, we use the position in the household to distinguish between individuals who are head of household or spouse or children (grounded) from individuals who are household employees, like servants, workers (unattached). Finally, we use occupation to compare shop owners (grounded because they likely to have invested capital into the host country) and workers (unattached because they are likely to only receive salaried income). We provide more details on the construction of these variables in Online Appendix F where we also display summary statistics (Table F.2).

Tables 8 and 9 look at the effect of exposure to violence on assimilation and exit, respectively, for grounded and unattached individuals separately (see Tables F.3-F.8 in Online Appendix F for additional results including regressions with triple interactions). Results are consistent with our theoretical expectations. However we measure the roots of an individual (mixed marriage, position in household, occupation), we observe that grounded individuals are more likely to assimilate and less likely to exit following exposure to violence. The difference between coefficients is usually statistically significant as we show in Online Appendix F, except for the

[16]For completeness, in Online Appendix E, we also look at partnerships with French nationals by exposure to violence unconditionally (treating as zero individuals who are not matched). Table E.1 suggests that higher exposure reduces inter-partnership rates, though the effect could be driven in part by individuals who leave their municipality.

Table 8: Applied for naturalization: heterogeneity by baseline integration levels

| | Mixed households | | Position in Household | | Occupation | |
|---|---|---|---|---|---|---|
| | In partnership with foreigner | In partnership with French | Household employee | Head of household | Works as a worker | Works as a shop owner |
| 1891 | 0.005 | 0.002 | -0.000 | 0.002 | 0.002 | -0.002 |
| | (0.001) | (0.003) | (0.000) | (0.000) | (0.001) | (0.002) |
| Italian | 0.010 | 0.018 | 0.008 | 0.006 | 0.007 | 0.014 |
| | (0.001) | (0.005) | (0.001) | (0.001) | (0.001) | (0.003) |
| 1891 × Italian | 0.007 | 0.026 | 0.003 | 0.006 | 0.008 | 0.026 |
| | (0.002) | (0.006) | (0.001) | (0.001) | (0.001) | (0.006) |
| Observations | 6,661 | 2,000 | 5,115 | 21,009 | 10,269 | 817 |
| # of municipalities | 63 | 51 | 85 | 97 | 82 | 19 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

comparison of family and household employees.

Table 9: Left the municipality: heterogeneity by baseline integration levels

| | Mixed households | | Position in Household | | Occupation | |
|---|---|---|---|---|---|---|
| | In partnership with foreigner | In partnership with French | Household employee | Head of household | Works as a worker | Works as a shop owner |
| 1891 | -0.030 | 0.035 | -0.026 | -0.006 | -0.022 | 0.061 |
| | (0.005) | (0.020) | (0.013) | (0.007) | (0.016) | (0.009) |
| Italian | -0.009 | 0.026 | -0.061 | 0.006 | -0.011 | 0.017 |
| | (0.008) | (0.016) | (0.006) | (0.005) | (0.016) | (0.019) |
| 1891 × Italian | 0.047 | -0.028 | 0.029 | 0.017 | 0.040 | -0.027 |
| | (0.010) | (0.027) | (0.010) | (0.007) | (0.017) | (0.016) |
| Observations | 6,661 | 2,000 | 7,087 | 30,203 | 11,294 | 859 |
| # of municipalities | 63 | 51 | 102 | 113 | 99 | 23 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Motivated by previous works (e.g., McKenzie and Rapoport, 2010), we also look at exit and assimilation patterns for long-term residents of a commune, which are likely to be grounded, and recent arrivals, which are more likely to still be unattached.[17] We define long-term residents in census $t$ as an individual we can link to census $t-5$ ($t \in \{1886, 1891\}$. A recent arrival

[17]In itself, it is not obvious that long-term foreigners have higher levels of integration on average. After all, these are individuals who have not yet naturalized despite being in the country for long. In the French context of the time, however, the benefits of naturalization were low, whereas the cost of naturalization (military service for oneself or one's children) were high prior to the reform of 1889. Very few foreigners chose to naturalize then, which was one of the reasons for the reform. Hence, in our context, long-term residency is a good proxy for attachment to the host country, France.

is an individual we can only find in a census at time $t$. Results displayed in Table F.9 show little difference when it comes to exit, but more assimilation by long-term residents following exposure to violence.

Finally, we also look at foreigners' decisions based on their pre-treatment economic situation. While we do not have exact measure of how rich an individual is, we have two good proxies: whether an individual employs a domestic in their household (a sign of sufficiently high income) and whether an individual employs workers (a sign of economic success). Foreigners with a good economic situation are not necessarily better integrated in their host society (e.g., Aleksynska and Algan, 2010; Dustmann, 1996). Hence, we treat this heterogeneous test as separate from those we carried above. In Table F.10, we document that compared to their less successful peers, prosperous individuals are slightly less likely to apply for citizenship (with the difference not statistically significant) and much more likely to exit. Hence, this test suggests that some foreigners may be constrained to stay in their municipality due to the cost of moving. It is also in line with the evidence in Ferrara and Fishback (2022) who find that counties who lost a large proportion of Germans during and after World War 1 also suffered the most economically.

# 6 Discussion and conclusion

How do foreigners react when exposed to xenophobic violence? Do they exit? Do they assimilate? To answer these questions, we exploit unexpected anti-Italian riots around Lyon triggered by the assassination of French President Carnot in June 1894 and nominative censuses that allow us to track individuals over time. We uncover that exposure to violence triggers a rise in *both* exit and assimilation. Our unique dataset, tracking foreigners over time, also allows us to establish the fine-grained reactions of foreigners witnessing violence. We document that greater exposure, as proxied by the known locations of riots, yields greater assimilation and exit. We also find that a higher level of integration pre-violence, as approximated by mixed partnership, family living together, or occupation, consistently leads to higher assimilation and less exit post-violence.

While our paper looks into the past, we believe that our empirical results coupled with our theoretical framework can prove useful for contemporary immigration policies, a highly controversial topic these days. Those policies can be divided into camps. Some reforms are meant

to reduce the payoff from assimilation or to increase the cost of staying in the host country, like a hostile environment or a ban on certain cultural practices such as wearing a veil (e.g. Abdelgadir and Fouka, 2020; Bowen, 2007). As our paper indicates, individuals with the lowest level of integration will tend to exit more, whereas well-integrated foreigners will likely assimilate more. Hence, such policies favour assimilation of some foreigners at great cost for others. Other measures have attempted to increase the benefits of assimilation. This includes, among others, language training and compulsory civic courses (Emeriau et al., 2022), facilitating the accession to the host society's nationality (Dahl et al., 2022) or indirectly the abolition of military service for nationals (Govind and Sirugue, 2023). Such policies, building on our theoretical findings, are likely to favour well-integrated foreigners over those with lower level of integration.[18] Hence, if policy-makers want to target immigrants with intermediary level of integration, reforms to facilitate assimilation may well fail to fully fulfill the government's objective. More generally, scholars and policy-makers should be careful in evaluating any immigration reforms. The same policy change may have distinct impacts depending on the outcome used for evaluation. Further, even when studying a single outcome, some types of immigrants and foreigners may react very differently than others. To return to the questions that motivate our paper, the correct interrogations are not whether foreigners get out, whether they buy in, but rather which foreigners get out, which of them buy in.

[18]By applying a similar reasoning as for Proposition 3, the increase in assimilation increases with the level of integration whenever $P(A)$ is not too large.

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# Supplementary Information For Online Publication

# A Formal model

## A.1 Proofs

The proofs of the results in the main text make use of the partial derivative of an individual's odds of choosing one of the three options available to them with respect to variable $v \in \{\kappa, \tau\}$. Using Equation 1-3, these partial derivates are after rearranging:

$$\frac{\partial P(E|\tau_i,\kappa)}{\partial v} = - V_v^F(\tau_i,\kappa)P(F|\tau_i,\kappa)P(E|\tau_i,\kappa) - V_v^A(\tau_i,\kappa)P(A|\tau_i,\kappa)P(E|\tau_i,\kappa) \tag{A.1}$$

$$\frac{\partial P(F|\tau_i,\kappa)}{\partial v} = V_v^F(\tau_i,\kappa)P(F|\tau_i,\kappa)(P(E|\tau_i,\kappa) + P(A|\tau_i,\kappa)) - V_v^A(\tau_i,\kappa)P(A|\tau_i,\kappa)P(F|\tau_i,\kappa) \tag{A.2}$$

$$\frac{\partial P(A|\tau_i,\kappa)}{\partial v} = V_v^A(\tau_i,\kappa)P(A|\tau_i,\kappa)(P(E|\tau_i,\kappa) + P(F|\tau_i,\kappa)) - V_v^F(\tau_i,\kappa)P(F|\tau_i,\kappa)P(A|\tau_i,\kappa) \tag{A.3}$$

**Lemma A.1.** *For all* $\tau_i \in [\underline{\tau}, \overline{\tau}]$, $\dfrac{\partial Pr(E|\tau_i,\kappa)}{\partial \kappa} > 0$.
*The sign of* $\dfrac{\partial Pr(A|\tau_i,\kappa)}{\partial \kappa}$ *is undetermined.*

*Proof.* By Equation A.1, it is direct that $\dfrac{\partial Pr(E|\tau_i,\kappa)}{\partial \kappa} > 0$ for all $\tau_i$ since $V_\kappa^A(\cdot) \leq 0$ and $V_\kappa^F(\cdot) < 0$.
Further, notice that $\frac{\partial P(F|\tau_i,\kappa)}{\partial v} \leq 0$ under the assumption that $V_\kappa^A(\cdot) \geq V_\kappa^F(\cdot)$. This and Equation A.3 imply that the sign of $\dfrac{\partial Pr(A|\tau_i,\kappa)}{\partial \kappa}$ is undetermined. □

**Proof of Proposition 1**

The proof follows directly from Lemma A.1. □

As noted in the main text, we state and prove slightly more formal statements than Proposition 2 and 3 in this appendix: respectively, Proposition A.1 and Proposition A.2. To do so, define the following quantities

- $\overline{P_1^A} \equiv 1$ if $V_{\tau\kappa}^A(\tau_i,\kappa) \geq 0$ and $\overline{P_1^A} \equiv \min_{\tau_i \in [\underline{\tau},\overline{\tau}]} \frac{P_\tau(E|\tau_i,\kappa)V_\kappa^F(\tau_i,\kappa)}{V_{\tau\kappa}^A(\tau_i,\kappa)} \frac{P(E|\tau_i,\kappa)-P(F|\tau_i,\kappa)}{P(E|\tau_i,\kappa)}$ otherwise (notice that $\overline{P_1^A} > 0$ whenever $P(E|\tau_i,\kappa) < P(F|\tau_i,\kappa)$).
- $\overline{P_2^A} \equiv 1$ if $V_{\tau\kappa}^A(\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa)) - V_{\tau\kappa}^F(\tau_i,\kappa)P(F|\tau_i,\kappa) + V_\kappa^A(\tau_i,\kappa)P_\tau(E|\tau_i,\kappa) \geq$

0 and $\overline{P_2^A} \equiv \min_{\tau_i \in [\underline{\tau}, \overline{\tau}]} \frac{P_\tau(A|\tau_i,\kappa)\left(V_\kappa^A(\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa))-V_\kappa^F(\tau_i,\kappa)P(F|\tau_i,\kappa)\right)}{V_{\tau\kappa}^F(\tau_i,\kappa)P(F|\tau_i,\kappa)-V_{\tau\kappa}^A(\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa))-V_\kappa^A(\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)}$ otherwise (notice that $\overline{P_2^A} > 0$ if $P_\kappa(A|\tau_i,\kappa) > 0$ as per Equation A.3).

- $\underline{V^F} \equiv \max_{\tau_i \in [\underline{\tau}, \overline{\tau}]} \frac{P_\tau(F|\tau_i,\kappa)}{P(F|\tau_i,\kappa)}\left(V_\kappa^F(\tau_i,\kappa) - V_\kappa^A(\tau_i,\kappa)\right) - V_\kappa^A(\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)\frac{P(A|\tau_i,\kappa)}{P(F|\tau_i,\kappa)P(E|\tau_i,\kappa)} < 0.$

**Proposition A.1.** *If (i)* $P(F|\tau_i,\kappa) > P(E|\tau_i,\kappa)$*, (ii)* $P(A|\tau_i,\kappa) \leq \overline{P_1^A}$*, and* $V_{\kappa\tau}^F(\tau_i,\kappa) \geq \underline{V^F}$ *for all* $\tau_i$*, then* $\mathbb{P}(E|\kappa, I=1) - \mathbb{P}(E|0, I=1) < \mathbb{P}(E|\kappa, I=0) - \mathbb{P}(E|0, I=0)$ *for all* $\kappa > 0$*.*

*Proof.* We first show that $\frac{\partial^2 P(E|\tau_i,\kappa)}{\partial\kappa\partial\tau} \leq 0$ for all $\tau_i$ with strict inequality for some $\tau_i$ under the conditions of the proposition. Using Equation A.1, we get (using subscript to denote partial derivative again):

$$\begin{aligned}\frac{\partial^2 P(E|\tau_i,\kappa)}{\partial\kappa\partial\tau} =& - V_{\tau\kappa}^F(\tau_i,\kappa)P(F|\tau_i,\kappa)P(E|\tau_i,\kappa) - V_{\tau\kappa}^A(\tau_i,\kappa)P(A|\tau_i,\kappa)P(E|\tau_i,\kappa)\\ &- V_\kappa^F(\tau_i,\kappa)\left(P_\tau(F|\tau_i,\kappa)P(E|\tau_i,\kappa) + P(F|\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)\right)\\ &- V_\kappa^A(\tau_i,\kappa)\left(P_\tau(A|\tau_i,\kappa)P(E|\tau_i,\kappa) + P(A|\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)\right)\end{aligned}$$

We also must have: $P_\tau(E|\tau_i,\kappa) + P_\tau(F|\tau_i,\kappa) + P_\tau(A|\tau_i,\kappa) = 0$. Hence, we can rearrange the previous equation as:

$$\begin{aligned}\frac{\partial^2 P(E|\tau_i,\kappa)}{\partial\kappa\partial\tau} =& - V_{\tau\kappa}^F(\tau_i,\kappa)P(F|\tau_i,\kappa)P(E|\tau_i,\kappa) - V_{\tau\kappa}^A(\tau_i,\kappa)P(A|\tau_i,\kappa)P(E|\tau_i,\kappa)\\ &- V_\kappa^A(\tau_i,\kappa)P(A|\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)\\ &+ P_\tau(F|\tau_i,\kappa)P(E|\tau_i,\kappa)\left(V_\kappa^F(\tau_i,\kappa) - V_\kappa^A(\tau_i,\kappa)\right)\\ &+ P_\tau(E|\tau_i,\kappa)V_\kappa^F(\tau_i,\kappa)\left(P(E|\tau_i,\kappa) - P(F|\tau_i,\kappa)\right)\end{aligned}$$

Notice that the terms on the last three lines are negative, two of them strictly for sure. Further, under the assumption that $V_{\tau\kappa}^F(\tau_i,\kappa) \geq \underline{V^F}$ for all $\tau_i$ and $P(A|\tau_i,\kappa) \leq \overline{P_1^A}$, we obtain $\frac{\partial^2 P(E|\tau_i,\kappa)}{\partial\kappa\partial\tau} \leq 0$ for all $\tau_i$ with strict inequality for some $\tau_i$.

The second step proves the claim. Consider the function $\delta(\tau_i) = P(E|\tau_i,\kappa) - P(E|\tau_i,0)$ for a fixed $\kappa > 0$. Notice that $\delta'(\tau_i) = \int_0^\kappa \frac{\partial^2 P(E|\tau_i,\widetilde{\kappa})}{\partial\kappa\partial\tau} d\widetilde{\kappa}$. Hence, $\delta'(\tau_i) < 0$ by our first step. Using the fact that $G(\cdot|I=1)$ first order stochastically dominate $G(\cdot|I=0)$, we obtain $\int_{\underline{\tau}}^{\overline{\tau}} \delta(\widetilde{\tau}) dG(\widetilde{\tau}|I=1) < \int_{\underline{\tau}}^{\overline{\tau}} \delta(\widetilde{\tau}) dG(\widetilde{\tau}|I=0)$. This corresponds to $\mathbb{P}(E|\kappa, I=1) - \mathbb{P}(E|0, I=1) < \mathbb{P}(E|\kappa, I=0) - \mathbb{P}(E|0, I=0)$. □

**Proposition A.2.** *If (i)* $P_\kappa(A|\tau_i,\kappa) > 0$ *and (ii)* $P(A|\tau_i,\kappa) \leq \overline{P_2^A}$ *for all* $\tau_i$*, then* $\mathbb{P}(A|\kappa, I = 1) - \mathbb{P}(A|0, I = 1) > \mathbb{P}(A|\kappa, I = 0) - \mathbb{P}(A|0, I = 0)$.

*Proof.* We proceed along te same steps as the proof of Proposition A.1. Using Equation A.3 and re-arranging, we obtain:

$$\begin{aligned}\frac{\partial^2 P(A|\tau_i,\kappa)}{\partial\kappa\partial\tau} =& V^A_{\tau\kappa}(\tau_i,\kappa)P(A|\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa)) - V^F_{\tau\kappa}(\tau_i,\kappa)P(F|\tau_i,\kappa)P(A|\tau_i,\kappa)\\ &+ P_\tau(A|\tau_i,\kappa)\left(V^A_\kappa(\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa)) - V^F_\kappa(\tau_i,\kappa)P(F|\tau_i,\kappa)\right)\\ &+ P(A|\tau_i,\kappa)\left(V^A_\kappa(\tau_i,\kappa)P_\tau(F|\tau_i,\kappa) - V^F_\kappa(\tau_i,\kappa)P_\tau(F|\tau_i,\kappa)\right)\\ &+ V^A_\kappa(\tau_i,\kappa)P(A|\tau_i,\kappa)P_\tau(E|\tau_i,\kappa)\end{aligned}$$

Notice that $P_\kappa(A|\tau_i,\kappa) > 0$ implies $V^A_\kappa(\tau_i,\kappa)(P(E|\tau_i,\kappa)+P(F|\tau_i,\kappa)) - V^F_\kappa(\tau_i,\kappa)P(F|\tau_i,\kappa) > 0$ and by the same token $\overline{P_2^A} > 0$ as noted above. Further, given that $V^A_\kappa(\tau_i,\kappa)P_\tau(F|\tau_i,\kappa) - V^F_\kappa(\tau_i,\kappa)P_\tau(F|\tau_i,\kappa) \geq 0$, we obtain that $\frac{\partial^2 P(A|\tau_i,\kappa)}{\partial\kappa\partial\tau} \geq 0$ for all $\tau_i$ with strict inequality for some. We can then apply the first order stochastic dominance property of the distribution function and the same logic as in the proof of Proposition A.1 to prove the claim. □

## A.2 Possible microfoundations

In this subsection, we present two possible ways to microfound our continuation values for remaining foreigner and assimilating. These two approaches are not mutually exclusive and definitely not exhaustive. They are just meant to illustrate the problems foreigners face. The first microfoundation looks at the labour market and the other at social relationships.

### Labour market

We consider a model with an infinite number of discrete periods denoted by $t$ and with a discount rate $\beta \in (0,1)$. Each individual $i$ who belongs to group $J \in \{F, A\}$ (foreign or assimilated) can be either employed, status $s = e$, or unemployed, status $s = u$. Each period, an individual starts with one of the two statuses. In this context, we assume that the level of integration equates the productivity in the host economy.

If they are employed at the start of period $t$, they earn a wage $w(\tau_i,\kappa;J)$ weakly increasing in $\tau_i$. In other words, individuals with higher level of productivity receive higher wage. At the end

of the period, with probability $\lambda \in (0,1)$ an employed individual loses their job and becomes unemployed next period, with probability $1-\lambda$, they remain employed.

If they are unemployed at the start of period $t$, they earn zero (a normalization). At the end of the period, with probability $\pi(\tau_i, \kappa; J)$ they find a job and become employed next period, with the remaining probability they remain unemployed. We assume that $\pi(\tau_i, \kappa; J)$ is weakly increasing in $\tau_i$ (higher productivity helps finding jobs, e.g.).

Denote $W^e(\cdot)$ and $W^u(\text{cot})$, the labor market continuation values from being employed or unemployed at the beginning of a period $t$. These continuation values are then defined by the following two equations:

$$\begin{aligned} W^e(\tau_i, \kappa; J) =& w(\tau_i, \kappa; J) + \beta(1-\lambda)W^e(\tau_i, \kappa; J) + \beta\lambda W^u(\tau_i; J) \\ W^u(\tau_i, \kappa; J) =& 0 + \beta\pi(\tau_i, \kappa; J)W^e(\tau_i, \kappa; J) + \beta(1-\pi(\tau_i, \kappa; J))W^u(\tau_i, \kappa; J) \end{aligned}$$

This yields:

$$\begin{aligned} W^e(\tau_i, \kappa; J) =& \frac{1-\beta+\beta\pi(\tau_i, \kappa; J))}{(1-\beta)(1-\beta+\beta(\pi(\tau_i, \kappa; J)+\lambda))} w(\tau_i, \kappa; J) \\ W^u(\tau_i; J) =& \frac{\beta\pi(\tau_i, \kappa; J)}{(1-\beta)(1-\beta+\beta(\pi(\tau_i, \kappa; J)+\lambda))} w(\tau_i, \kappa; J) \end{aligned}$$

Notice that both labor market continuation values are strictly increasing with $\pi(\cdot)$ and $w(\cdot)$ and so are increasing with $\tau_i$.

In the main analysis, we do not distinguish between employed and unemployed, so we can take an ex-ante perspective assuming that an individual with type $\tau_i$ has a probability $\alpha(\tau_i, \kappa; J)$ of being employed.[1] Since $\pi(\tau_i; J)$ is increasing with $\tau_i$, so is $\alpha(\tau_i; J)$. Hence, the continuation value for an individual who chooses option $J \in \{F, A\}$ is: $V^J(\tau_i, \kappa) = \alpha(\tau_i, \kappa; J)W^e(\tau_i, \kappa; J) + (1-\alpha(\tau_i, \kappa; J))W^u(\tau_i, \kappa; J)$. The assumption in the text about the level of integration is equivalent to $\pi_\tau(\tau_i, \kappa; A) \geq \pi_\tau(\tau_i, \kappa; F)$, which should yield $\alpha_\tau(\tau_i, \kappa; A) \geq \alpha_\tau(\tau_i, \kappa; F)$, and $w_\tau(\tau_i, \kappa; A) \geq w_\tau(\tau_i, \kappa; F)$ with one strict inequality. Basically, we are assuming that the labor market benefit from being more productive is higher for an individual that chooses assimilation than an individual that chooses to remain foreigner. Assimilation could yield a better network

[1] Note that we are not considering a general equilibrium on the labour market.

making it easier for all to find jobs, especially for highly productive individuals. Alternatively, assimilation could open new occupations improving both the odds of finding a job as well as the salary, especially for the most productive individuals (notice that we omit all costs of assimilation here).

What about exposure to violence ($\kappa$)? We think that there are many ways it could affect labor market outcomes. First, there can be a rise in individual discrimination forcing foreigners to take a wage cut. In this case, we can think that $w(\tau_i, \kappa; J) = w(\tau_i; J) - c(\kappa; J)$ with $c(\kappa; J)$ increasing in $\kappa$ and $c(\kappa; F) > c(\kappa; A)$ (assimilation protects against discrimination). Alternatively, the violence could trigger a form of social discrimination as in Peski and Szentes (2013) and Dewan and Wolton (2022). Then as shown in Dewan and Wolton (2022), such type of discrimination could yell a lower probability of finding employment ($\pi(\tau_i, \kappa; J)$ is strictly decreasing with $\kappa$) and a lower wage conditional on being employed ($w(\tau_i, \kappa; J)$ is also strictly decreasing with $\kappa$). As this form of discrimination depends on social identity being salient, one can assume that individuals who choose $A$ expose less their foreign identity than individuals who choose $F$ yielding $\pi_\kappa(\tau_i, \kappa; A) \geq \pi_\kappa(\tau_i, \kappa; F)$ and $w_\kappa(\tau_i, \kappa; A) \geq w_\kappa(\tau_i, \kappa; F)$. In both possible scenarios, assuming $c(\kappa; F)$ is sufficiently larger than $c(\kappa; A)$, we would recover $V_\kappa^F(\tau_i, \kappa) \leq V_\kappa^A(\tau_i, \kappa) < 0$.

**Social relations**

We now build a one-period model of social relations (we briefly discuss extending the model to a multi-period game below). A foreigner $i$ with type $\tau_i$ chooses how much effort $e$ to spend interacting with natives relative to members of their own group. The benefit of interacting with natives depends on two factors: (i) the proportion of open-minded individuals versus close-minded individuals in the population and (ii) the choice of a foreigner to keep their status ($J = F$) or to assimilate ($J = A$). The gain from interacting with one group also depends on the foreigner's choice. Finally, we assume that $\tau_i$ affects the cost of effort interacting with natives. As such, we can think of the level of integration $\tau_i$ as how well individual $i$ has incorporated the cultural norms of the host country.

When a foreigner $i$ interacts with a member of his own group, they receive a payoff $u(J)$ with $u(A) < u(F)$. That is, leaving one's nationality group to assimilate into the host country makes it harder to have good relationships with in-group members. This is in line with many

papers that show that even discriminated minorities can make it hard to leave one's own group (e.g., Austen-Smith and Fryer, 2005; Carvahlo, 2013; Schnakenberg, 2013). When a foreigner $i$ interacts with an open-minded native, they get a payoff of $\overline{v}$, when they interact with a closed-minded native, they get a payoff of $\underline{v}(J)$. We assume that $\underline{v}(F) < \underline{v}(A)$ so that an assimilated foreigner has less trouble with closed-minded natives. This in line with evidence from our context in which during the riots in Lyon and around, the mob asked individuals their identity paper to verify their nationality and shop owners who had naturalized where displaying their nationality certificate in their shop. We also assume that the proportion of closed-minded types is unknown and his characterized by a belief $\pi(\kappa)$, strictly increasing in $\kappa$. In other words, exposure to violence provides information about the tolerance of the host country population. Higher exposure leads to worse opinion about the acceptance of natives. Finally, we assume that the cost of interacting with natives is $c(e, \tau_i)$ which is strictly increasing and convex with $e$, strictly decreasing with $\tau_i$, and satisfying $c_{e\tau}(e, \tau_i) < 0$. In other words, the marginal cost of effort is decreasing with the level of integration.

The utility of an individual with type $\tau_i$ who has made choice $J \in \{A, F\}$ after exposure to violence $\kappa$ is:

$$U(e; \tau_i, \kappa, J) = (1 - e)u(J) + e(\pi(\kappa)\overline{v} + (1 - \pi(\kappa))\underline{v}(J)) - c(e, \tau_i)$$

Individual $i$ then maximizes their utility with respect to $e$. Denote $e^*(\tau_i, \kappa, J)$ the equilibrium choice (i.e., the solution to $c_e(e, \tau_i) = \pi(\kappa)\overline{v} + (1 - \pi(\kappa))\underline{v}(J) - u(J)$ assuming the right-hand side is always strictly positive). We then have that the value functions in the main text are:

$$V^J(\tau_i, \kappa) = U(e^*(\tau_i, \kappa, J); \tau_i, \kappa, J)$$

By the Envelop Theorem, we have $V^J_\tau(\tau_i, \kappa) = -c_\tau(e^*(\tau_i, \kappa, J), \tau_i) > 0$. Notice that since $u(A) < u(F)$ and $\underline{v}(A) > \underline{v}(F)$, we have $e^*(\tau_i, \kappa, A) > e^*(\tau_i, \kappa, F)$, which implies $V^A_\tau(\tau_i, \kappa) > V^F_\tau(\tau_i, \kappa)$.

By the Envelop Theorem, we also have $V^J_\kappa(\tau_i, \kappa) = e^*(\tau_i, \kappa; J)\pi'(\kappa)(\overline{v} - \underline{v}(J)) < 0$. Notice that $V^J_\kappa(\tau_i, \kappa) \to 0$ as $\underline{v}(A) \to \overline{v}$. Given that every term is continuous, there exists $\widehat{v} < \underline{v}$ (we omit the dependence of $\widehat{v}$ on other parameters) such that as long as $\underline{v}(A) \geq \widehat{v}$, $V^F_\kappa(\tau_i, \kappa) \leq V^A_\kappa(\tau_i, \kappa)$.

In other words, as long as assimilation protects against the loss from interacting with closed-minded natives, we recover that exposure to violence decreases the payoff of remaining foreigner faster than the utility from assimilating. In our context, we have discussed in the main text the social benefit from naturalizing (protection against beatings by the mob and some form of insurance against ransacking or so shop owners believed).

As a final note, let us discuss how the model can be extended to multiple periods. Foreigners make multiple interactions over the course of their life and should learn something about the underlying distribution of types. We can extend the model to a $T$-period game (with $T$ finite). Every period, a foreigner $i$ chooses their effort to interact with natives. We suppose that every period they have one interaction either with an open-minded native ($r = 1$) or with a closed-minded native ($r = 0$). The only difference with the model above is then that a foreigner $i$ when she chooses her effort has a belief $\pi_t(\{\kappa_h\}_{h=1}^t; \{r_h\}_{h=1}^{t-1})$ based on past and present exposure to violence and her interaction in the past periods (we denote the prior $\pi_0$ and the interim belief after exposure to violence in period 1 by $\pi_0(\kappa_1)$).

The timing each period is then as follows: 0. Each foreigner inherits the belief $\pi_{t-1}(\{\kappa_h\}_{h=1}^{t-1}; \{r_h\}_{h=1}^{t-1})$ from the previous period (and $\pi_0$ in period 1). 1. Each foreigner observes possible violence $\kappa_t \geq 0$. 2. Each foreigner chooses $e_t$. 3. Each foreigner as an interaction with an open-minded native ($r_t = 1$) or a closed-minded native ($r_t = 0$).

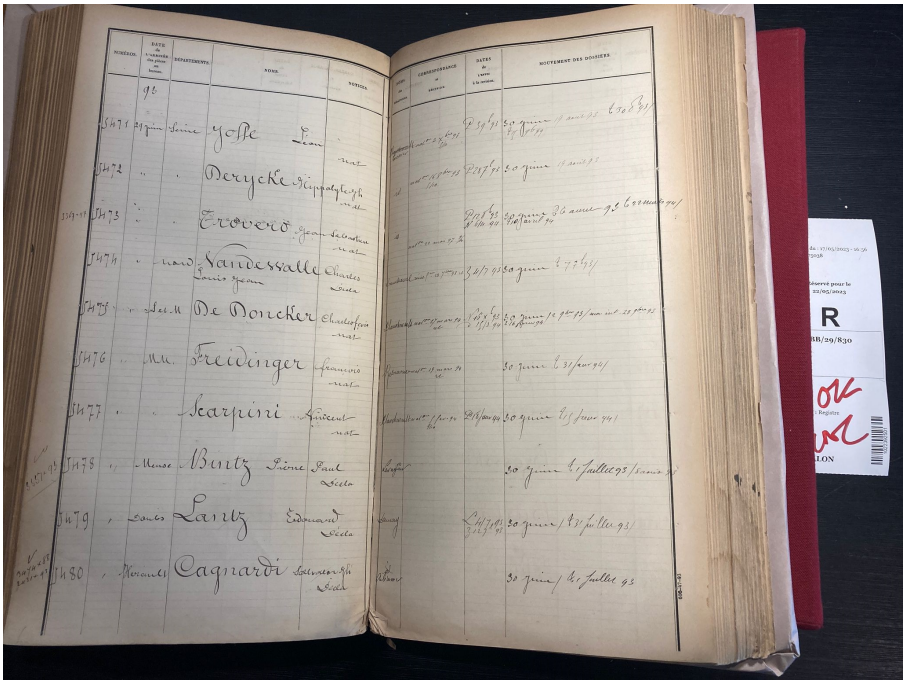
To make the model tractable, we can assume that (i) the prior $\pi_0$ (prior to seeing $\kappa_1$) follows a Beta distribution with parameters $P > 0$ and $N > 0$ and (ii) observing violence $\kappa$ corresponds to an exogenous shock on the parameter $N$ which increases by $\kappa$. With this assumption, each period $t \geq 1$, the posterior only depends on the number of positive encounters in the past, which we denote $p_t$, with $0 \leq p \leq t-1$ (the remaining encounters with natives are with closed minded individuals and equal $n_t = t - 1 - p_t$). The posterior in period $t$ when individual $i$ chooses their effort is then: $\pi_t(\{\kappa_h\}_{h=1}^t; \{r_h\}_{h=1}^{t-1}) \equiv \pi(\sum_{h=1}^t \kappa_h, p_t) = \frac{P+p_t}{P+N+(t-1)+\sum_{h=1}^t \kappa_h}$. Hence, all the results above can be recovered in a dynamic model with $T$ periods.

# B Background information

## Example of naturalization decrees

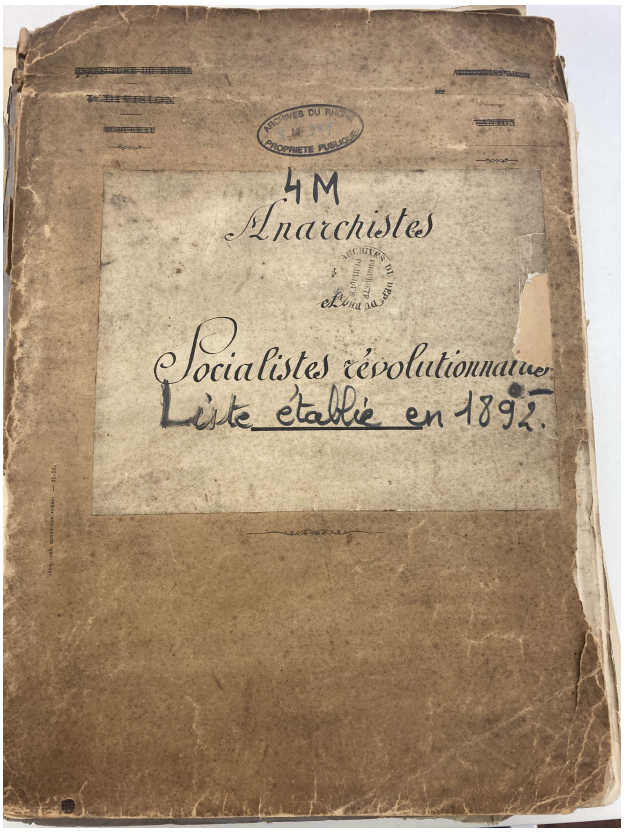
Figure B.1: Example of naturalization decrees: indexed record (January 4, 1887)

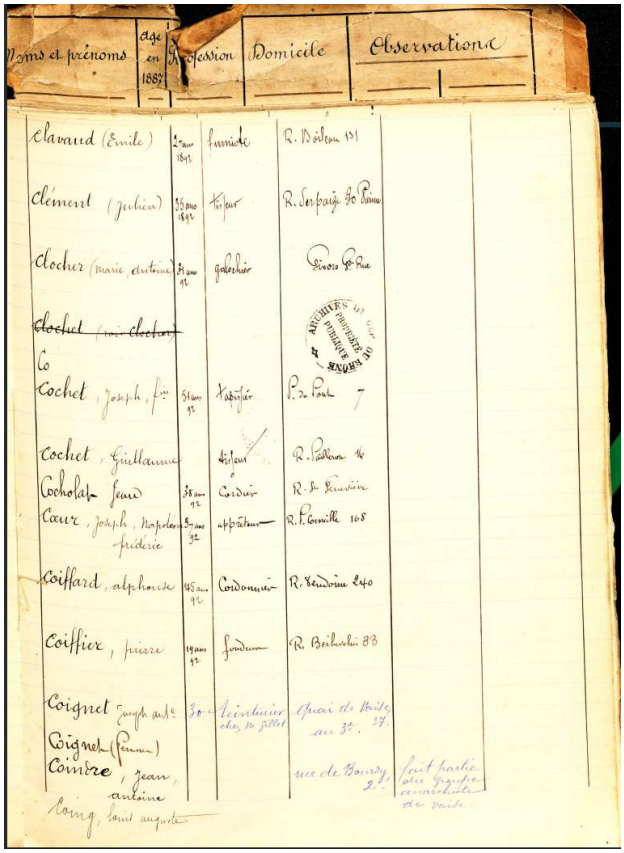

Figure B.2: Example of a page from the naturalization register



## Anarchists in the Rhône in 1892

Figure B.3: Example of a page from the list of anarchists compiled by the Rhône prefecture in 1892



## Linking individuals

We construct our two main outcomes (exit from the municipality and naturalization application) by linking individuals either between census records or from the census records to naturalization decrees. We do so using a probabilistic model (rather than deterministic methods) which allows us to incorporate the uncertainty inherent to the merging process in the post-merge analysis. In practice, we use Enamorado et al.'s (2017) `fastlink` R package which proceeds in three steps.

First, it computes an agreement vector $\gamma_{ij}$ of length $K$ (the number of linking variables) for each combination $(i, j)$ of $i$th observation from dataset A and $j$th observation from data B. For string variables, the agreement is computed using the Jaro-Winkler similarity (Jaro, 1989; Winkler, 1990) and categorized as nearly identical (coded as 2) if the similarity is greater than .92, as similar (coded as 1) if the similarity is between .88 and .92, different (coded as 0) if the similarity is below .88, or missing if the variable is missing in either dataset. The agreement on numeric variables is equal to 2 if they are similar, 0 if they differ, and missing if one of them is missing.

Second, the package uses the Expectation-Maximization Algorithm to estimate $\lambda$, the probability of a match across all pairwise comparisons, and $2 \times K$ vectors $\pi_{km}$ of length $L_k$, the probability of each agreement level (0,1, 2 or NA for string variables and 0, 2 or NA for numeric variables) given that the pair is a match ($m = 1$) or a nonmatch ($m = 0$).

Finally, it computes the match probability for each pair using Bayes' rule using the estimates of $\lambda$ and $\pi_{km}$. We select the best match among matches with a probability greater than .85 to produce our main results but show robustness when keeping all matches above .85 and weighting outcomes by match probability, as recommended by Enamorado et al. (2017).

We link individuals using 3 main variables: the first and last names and the year of birth. We allow for the year of birth to vary by 1 year between linked datasets because the census enumerators recorded sometimes individuals' year of birth and sometimes their age, generating uncertainty about individuals' exact year of birth. In the Census data, identifying first and last names is complicated by the fact that first and last names are listed in a single column. Given that having more than one first name and more than one last name is not uncommon, we identify first and last names as follows. First, we clean full names by (1) converting all abbreviations to full names (for instance, Jean, which is a common first name is often written

as Jn, similarly Francois, also very frequent, is often listed as Fcois); (2) remove all auxiliary information and particles from full names (for instance, we drop elements like "de", "mlle," "veuve") (3) remove all accents and other special characters. Second, we count the total number of remaining elements of the full name. In the full sample ($N = 2,277,497$), this ranges from 0 to 7. 0.51% have strictly less than two names, 89.34% have exactly two, and 10% have more than 2. Third, using information on the number of elements of the full name, we extract the first name as the first element and the last name as the last, effectively dropping information from all middle elements, and replacing first and last names with missing values when the full name as less than 2 elements. In the naturalization decrees, however, first and last names are separated with a comma, though full names can also have more than two elements. 34% have 2 names, 43% have three and 20% have four. Individuals with only 2 names elements are less common in the naturalization decrees than in the census is due to the administrative nature of the two document. Naturalization decrees are official civil status document and must list all first names, whereas the census is not an official document and individuals tend to report only lists the first name they use in everyday life. To deal with this issue, we duplicate each observation using all possible combinations of first and last names. For instance, "veuve dubail née doré, françoise anne" appears in our preprocessed dataset to be used for linking four times as "francoise dubail," "francoise dore," "anne dubail," and "anne dore."

We treat information regarding the place of residence differently depending on whether we link between census records (where we block on municipality) or whether we link census records to naturalization decrees (we don't use information about the municipality of residence). When linking census records, we link foreigners from the 1886 census to the full 1891 census and foreigners from the 1891 census to the full 1896 census. We match foreigners in 1886 and 1891 to full censuses in the next period because we want to track people who naturalized and might be counted as French in the next census period.

Matching approximately 15,000 foreigners to 750,000 individual entries amounts to roughly 10 billion pairs each time to evaluate 3 variables, which makes the linking computationally intensive. We simplify computation by first estimating the relevant probabilities using 20% of each census year (not blocking on municipality). This first step involves estimating the probability that a pair is a match conditional on all combinations of agreement. Second, we match individuals blocking on municipality using the match probabilities estimates from step

1 to eliminate unlikely match. While our primary motivation is to simplify computation, the resulting linkage captures whether an individual is still living in the same municipality in the next census period.

Table B.1: Difference in exit rate pre-treatment (1886-1891) between travellers and others

| | Left the municipality by t+5 | | | Left the department by t+5 | |
|---|---|---|---|---|---|
| | Among French | Among Italian | Among Suisse | Among Italian | Among Suisse |
| Traveller | 0.076 | 0.295 | 0.175 | 0.377 | 0.252 |
| | (0.102) | (0.054) | (0.230) | (0.011) | (0.185) |
| Observations | 57,396 | 7,021 | 2,988 | 7,021 | 2,988 |
| # of municipalities | 127 | 67 | 33 | 67 | 33 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

# C Main analysis: Pre-trends and additional results

## Pre-trends for exit

As noted in the main text, the 1881 census does not include nationality, making it difficult to test for pre-trends for exit. We use an alternative approach to determine whether pre-trends differ between Italians and Swiss. We use the association between last name and nationality in the 1886 census, to predict nationality based on last names in the 1881, 1886 and 1891 censuses. We restrict our attention to last names listed in all censuses and code as Italian or Swiss-sounding individuals (regardless of their reported nationality) if they have a last name index above 90 following the methodology in (Fouka, 2019, 2020)Fouka (2019, 2020).

More specifically, we first calculate the proportion of Italians having a certain last name $P(\text{name}|\text{Italian})$ and the proportion of non-Italians having the same last name $P(\text{name}|\neg\text{ Italian})$ based on the 1886 census. We do the same for Swiss nationals. We then compute the following two indices:

$$\text{INI}_{name} = \frac{P(\text{name}|\text{Italian})}{P(\text{name}|\text{Italian}) + P(\text{name}|\neg\text{ Italian})} \times 100$$
$$\text{SNI}_{name} = \frac{P(\text{name}|\text{Swiss})}{P(\text{name}|\text{Swiss}) + P(\text{name}|\neg\text{ Swiss})} \times 100$$

We keep in our sample individuals who have a name with an $\text{INI}_{name}$ and a $\text{SNI}_{name}$ above 90 in the 1881, 1886, and 1891 censuses. Our dummy Italian then equals one for individuals with an $\text{INI}_{name}$ over 90 and zero for all other individuals in the sample. We then link the individuals in our sample over censuses using the same methodology as in the main text (probabilistic matching using first and last names and years of birth, plus or minus one, but not nationality, treating the municipality of residence as a blocking variable). We generate an indicator variable equal to one if we cannot link an individual in census $t$ to an individual in census $t+5$, $t \in \{1881, 1886, 1891\}$ (in the sense of finding a match with a probability score greater than 0.85). We link 42% of individuals with an Italian and Swiss sounding last name in the 1881, 1886 and 1891 to an individual in census 5 years later.

Table C.1 shows the result of our regression using Equation 7 on the sample described above. The year 1886 is our reference year and is, thus, omitted from the analysis. We see little

evidence of pre-trends, the only statistically significant coefficients are for the year 1891. This provides some evidence in favour of the parallel trend assumptions holding in our context for exit.

Table C.1: Left the municipality: Pre-trends

| | Left the municipality |
|---|---|
| 1881 | -0.017 |
| | (0.007) |
| 1891 | -0.029 |
| | (0.007) |
| Italian | 0.016 |
| | (0.007) |
| 1881 × Italian | -0.013 |
| | (0.017) |
| 1891 × Italian | 0.020 |
| | (0.007) |
| Constant | 0.581 |
| | (0.003) |
| Observations | 30,180 |
| # of municipalities | 230 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

## Additional results: exit

Table C.2: Left the department: Main result

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | -0.001 | -0.010 | -0.009 | -0.012 | -0.011 |
| | (0.006) | (0.003) | (0.003) | (0.005) | (0.005) |
| Italian | | | -0.023 | | 0.014 |
| | | | (0.004) | | (0.005) |
| 1891 × Italian | | | 0.007 | | 0.009 |
| | | | (0.007) | | (0.006) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table C.3: Relocated within the department: Main result

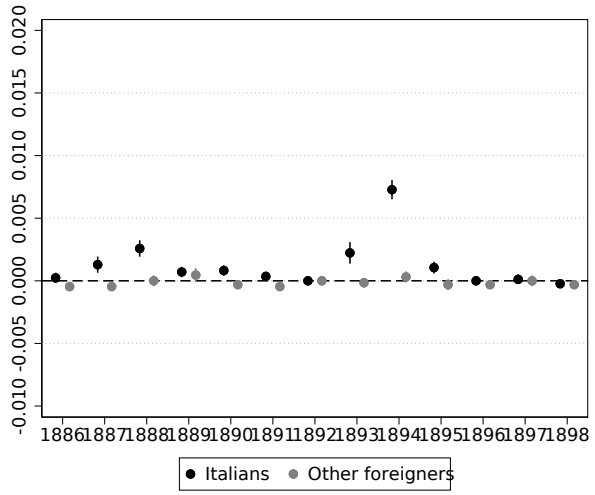
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | 0.005 | -0.001 | -0.002 | 0.003 | -0.000 |
| | (0.006) | (0.003) | (0.004) | (0.003) | (0.005) |
| Italian | | | 0.007 | | 0.002 |
| | | | (0.005) | | (0.009) |
| 1891 × Italian | | | 0.007 | | 0.006 |
| | | | (0.009) | | (0.011) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

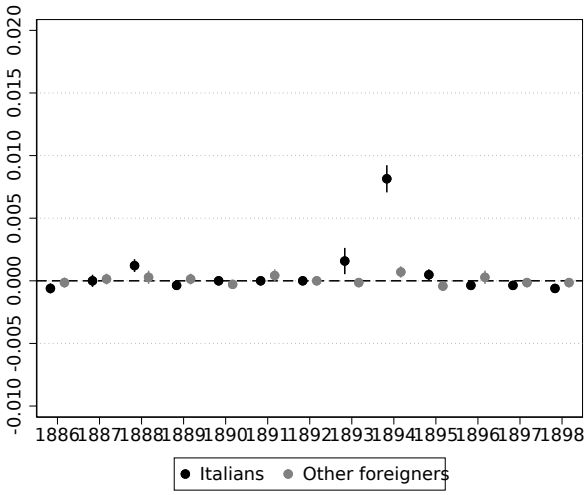
# D Robustness

Figure D.1: Applied for citizenship: Results from panel data estimation

(a) Using only the 1886 census




(b) Using only the 1891 census




*Notes:* Coefficients and 95% percent confidence intervals from robust standard errors clustered at the municipality level from two linear probability models (one for Italians and one for other foreigners) using only individuals from the 1886 Census (panel a) or only individuals from the 1891 Census (panel b). In each model, we regress an indicator variable for whether applicants applied in a given year on year of application fixed effects taking 1892 as the reference category and controlling for census year fixed effects and municipality fixed effects. The data format is an individual-year application panel dataset of 15,028 foreigners in 1886 and 15,313 foreigners in 1891 times 11 years of application.

Table D.1: Robustness tests

| | DV: Applied for citizenship | | | | | | |
|---|---|---|---|---|---|---|---|
| | Main Estimate | Weighing all matches | Keeping only known genders | Only Men | Only Female | Less than 50 | Only 2 name elements |
| 1891 | 0.000 | 0.000 | -0.000 | -0.000 | -0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.001) | (0.002) | (0.000) | (0.000) | (0.000) |
| Italian | 0.005 | 0.005 | 0.005 | 0.011 | 0.000 | 0.005 | 0.004 |
| | (0.001) | (0.000) | (0.001) | (0.002) | (0.000) | (0.001) | (0.000) |
| 1891 × Italian | 0.006 | 0.006 | 0.003 | 0.004 | 0.002 | 0.006 | 0.006 |
| | (0.001) | (0.001) | (0.001) | (0.002) | (0.000) | (0.001) | (0.001) |
| Observations | 30,311 | 30,311 | 13,740 | 4,807 | 8,899 | 25,883 | 28,431 |
| # of municipalities | 132 | 132 | 99 | 50 | 85 | 122 | 119 |
| | DV: Left the municipality | | | | | | |
| | Main Estimate | Weighing all matches | Keeping only known genders | Only Men | Only Female | Less than 50 | Only 2 name elements |
| 1891 | -0.010 | -0.008 | -0.032 | -0.044 | -0.030 | -0.015 | -0.008 |
| | (0.005) | (0.005) | (0.006) | (0.011) | (0.005) | (0.006) | (0.004) |
| Italian | -0.017 | -0.016 | -0.038 | -0.034 | -0.033 | -0.015 | -0.012 |
| | (0.006) | (0.006) | (0.006) | (0.009) | (0.006) | (0.006) | (0.005) |
| 1891 × Italian | 0.014 | 0.015 | 0.053 | 0.071 | 0.048 | 0.016 | 0.011 |
| | (0.006) | (0.005) | (0.006) | (0.011) | (0.007) | (0.007) | (0.005) |
| Observations | 30,311 | 30,311 | 13,740 | 4,807 | 8,899 | 25,883 | 28,431 |
| # of municipalities | 132 | 132 | 99 | 50 | 85 | 122 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.2: Effect of violence on the probability to change one's last name

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | -0.000 | 0.000 | 0.000 | -0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) |
| Italian | | | 0.001 | | 0.001 |
| | | | (0.000) | | (0.001) |
| 1891 × Italian | | | -0.001 | | -0.001 |
| | | | (0.001) | | (0.001) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.3: Linked to a French individual (conditional on being linked to an individual in the same municipality in the next census)

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | 0.191 | 0.108 | 0.109 | 0.087 | 0.088 |
| | (0.007) | (0.007) | (0.007) | (0.004) | (0.005) |
| Italian | | | -0.059 | | -0.040 |
| | | | (0.017) | | (0.017) |
| 1891 × Italian | | | 0.081 | | 0.102 |
| | | | (0.012) | | (0.010) |
| Observations | 5,339 | 4,283 | 9,635 | 2,921 | 8,270 |
| # of municipalities | 55 | 43 | 68 | 37 | 67 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.4: Linked to a French individual within municiality (unconditional)

| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Swiss | Comparing Italians to Swiss |
|---|---|---|---|---|---|
| 1891 | 0.061 | 0.040 | 0.040 | 0.034 | 0.036 |
| | (0.002) | (0.004) | (0.003) | (0.003) | (0.003) |
| Italian | | | -0.011 | | -0.023 |
| | | | (0.005) | | (0.004) |
| 1891 × Italian | | | 0.020 | | 0.024 |
| | | | (0.004) | | (0.004) |
| Observations | 16,718 | 13,563 | 30,311 | 8,342 | 25,088 |
| # of municipalities | 96 | 92 | 132 | 69 | 119 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.5: Effect on partnership with French (conditional)

| | All | | | Singles | | | In Partnership | | |
|---|---|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners |
| 1891 | 0.135 | 0.036 | 0.032 | 0.124 | 0.070 | 0.055 | 0.161 | 0.045 | 0.042 |
| | (0.007) | (0.016) | (0.019) | (0.016) | (0.015) | (0.029) | (0.009) | (0.023) | (0.024) |
| Italian | | | -0.082 | | | -0.084 | | | -0.062 |
| | | | (0.018) | | | (0.020) | | | (0.026) |
| 1891 × Italian | | | 0.101 | | | 0.062 | | | 0.115 |
| | | | (0.018) | | | (0.021) | | | (0.022) |
| Observations | 2,063 | 1,665 | 3,741 | 429 | 324 | 767 | 1,196 | 990 | 2,199 |
| # of municipalities | 45 | 31 | 57 | 12 | 8 | 21 | 36 | 26 | 48 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.6: Effect on partnership with French (unconditional)

| | All | | | Singles | | | In Partnership | | |
|---|---|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners |
| 1891 | 0.027 | 0.023 | 0.023 | -0.000 | 0.013 | 0.012 | 0.050 | 0.040 | 0.040 |
| | (0.002) | (0.004) | (0.004) | (0.002) | (0.003) | (0.003) | (0.003) | (0.007) | (0.007) |
| Italian | | | -0.002 | | | 0.008 | | | -0.015 |
| | | | (0.003) | | | (0.004) | | | (0.006) |
| 1891 × Italian | | | 0.003 | | | -0.012 | | | 0.007 |
| | | | (0.004) | | | (0.004) | | | (0.007) |
| Observations | 13,438 | 10,940 | 24,408 | 5,203 | 4,296 | 9,521 | 4,608 | 3,506 | 8,129 |
| # of municipalities | 92 | 88 | 127 | 73 | 63 | 99 | 61 | 52 | 77 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table D.7: Heterogeneity by anarchist status

| | Left the commune | | | Applied for citizenship | | |
|---|---|---|---|---|---|---|
| | Anarchiste | Not Anarchiste | Both | Anarchiste | Not Anarchiste | Both |
| 1891 | -0.041 | -0.008 | -0.009 | -0.002 | 0.001 | 0.000 |
| | (0.011) | (0.005) | (0.005) | (0.000) | (0.000) | (0.000) |
| Italian | -0.100 | -0.018 | -0.018 | 0.011 | 0.005 | 0.005 |
| | (0.035) | (0.006) | (0.006) | (0.002) | (0.001) | (0.001) |
| 1891 × Italian | 0.022 | 0.013 | 0.013 | -0.007 | 0.007 | 0.007 |
| | (0.036) | (0.006) | (0.006) | (0.001) | (0.001) | (0.001) |
| Anarchiste | | | -0.118 | | | 0.002 |
| | | | (0.011) | | | (0.000) |
| 1891 × Anarchiste | | | -0.029 | | | -0.003 |
| | | | (0.015) | | | (0.000) |
| Italian × Anarchiste | | | -0.075 | | | 0.005 |
| | | | (0.042) | | | (0.003) |
| 1891 × Italian × Anarchiste | | | 0.015 | | | -0.010 |
| | | | (0.032) | | | (0.005) |
| Observations | 1,798 | 28,487 | 30,311 | 1,798 | 28,487 | 30,311 |
| # of municipalities | 29 | 129 | 132 | 29 | 129 | 132 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

# E Exposure to violence: Additional results

Table E.1: Effect on partnership with French: Heterogeneity by exposure to violence (unconditional)

| | High exposure | | | Low exposure | | |
|---|---|---|---|---|---|---|
| | All | Single | In Partnership | All | Single | In Partnership |
| 1891 | 0.051 | 0.029 | 0.074 | 0.022 | 0.011 | 0.039 |
| | (0.000) | (0.003) | (0.001) | (0.004) | (0.003) | (0.007) |
| Italian | 0.025 | 0.029 | 0.063 | -0.004 | 0.007 | -0.019 |
| | (0.000) | (0.006) | (0.001) | (0.003) | (0.004) | (0.006) |
| 1891 × Italian | -0.004 | -0.010 | -0.054 | 0.004 | -0.012 | 0.010 |
| | (0.000) | (0.006) | (0.001) | (0.004) | (0.004) | (0.007) |
| Observations | 1,376 | 640 | 455 | 23,032 | 8,879 | 7,673 |
| # of municipalities | 4 | 2 | 3 | 127 | 99 | 76 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

# F Heterogeneity: Details and additional results

## Proxies for baseline integration level

**Mixed households** To identify individuals living in a mixed household, we first restrict the sample to the 1,509,124 individuals in the 1886 and 1891 censuses. We then restrict further to 681,682 heads and spouses only. And then further to the 490,772 individuals in a household with 2 persons (i.e. households with a head and a spouse). We then count the number of French individuals in the household. Foreigners in a household with a French person are coded as in a mixed marriage and foreigners living in a household with no French are coded as not living in a mixed marriage. In total, we have the information on mixed households for 910,347 individuals (or on 245,386 households which represents 45% of all households of 1886 and 1891).

**Position in the household.** We use one feature of the nominative census which lists the relationships of every occupant in a given household. We look separately at individuals who are listed as head of household, spouse or children and those who are listed as domestiques.

**Occupation.** The nominative census also includes information about the occupation of individuals. Since occupation is self-reported, there is a large number of different occupations listed. Among the 30,323 Italians and other foreigners with known nationality listed in the 1886 and the 1891 census, we have information about their occupation for 17,195 of them (57%), corresponding to 1,389 different occupations, 784 of which are unique to a single individual and only 37 occupations are reported by more than 100 individuals. We therefore focus on a few occupations that are stated relatively often. We separate individuals between those we can identify as holding a salaried position and those who are more likely to have invested capital in the host country. We refer to the first as workers and to the second as shopkeepers. In Table F.1, we list the top 30 occupations coded in either category.

**Economic situation.** As noted above, the nominative census lists all individuals living in the same household. We can use this feature to identify family who also hosts domestics and/or workers. While we can guarantee that domestics are working in the household, some workers may have been renting a room in the house. Nonetheless, we treat the presence of either (or both) as evidence of economic success. That is, we look separately at households with and without domestics/workers living in their premise.

Table F.1: 30 most common occupation coded as shopkeepers or workers

| Shop owners | | Workers | |
|---|---|---|---|
| *occupation* | *count* | *occupation* | *count* |
| epicier | 189 | platrier | 899 |
| cafetier | 56 | manoeuvre | 817 |
| bijoutier | 36 | employe | 778 |
| marchand | 33 | journalier | 665 |
| boulanger | 25 | cordonnier | 490 |
| restaurateur | 25 | couturier | 480 |
| representant de commerce | 22 | tisseur | 355 |
| debitant | 22 | peintre | 302 |
| marchand ambulant | 21 | menuisier | 257 |
| debitante | 20 | tailleur | 242 |
| limonadier | 19 | ouvrier | 234 |
| marchand de vins | 18 | ovaliste | 189 |
| revendeuse | 16 | verrier | 180 |
| logeuse | 15 | ferblantier | 180 |
| marchand de soie | 15 | ebeniste | 170 |
| confiseur | 14 | mineur | 161 |
| logeur | 14 | cuisinier | 158 |
| charcutier | 12 | teinturier | 155 |
| voyageur de commerce | 11 | mecanicien | 153 |
| patissier | 11 | chapelier | 138 |
| garcon limonadier | 11 | blanchisseur | 134 |
| commercant | 9 | etameur | 128 |
| marchande | 8 | corroyeur | 128 |
| revendeur | 6 | brodeur | 122 |
| marchand de charbons | 6 | peintre platrier | 115 |
| marchand de bois | 5 | devideuse | 114 |
| chocolatier | 5 | sculpteur | 110 |
| cabaretiere | 5 | serrurier | 100 |
| restauratrice | 5 | macon | 98 |
| limonadiere | 4 | paveur | 75 |

Table F.2: Summary statistics on baseline integration proxies

| | 1886 | | | | | 1891 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | S.d. | Min | Max | N | Mean | S.d. | Min | Max |
| **Among Italians** | | | | | | | | | | |
| Lives in a mixed household | 2,367 | 0.211 | 0.408 | 0 | 1 | 2,556 | 0.156 | 0.363 | 0 | 1 |
| Household heads or spouses | 7,247 | 0.810 | 0.392 | 0 | 1 | 7,356 | 0.842 | 0.365 | 0 | 1 |
| Is a worker | 7,053 | 0.508 | 0.500 | 0 | 1 | 7,171 | 0.457 | 0.498 | 0 | 1 |
| Is a shop owner | 7,053 | 0.030 | 0.171 | 0 | 1 | 7,171 | 0.033 | 0.180 | 0 | 1 |
| **Among other foreigners** | | | | | | | | | | |
| Lives in a mixed household | 1,770 | 0.324 | 0.468 | 0 | 1 | 1,995 | 0.279 | 0.448 | 0 | 1 |
| Household heads or spouses | 5,467 | 0.762 | 0.426 | 0 | 1 | 6,121 | 0.785 | 0.411 | 0 | 1 |
| Is a worker | 5,009 | 0.332 | 0.471 | 0 | 1 | 5,844 | 0.307 | 0.461 | 0 | 1 |
| Is a shop owner | 5,009 | 0.033 | 0.179 | 0 | 1 | 5,844 | 0.036 | 0.186 | 0 | 1 |

Table F.3: Applied for naturalization: comparing mixed and non-mixed households

| | In partnership with foreigner | | | In partnership with French | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among foreigners in a partnership |
| 1891 | 0.011 | 0.005 | 0.005 | 0.026 | 0.005 | 0.002 | 0.005 |
| | (0.001) | (0.000) | (0.001) | (0.007) | (0.003) | (0.003) | (0.001) |
| Italian | | | 0.010 | | | 0.018 | 0.010 |
| | | | (0.001) | | | (0.005) | (0.001) |
| 1891 × Italian | | | 0.007 | | | 0.026 | 0.006 |
| | | | (0.002) | | | (0.006) | (0.002) |
| In partnership with French | | | | | | | 0.001 |
| | | | | | | | (0.001) |
| 1891 × In partnership with French | | | | | | | -0.003 |
| | | | | | | | (0.002) |
| Italian × In partnership with French | | | | | | | 0.008 |
| | | | | | | | (0.006) |
| 1891 × Italian × In partnership with French | | | | | | | 0.021 |
| | | | | | | | (0.007) |
| Observations | 4,026 | 2,635 | 6,661 | 874 | 1,107 | 2,000 | 8,667 |
| # of municipalities | 58 | 34 | 63 | 34 | 38 | 51 | 79 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.4: Left the municipality: comparing mixed and non-mixed households

| | In partnership with foreigner | | | In partnership with French | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among foreigners in a partnership |
| 1891 | 0.017 | -0.029 | -0.030 | -0.004 | 0.043 | 0.035 | -0.030 |
| | (0.007) | (0.006) | (0.005) | (0.009) | (0.015) | (0.020) | (0.006) |
| Italian | | | -0.009 | | | 0.026 | -0.010 |
| | | | (0.008) | | | (0.016) | (0.009) |
| 1891 × Italian | | | 0.047 | | | -0.028 | 0.046 |
| | | | (0.010) | | | (0.027) | (0.010) |
| In partnership with French | | | | | | | -0.080 |
| | | | | | | | (0.013) |
| 1891 × In partnership with French | | | | | | | 0.062 |
| | | | | | | | (0.023) |
| Italian × In partnership with French | | | | | | | 0.031 |
| | | | | | | | (0.018) |
| 1891 × Italian × In partnership with French | | | | | | | -0.068 |
| | | | | | | | (0.030) |
| Observations | 4,026 | 2,635 | 6,661 | 874 | 1,107 | 2,000 | 8,667 |
| # of municipalities | 58 | 34 | 63 | 34 | 38 | 51 | 79 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.5: Applied for naturalization: comparing household heads and household employees

| | Heads, Spouses and Children | | | Workers, Domestiques and Others | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | All |
| 1891 | 0.008 | 0.002 | 0.002 | 0.004 | -0.000 | -0.000 | -0.000 |
| | (0.001) | (0.000) | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) |
| Italian | | | 0.006 | | | 0.008 | 0.007 |
| | | | (0.001) | | | (0.001) | (0.001) |
| 1891 × Italian | | | 0.006 | | | 0.003 | 0.003 |
| | | | (0.001) | | | (0.001) | (0.001) |
| Household heads | | | | | | | 0.001 |
| | | | | | | | (0.001) |
| 1891 × Household heads | | | | | | | 0.002 |
| | | | | | | | (0.000) |
| Italian × Household heads | | | | | | | -0.001 |
| | | | | | | | (0.002) |
| 1891 × Italian × Household heads | | | | | | | 0.003 |
| | | | | | | | (0.001) |
| Observations | 12,049 | 8,946 | 21,009 | 2,510 | 2,581 | 5,115 | 26,157 |
| # of municipalities | 80 | 64 | 97 | 62 | 51 | 85 | 124 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.6: Left the municipality: comparing household heads and household employees

| | Heads, Spouses and Children | | | Workers, Domestiques and Others | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | All |
| 1891 | 0.014 | -0.002 | -0.002 | 0.004 | -0.020 | -0.022 | -0.022 |
| | (0.004) | (0.005) | (0.005) | (0.009) | (0.010) | (0.011) | (0.011) |
| Italian | | | 0.004 | | | -0.063 | -0.056 |
| | | | (0.006) | | | (0.006) | (0.007) |
| 1891 × Italian | | | 0.016 | | | 0.027 | 0.025 |
| | | | (0.007) | | | (0.009) | (0.010) |
| Household heads | | | | | | | -0.127 |
| | | | | | | | (0.012) |
| 1891 × Household heads | | | | | | | 0.020 |
| | | | | | | | (0.010) |
| Italian × Household heads | | | | | | | 0.059 |
| | | | | | | | (0.010) |
| 1891 × Italian × Household heads | | | | | | | -0.009 |
| | | | | | | | (0.010) |
| Observations | 12,049 | 8,946 | 21,009 | 2,510 | 2,581 | 5,115 | 26,157 |
| # of municipalities | 80 | 64 | 97 | 62 | 51 | 85 | 124 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.7: Applied for naturalization: comparing shopkeepers and workers

| | Shopk owners | | | Workers | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Shop owners & Workers |
| 1891 | 0.020 | 0.000 | -0.002 | 0.010 | 0.002 | 0.002 | 0.002 |
| | (0.002) | (.) | (0.002) | (0.001) | (0.001) | (0.001) | (0.001) |
| Italian | | | 0.014 | | | 0.007 | 0.007 |
| | | | (0.003) | | | (0.001) | (0.001) |
| 1891 × Italian | | | 0.026 | | | 0.008 | 0.008 |
| | | | (0.006) | | | (0.001) | (0.002) |
| Shopkeeper | | | | | | | -0.004 |
| | | | | | | | (0.001) |
| 1891 × Shopkeeper | | | | | | | -0.000 |
| | | | | | | | (0.002) |
| Italian × Shopkeeper | | | | | | | 0.003 |
| | | | | | | | (0.002) |
| 1891 × Italian × Shopkeeper | | | | | | | 0.017 |
| | | | | | | | (0.007) |
| Observations | 448 | 367 | 817 | 6,825 | 3,422 | 10,269 | 11,100 |
| # of municipalities | 16 | 10 | 19 | 71 | 40 | 82 | 85 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.8: Left the municipality: comparing shopkeepers and workers

| | Shop owners | | | Workers | | | |
|---|---|---|---|---|---|---|---|
| | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Among Italians | Among other foreigners | Comparing Italians to other foreigners | Shopk owners & Workers |
| 1891 | 0.039 | 0.061 | 0.063 | 0.016 | -0.016 | -0.020 | -0.019 |
| | (0.007) | (0.007) | (0.009) | (0.006) | (0.013) | (0.014) | (0.014) |
| Italian | | | 0.016 | | | -0.010 | -0.011 |
| | | | (0.019) | | | (0.015) | (0.015) |
| 1891 × Italian | | | -0.022 | | | 0.039 | 0.040 |
| | | | (0.014) | | | (0.016) | (0.017) |
| Shopkeeper | | | | | | | -0.087 |
| | | | | | | | (0.014) |
| 1891 × Shopkeeper | | | | | | | 0.087 |
| | | | | | | | (0.021) |
| Italian × Shopkeeper | | | | | | | 0.025 |
| | | | | | | | (0.030) |
| 1891 × Italian × Shopkeeper | | | | | | | -0.093 |
| | | | | | | | (0.049) |
| Observations | 448 | 367 | 817 | 6,825 | 3,422 | 10,269 | 11,100 |
| # of municipalities | 16 | 10 | 19 | 71 | 40 | 82 | 85 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.9: Heterogeneity by previous residence

| | Left the commune | | | Applied for citizenship | | |
|---|---|---|---|---|---|---|
| | Was living in the municipality 5 years ago | Was not living in the municipality 5 years ago | All | Was living in the municipality 5 years ago | Was not living in the municipality 5 years ago | All |
| 1891 | 0.021 | -0.003 | -0.002 | 0.001 | -0.000 | -0.000 |
| | (0.005) | (0.005) | (0.005) | (0.001) | (0.000) | (0.000) |
| Italian | -0.018 | -0.012 | -0.014 | 0.011 | 0.002 | 0.003 |
| | (0.011) | (0.005) | (0.006) | (0.001) | (0.000) | (0.001) |
| 1891 × Italian | 0.029 | 0.013 | 0.012 | 0.013 | 0.003 | 0.003 |
| | (0.013) | (0.007) | (0.008) | (0.002) | (0.001) | (0.001) |
| Lived here 5y ago | | | -0.369 | | | 0.002 |
| | | | (0.012) | | | (0.001) |
| 1891 × Lived here 5y ago | | | 0.022 | | | 0.001 |
| | | | (0.006) | | | (0.001) |
| Italian × Lived here 5y ago | | | -0.002 | | | 0.008 |
| | | | (0.010) | | | (0.001) |
| 1891 × Italian × Lived here 5y ago | | | 0.022 | | | 0.010 |
| | | | (0.020) | | | (0.002) |
| Observations | 8,452 | 21,829 | 30,311 | 8,452 | 21,829 | 30,311 |
| # of municipalities | 59 | 126 | 132 | 59 | 126 | 132 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

Table F.10: Heterogeneity by whether they have domestics/workers

| | Applied for naturalisation | | | Left the municipality | | | Left the department | | | Moved to another municipality | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Has domestics or workers | Doesn't have domestics or workers | Full sample | Has domestics or workers | Doesn't have domestics or workers | Full sample | Has domestics or workers | Doesn't have domestics or workers | Full sample | Has domestics or workers | Doesn't have domestics or workers | Full sample |
| 1891 | 0.009 | 0.000 | 0.000 | -0.049 | 0.005 | 0.005 | -0.026 | 0.003 | 0.004 | -0.023 | 0.001 | 0.001 |
| | (0.001) | (0.000) | (0.000) | (0.018) | (0.005) | (0.005) | (0.013) | (0.006) | (0.006) | (0.010) | (0.005) | (0.005) |
| Italian | 0.005 | 0.006 | 0.006 | -0.093 | 0.012 | 0.013 | -0.052 | 0.004 | 0.004 | -0.040 | 0.008 | 0.009 |
| | (0.006) | (0.001) | (0.001) | (0.051) | (0.006) | (0.006) | (0.032) | (0.009) | (0.008) | (0.024) | (0.008) | (0.007) |
| 1891 × Italian | 0.002 | 0.007 | 0.007 | 0.144 | 0.006 | 0.006 | 0.099 | 0.005 | 0.004 | 0.045 | 0.002 | 0.002 |
| | (0.008) | (0.001) | (0.001) | (0.037) | (0.007) | (0.007) | (0.032) | (0.015) | (0.015) | (0.011) | (0.014) | (0.014) |
| has domestics/workers | | | -0.002 | | | 0.039 | | | 0.026 | | | 0.013 |
| | | | (0.001) | | | (0.016) | | | (0.012) | | | (0.015) |
| 1891 × has domestics/workers | | | 0.009 | | | -0.049 | | | -0.030 | | | -0.019 |
| | | | (0.002) | | | (0.015) | | | (0.019) | | | (0.011) |
| Italian × has domestics/workers | | | -0.005 | | | -0.119 | | | -0.071 | | | -0.047 |
| | | | (0.004) | | | (0.059) | | | (0.047) | | | (0.022) |
| 1891 × Italian × has domestics/workers | | | -0.004 | | | 0.132 | | | 0.093 | | | 0.039 |
| | | | (0.007) | | | (0.033) | | | (0.042) | | | (0.020) |
| Observations | 1,733 | 19,256 | 21,009 | 1,733 | 19,256 | 21,009 | 1,733 | 19,256 | 21,009 | 1,733 | 19,256 | 21,009 |
| # of municipalities | 41 | 87 | 97 | 41 | 87 | 97 | 41 | 87 | 97 | 41 | 87 | 97 |

*Notes:* Municipality fixed effects not shown. Robust standard errors clustered at the municipality level in parenthesis.

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