Xenophobic Violence and Foreigner Integration. A Framework and Evidence from 19th-century France*

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Any time, anywhere, foreigners face hostility from the native population and politicians. A large literature has attempted to understand the effect of exposure to violence on foreigners' integration efforts in their host society. Most of the existing studies, however, estimate the effect of violence for the population of foreigners present post-treatment. Using a potential outcome framework, we highlight that the identification assumptions required to estimate this quantity without bias are more stringent than implicitly assumed and likely to be violated in most contexts. We propose an alternative approach: estimating the impact of violence on the population of foreigners present pre-treatment. We show that an unbiased estimate of this estimand can be obtained with a difference-in-differences research design under the usual assumptions for identification. We illustrate how to make use of our framework with the case of violence against Italians in 19th-century France. In particular, our theoretical and empirical approaches stress the importance of considering the heterogeneous responses of foreigners to violence.

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On June 24, 1894, Sante Geronimo Caserio, an anarchist who happened to be Italian, assassinated French President Sadi Carnot in Lyon, triggering days of riots against Italians in the French city and around. On June 7, 2025, two 14-year-old boys, who happen to be Roma Romanians, allegedly attempted to rape a teenage girl in Ballymena, triggering racially motivated violence in the Northern Irish town. On July 9, 2025, three young men, who happen to be of North African origin, allegedly beat up a 68-year-old resident of Torre Pacheco, triggering days of anti-migrant unrest in the Spanish municipality. History does not repeat itself, but it rhymes.

These are just a few examples. In every epoch, in every place, waves of migration have led to tensions and conflicts between natives and foreigners. Foreigners were repeatedly lynched in 19th-century America (Seguin and Rigby, 2019) and 19th-century France (Dornel, 2004). The entry of the US in World War I came along with violence against Germans (Nagler, 1993). Foreign populations were forcibly expelled from their residence after World War 2 (Becker and Ferrara, 2019; Becker et al., 2020). Anti-Islamic hate crimes rose in the United States after the terrorist attacks of September 11, 2001 (Byers and Jones, 2007). Anti-foreigner sentiments spiked in Europe during the 2015 Refugee crisis (Benček and Strasheim, 2016; Rigoni, 2016). 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).

And this continues today. In recent years, populists have been mounting a fear of immigrants. Examples include Trump promising and implementing an illegal immigration crackdown (Politico, July 6, 2025), the Alternative für Deutschland in Germany openly campaigning on remigration (European Council on Refugees and Exiles, 2024; Oltermann, 2024), and Reform UK's leader Nigel Farage linking asylum seekers and rising crime (LBC, July 3, 2025). The issue has struck a cord with a large sway of voters not just because foreigners compete economically with natives, but also due to the fear that migrants are unable to integrate into their host society, today (Alesina and Tabellini, 2024; Margalit, 2019) as much as in the past (Tabellini, 2020).[1]

Not surprisingly, an extensive literature has tried to understand how violence against foreigners—

[1] To match our empirical setting, we use the term 'foreigner' in our paper. However the framework we study, the problems we uncover, and the solutions we propose can be applied to both immigrants and foreigners.

physical, verbal, or symbolic— affects foreigners' integration into their host society.[2] Yet, mixed effects abound. Greater hostility from natives yield both more integration (in terms of naturalisation petitions, Fouka, 2019; language, Fouka, 2019; kids' names, Fouka, 2019, Saavedra, 2021; cultural distance, Jaschke et al., 2022) and less integration (in terms of naturalisation petitions, Ferrara and Fishback, 2022; language, Steinhardt, 2018, Gould and Klor, 2016; kids' names, Fouka, 2020; cultural distance, Aksoy et al., 2023, Grewal and Hamid, 2022; intermarriage, Fouka, 2020, Gould and Klor, 2016; labor market participation, Aksoy et al., 2023, Gould and Klor, 2016, Ferrara and Fishback, 2022).

Without denying the contributions of these essential works, our contention with the literature is that we do not have a coherent framework to evaluate existing papers' findings. In this paper, we return to the fundamental question of empirical research (Lundberg et al., 2021): what is the estimand? With the use of a potential outcome framework, we argue that most previous papers, due to data limitations, attempt to estimate the effect of exposure to violence on integration on treated foreigners present post-treatment (one exception is Ferrara and Fishback, 2022). One issue we stress is that such an estimand is not easily estimated without bias. The identification assumptions for a difference-in-differences design, adopted by most existing works (besides Saavedra, 2021), to yield an unbiased estimate of the average treatment effect on the treated (ATT) for foreigners present post-treatment go beyond the parallel trends assumption. Indeed, one issue researchers face is that the exposure to violence does not just affect integration choices, but also the decisions to leave the host community (and possibly decisions to migrate to the locality). As such, identification requires an additional assumption in that the (unobservable) means of integration absent treatment in the treated group are the same whether they are calculated over the population of foreigners present in the locality post-violence or that would have been present absent violence.

This last assumption is unlikely to hold when the treatment affects the *composition* of the foreigners who live in the area. If one type of foreigners (e.g., richer foreigners) is both more likely to leave because of exposure to violence and would have been more likely to integrate absent treatment, then the bias is likely to be negative; it is likely to be positive otherwise. While

[2] We adopt the term 'integration,' which usually refers to the process by which immigrants adopt aspects of the host society and the host society accommodates immigrants' cultural diversity, over the term 'assimilation,' which is usually associated with the effort of immigrants to adopt their host society norms and cultures. However, in our case as well as in the literature, it is not always easy to distinguish between the two.

the assumption we highlight is itself untestable, we suggest that researchers should exploit heterogeneity among foreigners to get a sense of the bias in the estimation of the ATT for foreigners present post-treatment. This can be done by looking at differential exit rate triggered by exposure to violence (to determine which types are more likely to exit post-treatment) and at differential propensity to integrate in the control group (a proxy for integration efforts absent treatment) across some important dimensions of heterogeneity (e.g., occupation or wealth).

We do not simply identify issues with the existing approach taken by most works, we also suggest a solution, characterized by defining a different target population: the foreigners present *pre*-treatment. Our framework indicates that the ATT for foreigners present pre-treatment can be estimated without biases under the usual identification assumptions with a difference-in-differences research design.

We nonetheless highlight two threats to identification. Our recommended approach requires that researchers are able to track foreigners over time, from their presence pre-violence to their presence or absence post-violence. When working with historical data, this is far from guaranteed. While tremendous progress has been made in linking names across datasets (Abramitzky et al., 2021; Enamorado et al., 2019), some issues remain, notably the risk of false negatives (interpreting the lack of linkage for an individual as the individual exiting the locality). This means that we face issues in estimating the average integration of the treated and control groups in the post-treatment period. We explain how researchers can deal with this issue using external administrative data and recommend that every effect be expressed as a percentage of the mean or standard deviation. A second potential issue regards the parallel trends assumption. This assumption may be violated if (i) foreigners are heterogeneous, and (ii) foreigners with different traits are differently affected by the exposure to violence, both in their chances of leaving and integrating. We again provide a way to mitigate this risk with researchers running a difference-in-differences analysis on multiple subsamples.

We use the case of France in the 19th century to illustrate the usefulness of our theoretical framework. To empirically document how xenophobic violence affects foreigners' choices, we take advantage of a spontaneous flare of violence against Italians in the department of the Rhône following the assassination of the French President Sadi Carnot by an Italian anarchist in Lyon on June 24, 1894. Using nominative censuses for the years 1881, 1886, 1891, and 1896 and nominative naturalisation applications over the period 1886 and 1898, we look at two outcomes: exit and naturalisation. We document an increase for both. For naturalisation, we compare estimates of the ATT for the population of foreigners present pre-violence (our approach) with estimates of the ATT for the population of foreigners present post-violence (the way the literature proceeds). The estimates obtained with our approach tend to be significantly lower. One possible explanation for this difference is that the estimates of the ATT for foreigners present post-treatment are likely to be upwardly biased as our heterogeneity analysis indicates that the types of foreigners who are more likely to leave are generally those who are less likely to integrate following exposure to violence.

Overall, our paper offers two lessons for researchers studying foreigners' integration. The first regards the importance of precisely defining the estimand researchers seek to estimate and detailing the assumptions for identification. This is particularly important since foreigners always have multiple options in front of them (e.g., to leave, to do nothing, or to naturalize). The second lesson involves taking the diversity of types among foreigners seriously. The heterogeneous responses of immigrants with different characteristics to exposure to violence matter to test identification assumptions and better understand possible biases. Those varied effects also matter from a public policy perspective, especially in situations where natives are more tolerant of some forms of immigration over others, e.g., highly-skilled vs low-skilled immigrants (Alesina et al., 2023; Hainmueller and Hiscox, 2010).

1 Thinking about exposure to violence and integration

How does xenophobic violence spur or slow foreigners' integration? This looks like a simple question. Yet, it is far subtler than it seems. First, one has to ask: which foreigners are we talking about? Do we restrict attention to the foreigners who lived in the host country before the violence? Do we include all the foreigners pre- and post-violence? And what about integration? Even if we agree on a measure of integration, how do we consider foreigners who leave? Those who arrive? To illustrate the trade-offs scholars face in identifying the consequences of xenophobic violence, we describe a potential outcome framework tailored for our problem at hand.

Framework

We consider three periods $t \in \{-1, 0, 1\}$, with $t = -1$ and $t = 0$ the pre-treatment (pre-violence) periods and $t = 1$ the post-treatment (post-violence) period. Our unit of analysis is at the individual level, and we reserve the subscript i for individuals (we use individual/foreigner interchangeably). The treatment assignment of a foreigner i is denoted by $D_{i,t} \in \{0, 1\}$. For each period prior to $t = 1$, $D_{i,t} = 0$. In period 1, $D_{i,1} = 1$ denote being treated (and $D_{i,1} = 0$ indicates non-treated). Relatedly, we use $G_i \in \{0, 1\}$ to denote whether individual i belongs to the treatment group ($G_i = 1$) or the control group ($G_i = 0$). Each foreigner also either lives in location l at time t ($L_{i,t}(D_{i,t}) = 1$) or does not live in this location ($L_{i,t}(D_{i,t}) = 0$). Note that we allow for foreigners' presence in a locality l to be affected by the treatment at $t = 1$ when $D_{i,t}$ can take the value zero or one. It is important to notice that, for consistency, we are slightly abusing the usual potential outcome framework notations. We allow for variables to depend on $D_{i,t}$ even pre-treatment. However, as $D_{i,t} = 0$ for all $t \in \{-1, 0\}$, there is no potential outcome in the pre-treatment period (this also implies that we assume no treatment anticipation).

From one period to the next, an individual makes two choices. An individual i already present in location l at $t-1$ must decide whether to remain in the same locality in period t as a function of the treatment $D_{i,t}$. This is captured by the decision $L_{i,t}(D_{i,t}) \in \{0, 1\}$ for foreigners with $L_{i,t-1}(D_{i,t-1}) = 1$ (with $L_{i,t}(D_{i,t}) = 1$ then indicating staying and $L_{i,t}(D_{i,t}) = 0$ then indicating leaving). Alternatively, an individual i not present in locality in $t - 1$ ($L_{i,t-1}(D_{i,t-1}) = 0$) decides whether to come to location l between periods $t - 1$ and t: $L_{i,t}(D_{i,t}) \in \{0, 1\}$, with $L_{i,t}(D_{i,t}) = 1$ then denoting arriving in location l.

On top of this, all individuals decide how much integration effort to exert. We denote the integration effort of individual i in period $t \in \{0, 1\}$ as a function of treatment assignment $D_{i,t}$ by $Y_{i,t}(D_{i,t})$. Integration effort is binary $Y_{i,t}(D_{i,t}) \in \{0, 1\}$ with $Y_{i,t}(\cdot) = 1$ denoting high integration effort (e.g., applying for naturalisation, marrying a national from the host country, etc.) and $Y_{i,t}(\cdot) = 0$ meaning low integration effort (not applying for naturalisation, marrying an individual from their home country, etc.). Our main question of interest is then how the level of integration varies with exposure to violence. In what follows, we study different approaches to study this question.

Approach commonly used in the literature

Most of the papers in the literature, due to data restrictions or by choice, focus on individuals who are present post-treatment (Aksoy et al., 2023; Fouka, 2019, 2020; Gould and Klor, 2016; Jaschke et al., 2022; Steinhardt, 2018).[3] While those works do not define the estimand they are estimating, the most closely related quantity of interest is the effect of exposure to violence on treated individuals present after the treatment occurred. We denote by ATT^1 this estimand to mark that the target population consists of foreigners present post-treatment. This estimand is equal to:

$$ATT^1 = E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,1}(1) = 1) - E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,1}(1) = 1) \tag{1}$$

Following the literature, we assume that scholars have access to the following information. They can identify foreigners who live in a location l in two periods $t = 0$ and $t = 1$, their treatment group $G_i \in \{0, 1\}$, and their treatment status $D_{i,t} \in \{0, 1\}$ ($D_{i,1} = 1$ if and only if $G_i = 1$ and $D_{i,t} = 0$ for all $t < 1$). They also observe foreigners' presence in the locality—$\tilde{L}_{i,t} \in \{0, 1\}$—and foreigners' integration effort: $\tilde{Y}_{i,t}$ in periods $t \in \{0, 1\}$ (we use tilde accent to denote the observed location and integration effort, which may differ from the actual ones, see below). As is common, we can write the observed outcomes in terms of potential outcome framework as $\tilde{L}_{i,t} = \tilde{L}_{i,t}(0) + D_{i,t}(\tilde{L}_{i,t}(1) - \tilde{L}_{i,t}(0))$ and $\tilde{Y}_{i,t} = \tilde{Y}_{i,t}(0) + D_{i,t}(\tilde{Y}_{i,t}(1) - \tilde{Y}_{i,t}(0))$. For ease of exposition, we assume that a large sample of foreigners is available so that we can write all formulas with expectations.[4] Using a Difference-in-differences approach comparing the individuals' integration effort pre- and post-violence for the sample of foreigners present in the locality in each period, scholars recover an estimator of this form:

$$\begin{aligned} \tau^1 = \quad & E(\tilde{Y}_{i,1}(1)|G_i = 1, \tilde{L}_{i,1}(1) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 1, \tilde{L}_{i,0}(0) = 1) \\ - \quad & (E(\tilde{Y}_{i,1}(0)|G_i = 0, \tilde{L}_{i,1}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 0, \tilde{L}_{i,0}(0) = 1)) \end{aligned}$$

To highlight the assumptions required for identification, we rearrange the expressions above

[3] We discuss exceptions below, and we provide a summary of the literature in Table A.1 in the appendix.

[4] That is, in practice, in $t = 1$, scholars observe $\overline{Y}_{i,1}(D_{i,1}, G_i, \tilde{L}_{i,1}(D_{i,1}) = 1) = \frac{\sum_i \tilde{Y}_{i,1} \cdot h(G_i) \cdot \tilde{L}_{i,1}(D_{i,1})}{\sum_i h(G_i)\tilde{L}_{i,1}(D_{i,1})}$ with $h(x) = \mathbb{I}_{\{x=1\}}x + \mathbb{I}_{\{x=0\}}(1 - x)$. We, instead, use the notation $E(Y_{i,1}(D_{i,1})|G_i, \tilde{L}_{i,1}(D_{i,1}) = 1)$ to express the same quantity.

(by adding and substracting $E(\tilde{Y}_{i,1}(0)|G_i = 1, \tilde{L}_{i,1}(1) = 1)$ and $E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$). We obtain:

$$\begin{aligned}
\tau^1 = \quad & \Big[E(\tilde{Y}_{i,1}(1)|G_i = 1, \tilde{L}_{i,1}(1) = 1) - E(\tilde{Y}_{i,1}(0)|G_i = 1, \tilde{L}_{i,1}(1) = 1)\Big] \\
+ \quad & \Big[\big(E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 1, \tilde{L}_{i,0}(0) = 1)\big) \\
- \quad & \big(E(\tilde{Y}_{i,1}(0)|G_i = 0, \tilde{L}_{i,1}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 0, \tilde{L}_{i,0}(0) = 1)\big)\Big] \\
+ \quad & \Big[E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(1) = 1) - E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)\Big] \qquad (2)
\end{aligned}$$

It is immediate that three assumptions are necessary for τ^1 to be an unbiased estimate of ATT^1. The first assumption is that we can estimate the average integration of the treated in the post-treatment period of foreigners present post-treatment: $E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,1}(1) = 1) = E(\tilde{Y}_{i,1}(1)|G_i = 1, \tilde{L}_{i,1}(1) = 1)$ (and, implicitly, $E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,1}(1) = 1) = E(\tilde{Y}_{i,1}(0)|G_i = 1, \tilde{L}_{i,1}(1) = 1)$).

Second, we can impute the missing potential outcomes of the treated in the post-treatment period had they not been treated by equalling trends in the treated group absent treatment and trends in the control group. For this to be true, the parallel trends assumption must hold: $(E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 1, \tilde{L}_{i,0}(0) = 1)) - (E(\tilde{Y}_{i,1}(0)|G_i = 0, \tilde{L}_{i,1}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 0, \tilde{L}_{i,0}(0) = 1)) = 0$.

This last assumption, coupled with the estimation assumption above, however is not enough, unlike for traditional difference-in-differences setting. Indeed, if the parallel trends assumption holds, we obtain a value for the missing potential outcomes of the treated group absent treatment in term of integration effort and location decision: $E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$. However, we need instead the missing potential outcome of the treated group absent treatment in term of integration effort, but affected by the treatment when it comes to location decision: $E(\tilde{Y}_{i,1}(0)|G_i = 1, \tilde{L}_{i,1}(1) = 1)$. As such, an additional necessary assumption for unbiasedness is that the (unobservable) means of integration absent treatment in the treated group are the same whether they are calculated over the population of foreigners present in the locality post-violence or the population of foreigners that would have been present absent violence: $E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(1) = 1) = E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$.

This assumption that $E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(1) = 1) = E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$ is unlikely to hold if, for example, foreigners from the treated group who leave after exposure to treatment were less likely to integrate in the first place, in which case we generally have $E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(1) = 1) > E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$ and the researchers' estimator is upwardly biased. Inversely, if the treated foreigners who leave are more likely to exert integration effort absent treatment, researchers usually uncover a downwardly biased estimate of the ATT^1 (we formalize these results in Online Appendix D.4).

The additional assumption we highlight for identification ($E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(1) = 1) = E(\tilde{Y}_{i,1}(0)|G = 1, \tilde{L}_{i,1}(0) = 1)$) is obviously untestable since it requires observing two missing potential outcomes. Yet, if researchers have access to information about some characteristics of foreigners in their sample (e.g., their occupation or their wealth), they can devise two tests which can indicate the likely sign of the bias in their estimates (if any). To get a sense of whether the treatment affects the composition of the treated group, we can simply check whether exposure to violence yields distinct exit rates for different types of foreigners. This can be done in a difference-in-differences setting with exit by period t as the dependent variable for individuals present in the previous period for each type separately. To understand whether different types would have exerted different levels of integration effort absent treatment, we can look at the integration effort by types among the control group. If differences emerge across types in these two tests, it gives some indication that the identification assumption has high risk of not being satisfied. Further, if the two tests we briefly describe have the same sign (so that the type of foreigners who exit more because of the treatment is also the type that integrate more in the control group), the bias in the estimate is likely to be negative; it is positive if the two tests have opposite signs (i.e., the type who exits more following the treatment is the type who integrates less in the control group).

Before proposing an alternative approach, let us stress that an unbiased estimate of ATT^1 can be obtained with a different research design, data permitting. For example, Saavedra (2021) compares the names of children of Japanese-origin couples pre- and post-Pearl Harbour. Their data consist of the exit records of Japanese individuals incarcerated in internment camps (post-treatment). Their estimation strategy is a regression discontinuity design (RDD) using the date of birth of children. Hence, by doing so, both the control group (child born just before Pearl Arbour) and the treated group (child born after) belong to $L_{i,1}(1)$ and their estimator is unbiased for ATT^1 as long as the usual identification assumption for RDD applies.

Recommended estimand and estimation strategy

We argue here that a more promising quantity to estimate is the average impact of the treatment on treated foreigners present in the host country before the violence. In this case, the target population consists of foreigners present at $t = 0$ with the treatment occurring between $t = 0$ and $t = 1$. The estimand, which we label ATT^0, thus assumes the following form:

$$ATT^0 = E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,0}(0) = 1) - E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,0}(0) = 1) \tag{3}$$

Even then, it should be noted that this estimand is a weighted sum of two quantities. The effect of the treatment on the treated for those who stayed in the locality and the effect of the treatment on the treated for those who left, with the weight being the probability of staying:

$$\begin{aligned}
ATT^0 = &\Big(E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1)Pr(L_{i,1}(1) = 1|D_{i,1} = 1, L_{i,0}(0) = 1) \\
&- E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 1)Pr(L_{i,1}(0) = 1|D_{i,1} = 0, L_{i,0}(0) = 1)\Big) \\
&+ \Big(E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 0)Pr(L_{i,1}(1) = 0|D_{i,1} = 1, L_{i,0}(0) = 1) \\
&- E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 0)Pr(L_{i,1}(0) = 0|D_{i,1} = 0, L_{i,0}(0) = 1)\Big)
\end{aligned}$$

This poses an evident estimation issue, as it assumes we can track the integration effort of individuals who may relocate far from their original locality (in fact, who can even leave the host country). To address this issue, we recommend focusing on integration efforts in a specific locality. In other words, we consider the effect of exposure to violence on integration *in a given place*. Under this perspective, any individual who leaves their original location is given a low level of integration effort (i.e., $Y_{i,t}(D_{i,t}) = 0$ for $D_{i,t} \in \{0, 1\}$ if $L_{i,t}(D_{i,t}) = 0$). With this in mind, our average treatment effect on the treated, which we denote ATT^0_{pl} to mark that it is place specific, becomes:

$$\begin{aligned}
ATT^0_{pl} = &E(Y_{i,1}(1)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1)Pr(L_{i,1}(1) = 1|D_{i,1} = 1, L_{i,0}(0) = 1) \\
&- E(Y_{i,1}(0)|D_{i,1} = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 1)Pr(L_{i,1}(0) = 1|D_{i,1} = 0, L_{i,0}(0) = 1)
\end{aligned} \tag{4}$$

Two important remarks are in order when interpreting ATT^0_{pl}. The first is that the probability of staying ($Pr(L_{i,1}(D_{i,1}) = 1|D_{i,1}, L_{i,0}(0) = 1)$) is to be understood as staying in the same location. The second is that the smaller the geographical unit, the higher the chances that foreigners are leaving. Hence, the probability of staying is smaller for communes than for counties, for counties than for states, for states than for nations. In other words, the effect of violence on integration at the local level mechanically increases with the breadth of the geographical unit the researcher works with. In what follows, we restrict attention to ATT^0_{pl}.

To estimate ATT^0_{pl}, we need to identify foreigners who live in a location l in two periods $t = -1$ and $t = 0$, their treatment group $G_i \in \{0, 1\}$, and their treatment status $D_{i,t} \in \{0, 1\}$ (with the same property as above). For those individuals, we need to observe their location in the next period—$\tilde{L}_{i,t+1} \in \{0, 1\}$, with $\tilde{L}_{i,t+1} = \tilde{L}_{i,t+1}(0) + D_{i,t+1}(\tilde{L}_{i,t+1}(1) - \tilde{L}_{i,t+1}(0))$—and their integration effort—$\tilde{Y}_{i,t+1} \in \{0, 1\}$ with $\tilde{Y}_{i,t+1} = \tilde{Y}_{i,t+1}(0) + D_{i,t+1}(\tilde{Y}_{i,t+1}(1) - \tilde{Y}_{i,t+1}(0))$, recalling that we attribute a value of zero (low effort) to all individuals who left. We can then estimate the ATT^0_{pl} with a difference-in-differences research design on the sample of foreigners present in location l in period $t \in \{-1, 0\}$.[5] This estimation strategy yields the following estimator:

$$\begin{aligned}\tau^0_{pl} =& E(\tilde{Y}_{i,1}(1)|G_i = 1, \tilde{L}_{i,0}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 1, \tilde{L}_{i,-1}(0) = 1) \\ &-\big(E(\tilde{Y}_{i,1}(0)|G_i = 0, \tilde{L}_{i,0}(0) = 1) - E(\tilde{Y}_{i,0}(0)|G_i = 0, \tilde{L}_{i,-1}(0) = 1)\big) \qquad (5)\end{aligned}$$

As we focus on the population of foreigners present in the previous period (period 0 for integration effort in $t = 1$, period -1 for integration effort in period $t = 0$), we do not have to worry about population changes induced by the exposure to violence, unlike the approach commonly used in the literature. This means that our empirical strategy yields an unbiased estimate of the ATT^0_{pl} when two usual identification assumptions are met: (i) we can estimate the average integration of the treated in the post-treatment period of foreigners present pre-treatment, and (ii) the parallel trends assumption holds. Our approach, hence, relies on fewer and less stringent assumptions for identification, making it a better choice to evaluate the effect of exposure to violence on integration. Yet, it is important to stress some potential issues with those two identification assumptions.

[5] Like us, Ferrara and Fishback's (2022) target population is foreigners present pre-treatment (though they focus on those who leave rather than stay in a US country). Unlike us, as far as we can tell, Ferrara and Fishback only use a single census pre-treatment (with the treatment being the entry of the US into World War I). As a result, they do not have the required data to perform a difference-in-differences analysis.

The first assumption ($E(Y_{i,1}(1)|D_i = 1, L_{i,0}(0) = 1) = E(\tilde{Y}_{i,1}(1)|G_i = 1, \tilde{L}_{i,0}(0) = 1)$) presupposes that we can perfectly track individuals who remain in the same locality. The validity of this assumption depends on the data scholars have access to. It is likely to hold with administrative data (as in Govind et al., 2024), but less likely to be satisfied when linking over time is done with less precise datasets, such as historical censuses (as we and many scholars do). With imperfect linkage, the location we observe in the dataset may simply be correlated with individuals' actual location. Denoting $L_{i,t} \in \{0, 1\}$ the actual location of foreigner i, $\tilde{L}_{i,t}$ then satisfies: $Pr(\tilde{L}_{i,t} = 1|L_{i,t} = 1, L_{i,t-1} = 1) = \eta \in [0, 1]$ and $Pr(\tilde{L}_{i,t} = 1|L_{i,t} = 0, L_{i,t-1} = 1) = \gamma \in [0, 1]$ so that $1 - \eta$ is the false negative rate and $1 - \gamma$ is the false positive rate (note we assume that those rates are independent of the treatment group and status of individual i).

Assuming no false negative ($\gamma = 1$), the integration effort for an individual i we observe will have the following features. For a proportion η of individuals, we correctly observe their actual level of integration, denoted $Y_{i,t}$: $\tilde{Y}_{i,t} = Y_{i,t}$ as $\tilde{L}_{i,t} = L_{i,t}$. For the rest, due to linking problems, we attribute an integration value of zero—$\tilde{Y}_{i,t} = 0$ as $\tilde{L}_{i,t} = 0$—instead of their correct integration level which can be zero or one if they stayed in the locality—-$Y_{i,t} \in \{0, 1\}$ if $L_{i,t} = 1$. As a result, we underestimate the actual mean integration level by a factor of η: $E(\tilde{Y}_{i,t}|G_i, \tilde{L}_{i,t-1} = 1) = \eta E(Y_{i,t}|G_i, L_{i,t-1} = 1)$. This directly implies that our estimator for the ATT^0_{pl} is also downwardly biased by the same factor.

There are three ways to solve this issue. For scholars not interested in estimates per se, the simplest solution is to express results as percentage of the mean and/or standard deviation (for the treated group pre-treatment). Indeed, the measurement issue affects the first two moments in the same way as the estimate, so that the biases cancel out. If estimates matter, then to recover an unbiased estimate of the effect of exposure to violence on foreigners present pre-treatment, one needs to find an estimate of η and divide estimates by this factor. To do so, researchers can follow one of two routes. First, they can sample a number of individuals pre-treatment and manually check whether those individuals are present post-treatment as well in their data. Of course, this approach is more feasible when the geographical unit of analysis is small (town/municipality rather than county/department, county/department rather than state/region). An alternative is to use estimates from researchers in other contexts or at different points in time that do not rely on the same linkage technique. In our empirical setting below, we will present both results as percentages of the mean and standard deviation, and provide corrected estimates based on administrative data.

The parallel trends assumption, in turn, may be affected by heterogeneity among foreigners (this problem is not unique to our setting). We formalize the issue in Online Appendix D.5. Here, we briefly summarize when the issue arise and what can be done about it. The parallel trends assumptions holds in the presence of heterogeneity when one of two conditions are met: (i) the compositions of the treated and control groups are the same (in term of relevant characteristics for foreigners' response to violence, such as occupation or wealth) or (ii) different types of treated foreigners would have integrated at the same rate over time absent treatment.

There are reasons to doubt that the first condition is met as empirical and theoretical works highlight how groups may become the subject of violence because of their occupation (Grosfeld et al., 2020; Jha, 2013) or lack of economic integration (Dewan and Wolton, 2024) To get a sense whether the second condition holds, scholars can examine whether in the control group, different types integrate at different rates over time. If this test reveals differential trends in integration effort among the control group, scholars can then run a difference-in-differences on type-specific subsample and then compute an average according to the distribution of types among the treated group in the period preceding the treatment (the assumption, then, being that the integration of individuals depends only on their types, not on the proportion of individuals who have the same type as them).

Before turning to our empirical setting where we apply the lessons from our framework, let us briefly mention one advantage and one disadvantage of the approach we recommend. The advantage of focusing on the population of foreigners present pre-violence is that researchers can also look at another important outcome: exit from the locality, which is an extreme form of refusing to integrate. The main disadvantage is that our approach does not capture the impact of the treatment on all individuals present in the host country post-treatment. Indeed, the new arrivals between $t = 0$ and $t = 1$ are excluded from the analysis. This raises the question of whether we can obtain an unbiased estimator of the effect of the treatment on the foreigners affected by the violence and who were not present in the host country pre-treatment. In Online Appendix D, we show that this is impossible without imposing some strong assumptions on the effect of the treatment. To briefly summarize the issue, we do not have access to the population of *potential* migrants to the host country, only the foreigners who actually arrive in the locality. This means that any estimate is conditional on immigrants arriving, which generally provides a biased estimate of the relevant estimand (in Online Appendix D, we also discuss the identification assumptions necessary to obtain an unbiased estimate of the effect of exposure to violence on the treated conditional on staying in the host locality).

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 immigration. France 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).

As the number of foreigners increased, immigration became a political issue (Noiriel, 2007). In 1885, the French authorities carried out a vast study to "measure" the impact 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 targets of legislators were Italians, both due to their fast growth, causing a feeling of invasion, and to the fact that they were predominantly low-skill (Milza, 1986). Despite the cultural proximity, at least from our contemporary perspective, the integration of Italians into French society was claimed to be 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 one Italian during the *Vêpres Marseillaises* in 1881 (Liens, 1967) or the so-called Aigues-Mortes massacre 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

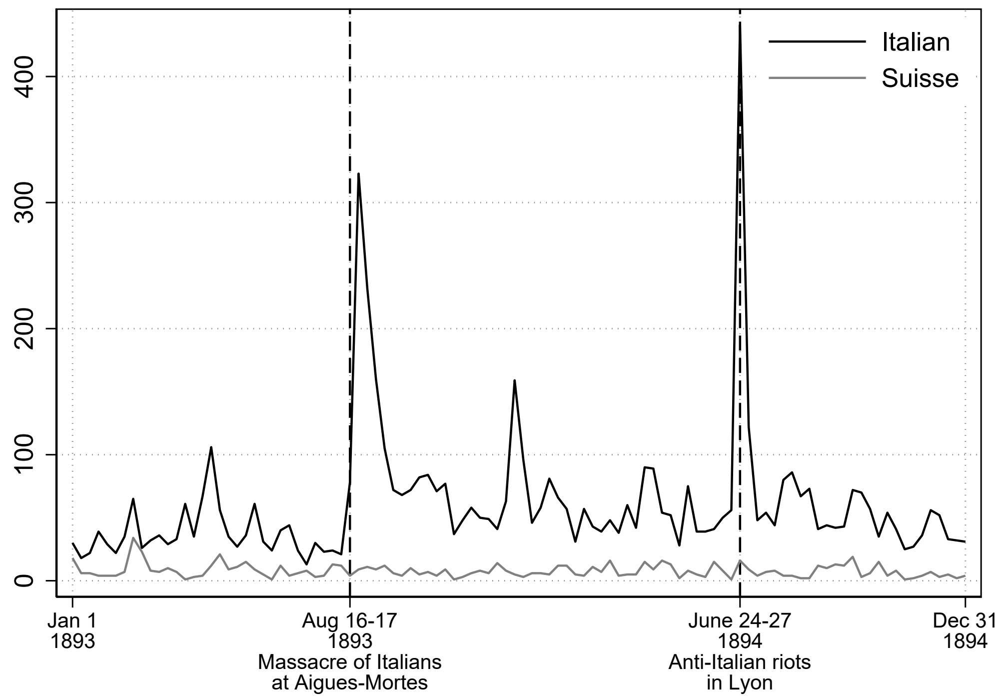
Paris in the Rhône department, to visit the *Exposition universelle, internationale et coloniale*, a world's fair including a colonial exhibition held at the Parc de la Tête d'Or. After patronizing the exhibition, Carnot participated in the banquet organized in his honor by the local chamber of commerce, 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 only 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. Sante Geronimo Caserio was an anarchist by choice and an Italian by accident of birth.

Yet, the actions of Sante Geronimo Casiero became the catalyst 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. Newspaper articles in the days following the violence indicate that damages were exceptionally high in the neighborhoods of La Guillotière, Les Brotteaux, Saint-Fons, and Vaise (e.g., the articles on the assassination of Carnot on 26 June 1894 in the Memorial de la Loire et Haute Loire and in La France de Bordeaux et du Sud Ouest). Individuals were also targeted by rioters demanding that anyone suspected of being Italian prove French citizenship. The violence was not limited to Lyon; it spread to nearby Grenoble and even briefly 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) and available on 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.[6] 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 1.

[6]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 1: Weekly number of paragraphs mentioning violent keywords and nationality groups (1893 - 1894)



Note: 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). Source: 24,080 regional newspapers published between January 1, 1893 and December 31, 1894 available on retronews.fr. See footnote 6 for the list of keywords used to identify violent events.

Two main patterns are worth noting. First, we observe two peaks in Figure 1. 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 roughly the same amount of attention, underscoring the significance 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. 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 integration. 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 30th.[7]

[7] Archive Departmental (AD) du Rhone, 4M224: *Troubles de juin 1894 ; affaire Casati ; état des indemnités réclaméespar les victimes des troubles de juin 1894 et des sommes représentantle montant approximatif des*

We are more interested in the long-lasting effect of the riots. On the one hand, some Italians may have come back. 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.[8] Observing violence may have changed the amount of trust that Italians had toward natives, and vice versa. Italians may also have feared future attacks.

When it comes to integration, our primary measure, following Fouka (2019), consists of petitions for naturalisation (petition decisions allow us to look at integration outcomes fully under the control of foreigners, unlike granting nationality, which depends on bureaucrats' decision, or intermarriage, which depends on natives' willingness to partner with foreigners). 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 naturalisation. Naturalisation was granted or denied after a moral inquiry by the public administration (Article 1 of the law on naturalisation from 3 December 1847). Naturalisation came at a cost; the new nationals and their children were subject to up to three years of compulsory military service until they reached 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). Naturalisation 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, as we noted above.

dégâts Link to inventory.

[8] 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.

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 naturalisation decrees.

Nominative census records. We primarily 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.

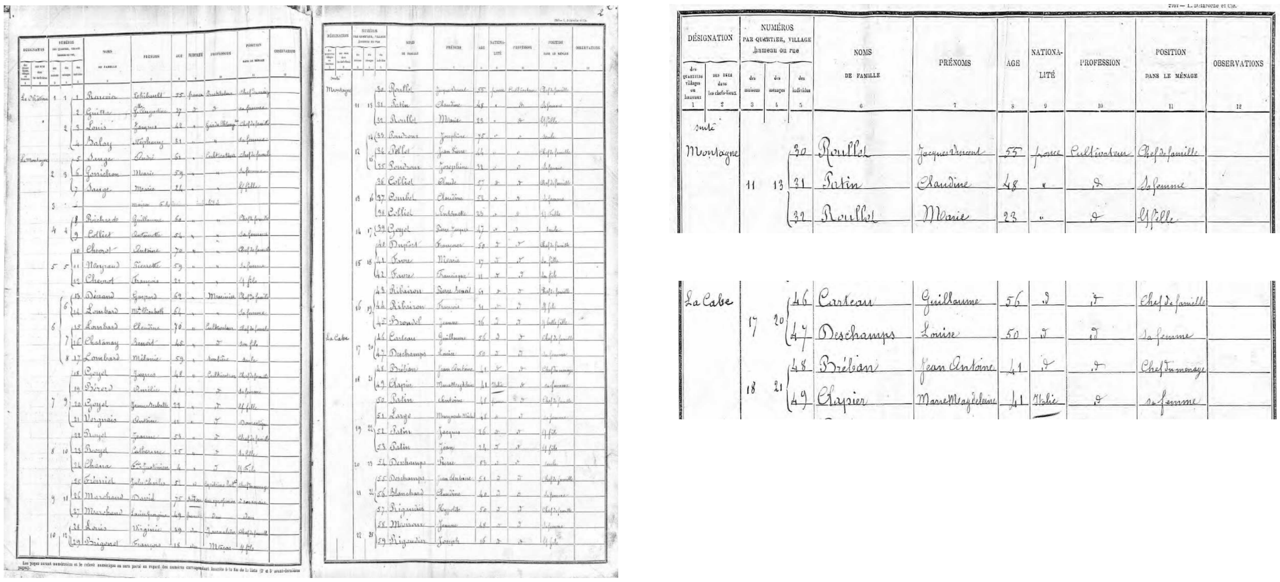
The nominative census contains the neighborhood of residence (and sometimes also the street), first and last names, nationality, age or year of birth, and occupation of all individuals living in the municipality. We can also identify members of the same household and how they relate to each other (head, spouse, child, household employees, and other relatives). As an example of our raw data, Figure 2 displays a page from the 1886 nominative census of Albigny, one of the municipalities in our sample.

These handwritten records are available for all municipalities online on the websites of the Rhône departmental archives. We hired a team of research assistants to extract indexed records of 39,477 pages, amounting to 2,317,137 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.[9] 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 equal to plus or minus 5% of the municipality-level official census counts. In practice, we drop 19 out of 798 (= 266 municipalities $\times$ 3 years) municipality-years from our sample, leaving us

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

Figure 2: Example page from nominative census records

(a) Full page (b) Sample of rows



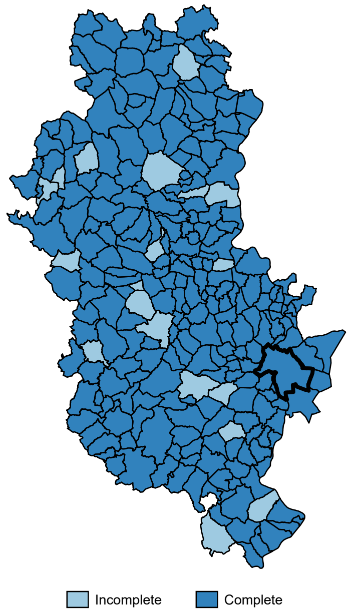
DÉSIGNATION des quartiers villages ou hameaux 1	des rues dans les chefs-lieux 2	NUMÉROS PAR QUARTIER, VILLAGE hameau ou rue: des maisons 3	des ménages 4	des individus 5	NOMS DE FAMILLE 6	PRÉNOMS 7	AGE 8	NATIONALITÉ 9	PROFESSION 10	POSITION DANS LE MÉNAGE 11	OBSERVATIONS 12
suite Montagne				30	Roullet	Jacques Vincent	55	France	Cultivateur	Chef de famille	
		11	13	31	Patin	Claudine	48	"	d°	Sa femme	
				32	Roullet	Marie	23	"	d°	Sa fille	
La Cabe		17	20	46	Carteau	Guillaume	56	d°	d°	Chef de famille	
				47	Deschamps	Louise	50	d°	d°	Sa femme	
		18	21	48	Bréban	Jean Antoine	41	d°	d°	Chef du ménage	
				49	Chapier	Marie Magdeleine	41	Italie	d°	sa femme	

Note: This figure reproduces a page from the nominative census records for the municipality of Albigny in 1886. Source: Archives departemental du Rhône.

with 248 (out of 266) municipalities for which we have near-complete individual-level data in 1886, 1891, and 1896. In Figure 3, 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 19 dropped municipalities are missing at random. Our sample comprises 2,262,584 individuals, French or foreign, in 248 municipalities over three census years, 1886, 1891, and 1896.

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 248 municipalities in our sample, we identified 32,889 of the 33,688 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 2,670 foreigners out of the 32,889. Since we cannot tie these "foreigners" to a specific nationality to study their exit decisions and naturalisation petitions, we exclude them from our analyses. Among the 30,219 foreigners with known nationality in the 1886 and 1891 censuses, which comprise our main sample, 16,668 (55%) are Italian, 8,346 are Swiss (28%),

Figure 3: Coverage of municipalities in our sample



Note: 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 in 1886, 1891, and 1896, and as incomplete otherwise.

and Germans make up another 2,109 (7%). There are only 727 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 1,377 inhabitants, 7 Italians, and four other known foreigners in 1886 (Panel B).[10] 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).

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 695,155 individuals. We do not use this census for our primary analyses as it does not contain information about the nationality of individuals. We only reserve these data for the ancillary tests below.

Nominative naturalisation decrees. Our second main data source is the official decisions

[10]In practice, out of the 248 municipalities in our sample, only 92 municipalities have at least one foreigner of known nationality in both 1886 and 1891. Excluding Lyon, these municipalities count roughly 2,200 inhabitants, 17 Italians, and 10 other known foreigners on average in 1886.

Table 1: Municipality-level summary statistics in 1886 and 1891

	1886					1891				
	N	Mean	S.d.	Min	Max	N	Mean	S.d.	Min	Max
A. All municipalities										
Total population	248	2,890	23,898	127	376,647	248	3,039	26,363	114	415,443
Number of Italians	248	34	436	0	6,864	248	33	423	0	6,663
Number of other foreigners	248	26	347	0	5,455	248	28	385	0	6,066
Number of Swiss	248	16	215	0	3,386	248	18	242	0	3,810
Number of Belgians	248	1	15	0	240	248	2	19	0	297
Number of Germans	248	2	25	0	391	248	2	24	0	384
Number of 'étranger' (excluded)	248	5	33	0	362	248	6	40	0	472
B. Excluding Lyon										
Total population	247	1,377	1,809	127	14,051	247	1,370	1,918	114	17,063
Number of Italians	247	7	30	0	301	247	6	22	0	182
Number of other foreigners	247	4	18	0	172	247	4	14	0	144
Number of Swiss	247	2	10	0	122	247	2	9	0	105
Number of Belgians	247	0	2	0	19	247	0	3	0	38
Number of Germans	247	0	2	0	17	247	0	1	0	12
Number of 'étranger' (excluded)	247	4	24	0	345	247	5	39	0	472

regarding applications for naturalisation, 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 until 1927, for instance). Decrees published between 1886 and 1898 were indexed through collaborative indexing organized by the French National Archives (project Natnum) in 2017 and made available on the French national archives' online reading room (www.siv.archives-nationales.culture.gouv.fr). We provide an extract of the naturalisation decrees published on January 1, 1887, both in their original form (Figure 4) and as presented in the online reading room (Figure E.1).

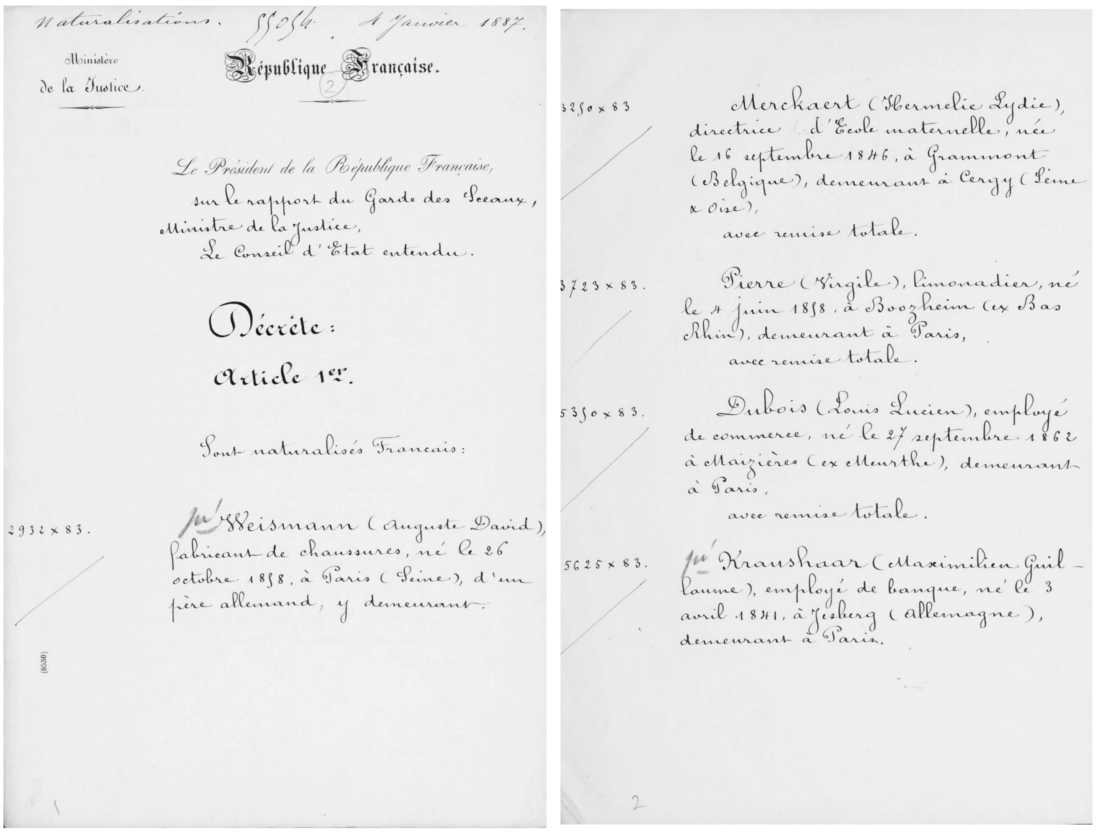
Among the 102,556 published decrees between 1886 and 1898, we uncover 1,807 decrees from applicants residing in the Rhône department. Among those, 889 individuals applied for naturalisation, 367 applied for admission to legal residency, 436 for reintegration into French nationality after marrying a foreigner, 111 for reintegration into French nationality from Alsatians, and four have 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 naturalisation were denied out of 647 (18%) decided between 1892 and 1898.[11]

Importantly, we can also recover the year an application was made using the application ref-

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

erence number as described by Weil (2002) (see the reference number on the left margin of Figure 4: it indicates that the application was made in 1883 by a ×83). As the exact date of application is missing from the data, we collected this information separately for the years 1893 and 1894, around the Aigues-Mortes massacre and the assassination of President Carnot, by consulting naturalisation books at the French National Archives.[12]

Figure 4: Example of naturalisation decrees (original document)


Naturalisations. 55054. 4 Janvier 1887.

Ministère de la Justice.

République Française.

(2)

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

Décrète:

Article 1er.

Sont naturalisés Français:

2932x83. Mr Weismann (Auguste David), fabricant de chaussures, né le 26 octobre 1858, à Paris (Seine), d'un père allemand, y demeurant.

3250x83 Merckaert (Hermelie Lydie), directrice d'École maternelle, née le 16 septembre 1846, à Grammont (Belgique), demeurant à Cergy (Seine et Oise),
avec remise totale.

3723x83. Pierre (Virgile), limonadier, né le 4 juin 1858, à Boozheim (ex Bas Rhin), demeurant à Paris,
avec remise totale.

5350x83. Dubois (Louis Lucien), employé de commerce, né le 27 septembre 1862 à Maizières (ex Meurthe), demeurant à Paris,
avec remise totale.

5625x83. Mr Kraushaar (Maximilien Guillaume), employé de banque, né le 3 avril 1841, à Jesberg (Allemagne), demeurant à Paris.

Note: This figure reproduces the first page of the original document of the naturalisation decrees published on January 4, 1887. Source: French national archives' online reading room (www.siv.archives-nationales.culture.gouv.fr)

In our analyses, we focus on naturalisation *applications*, which is a foreigner's choice, and we do not look at whether a foreigner becomes French, which could be affected by changes in behaviors from bureaucrats making decisions. We use these two sets of data—nominative censuses and naturalisation applications—to understand how exposure to violence affects assimilation and exit.

[12]BB/29/829 to BB/29/837. Figure E.2 in the Online Appendix provides an example page from these naturalisation books.

4 Empirical strategy

To estimate the effect of exposure to violence on treated foreigners present pre-treatment, we employ a difference-in-differences strategy at the individual level. Our sample comprises all foreigners of known nationality i present in municipality m in census year $t \in \{1886, 1891\}$. For exit, we define an indicator variable equal to one if individual i is not found in the same municipality in the census at $t+5$. To link individuals across censuses, we match individuals based on (i) first and last names and (ii) year of birth, (iii) blocking on municipality using the `fastlink` algorithm developed by Enamorado et al. (2017). We code an individual in census t as found in the census at $t+5$ if there is at least one match for this individual with a match probability above .85. We provide more details on our linking procedure in Supplemental Appendix E. We discuss our results when we do not block on municipality and look at exiting the department in Online Appendix G.1.

Table 2 presents estimates for the proportion of foreigners in the 1886 and the 1891 census who left their municipality of residency within 5 years. Our estimated exit rate is relatively high: around 69% of foreigners in the 1886 census left the municipality of residence by 1891. On top of this, we find that around 62% of foreigners left the department over the same period (Appendix Table B.1). 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. This linking rate for a one-year difference leads to a linking rate of 59% across 5 years, so an exit rate of 41%. Given that the raw data Enamorado et al. (2019) use are digital, whereas we use handwritten data that are indexed, we believe our linking rate is again within the right bounds. We also find 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 (Online Appendix Table E.1). This is reassuring, as this category tends to be more mobile.

For naturalisation application, we define an indicator variable that takes the value of one if an individual i present in municipality m in census t is linked to an individual who resides in the same municipality m and applied for naturalisation between t and $t+5$. To do so, we link

Table 2: Exit: Probability estimates by subgroup

	1886			1891		
	N	Mean	S.d.	N	Mean	S.d.
Left the commune by t+5						
Among all foreigners	15,016	0.686	0.464	15,221	0.673	0.469
Among Italians	8,510	0.681	0.466	8,170	0.675	0.468
Among other foreigners	6,506	0.693	0.461	7,051	0.670	0.470
Among Swiss	3,962	0.659	0.474	4,388	0.634	0.482

foreigners of known nationality to the list of 886 individuals who applied for French nationality (we drop 3 observations for which the year of birth is missing) using the same strategy as for exit. For each individuals in the census, we keep the best match with a match probability greated than .85, and code an individual as applying for naturalisation within 5 years if the year of application of the matched individual applied falls within 5 year of the census year (i.e., between 1886 and 1891 for individuals in the 1886 census, and between 1891 and 1896 for individuals in the 1891 census). Table 3 provides estimates of the probability of applying for naturalisation for different nationality groups.

Table 3: Naturalisation application: Probability estimates by subgroup

	1886			1891		
	N	Mean	S.d.	N	Mean	S.d.
Applied for naturalisation by t+5						
Among all foreigners	15,034	0.005	0.068	15,239	0.006	0.074
Among Italians	8,524	0.006	0.075	8,181	0.009	0.094
Among other foreigners	6,510	0.003	0.058	7,058	0.002	0.041
Among Swiss	3,964	0.003	0.053	4,394	0.002	0.043

For both exit and naturalisation applications, we measure the effect of exposure to violence on foreigners present pre-treatment using the following model:

$$Y_{imt} = \delta_m + \alpha_1 \text{Post-violence}_{imt} + \alpha_2 \text{Italian}_i + \beta^0_{pl} \text{Post-violence}_{imt} \times \text{Italian}_i + \epsilon_{imt} \tag{6}$$

The explanatory factors we include on the left-hand side of Equation 6 are: municipality fixed effects (δ_m), an indicator for whether individual i is present in municipality m in the census year 1891 (Post-violence$_{imt}$),[13] an indicator for whether individual i is marked as Italian in

[13]Note that we appropriately use post-violence for individuals present in the 1891 census because our outcome

one of the censuses (Italian$_i$) as well as the interaction of Post-violence$_{imt}$ and Italien$_i$. The coefficient β^0_{pl} is, thus, the regression equivalent for τ^0_{pl} (which we computed in Equation 5) and it corresponds to our estimate of ATT^0_{pl} (the average treatment effect on the treated for foreigners present pre-treatment). Finally, ϵ_{imt} corresponds to the error term, and we run all our regressions with standard errors clustered at the municipality level.

One problem we face is that we are very unlikely to recover the true exit rate of foreigners from their commune or department. Our linkage is very likely to suffer from a high rate of false negatives as names may be poorly transcribed by census enumerators (Abramitzky et al., 2021, suggest that this can account for 45% of link failures), individuals change names, or marry (for women). As we noted in Section 1, this downwardly biases our estimates. To approximate the true exit rate, we take advantage of the *Enquête des 3000 familles* used by Daudin et al. (2016). Compiled by the French national statistics (INSEE), this dataset tracks French families whose names start with the letters T, R, or A over a very long period of time using archives from Register Offices all over France. Daudin et al. (2016) estimate that only 17.3% of individuals left their department in a 50-year period (1861-1911). From this, we estimate that the true positive rate is approximately $\eta = 63\%$.[14] We also present all our findings relative to the mean and the standard deviations since the downward bias induced by false negatives cancels out when we look at those ratios.

Our framework also highlighted threats to the parallel trend assumptions for our naturalisation application outcome, and we will return to those in Section 6. For now, we display the number of applications by year in Figure 5a and by month in Figure 5b. A quick observation of the two graphs suggests no discernible pre-trends (especially in the months around the events in Lyon). The lack of difference between Italians and other nationals in the months prior to June 1894 also suggests that other concomittant events, such as the massacre of Italians in Aigues-Mortes on 16 and 17 August 1893 in the department of the Gard, south of the Rhône department, are unlikely to drive our findings.[15] We cannot exclude that the control group is also affected by

variable is measured by $t+5$ (hence, by 1896, i.e., post-violence).

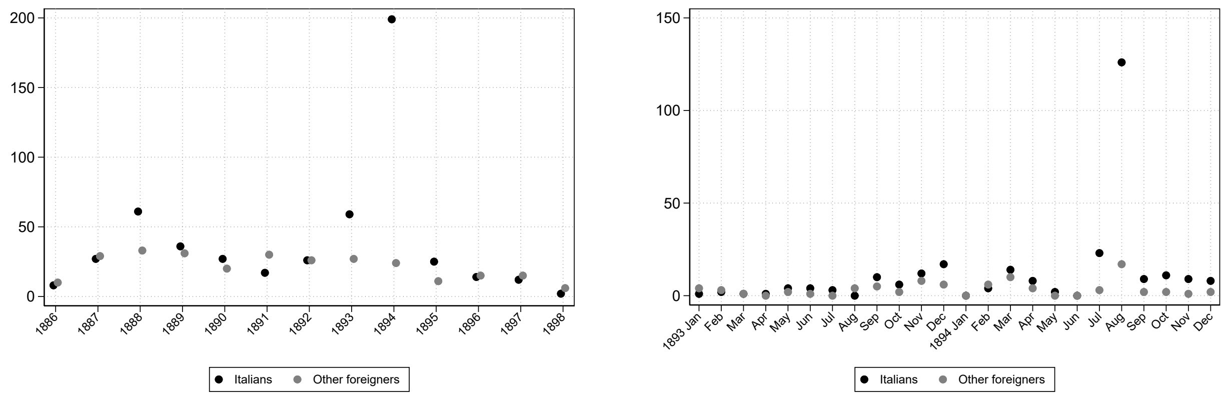
[14] To do so, we use a sample of French individuals and estimate their exit rate using our linking procedure. The exit rate from the department for this sample is 0.378 (see Table B.1). Denote $Pr(\tilde{L}_{i,t}(D_{i,t}) = 0|L_{i,t}(D_{i,t}) = 0, L_{i,t-1}(D_{i,t-1}) = 1) = CPr(L_{i,t}(D_{i,t}) = 0|L_{i,t-1}(D_{i,t-1}) = 1)$. By the law of total probability, $Pr(\tilde{L}_{i,t}(D_{i,t}) = 0|L_{i,t-1}(D_{i,t-1}) = 1) = (1-\eta)Pr(L_{i,t}(D_{i,t}) = 1|L_{i,t-1}(D_{i,t-1}) = 1) + Pr(L_{i,t}(D_{i,t}) = 0|L_{i,t-1}(D_{i,t-1}) = 1)$, with $Pr(\tilde{L}_{i,t}(D_{i,t}) = 0|L_{i,t-1}(D_{i,t-1}) = 1) = 0.378$ and $Pr(L_{i,t}(D_{i,t}) = 0|L_{i,t-1}(D_{i,t-1}) = 1) = (17.3\%)^{1/10} = 1 - Pr(L_{i,t}(D_{i,t}) = 1|L_{i,t-1}(D_{i,t-1}) = 1)$ from Daudin et al. (2016).

[15] Another concern is a change in laws in 1889 that required all foreigners residing in France to renew their

Figure 5: Number of applications for naturalisation in the Rhône department (1893-1894)

(a) Application to naturalisation by year

(b) Application to naturalisation by month



Note: This figure shows the number of naturalisation applications in the Rhône department by year (panel a) and by month (panel b) for Italians (black) and other foreigners (gray). The yearly data comes from the decrets de naturalisation and the monthly data from the naturalisation registers described in the main text.

the exposure to violence (though we saw little evidence that Swiss nationals were targeted, see Figure 1). In any case, if other foreigners were affected by the exposure to violence to a lower extent than Italians, this would tend to bias downward our estimates.

For exit, we cannot test for pre-trends because the 1881 census did not record respondents' nationality. We use names from the 1886 census to predict nationality in all censuses. Running our difference-in-differences model on the data with attributed nationalities (Equation 6), we find little evidence of pre-trends (see Table F.1 in the Supplemental Material).

We also compare our findings with those emerging from the usual strategy employed by the literature (i.e., attempting to estimate the impact of exposure to violence on the treated for foreigners present post-treatment). We can only look at this estimate for the naturalisation applications. To do so, we count the number of individuals with nationality $n \in$\{Italians, other nationalities\} who have applied for naturalization in a municipality m between census $t-1$ and census t and attribute this number to census t. To make the results more comparable with our individual analysis, we divide the number of naturalisation applications by individuals with nationality n in the commune by the number of nationals from group n present in census t in the same locality. We denote this proportion y_{nmt} where n is the nationality group (Italians or

status every 5 years or naturalize. This policy change, however, affects *all* foreign nationals. Hence, it is taken into account by our census dummy.

others). We then employ the following specification for census years 1891 and 1896:

$$y_{nmt} = \delta_m + \alpha_1 \text{Post-violence}_t + \alpha_2 \text{Italian}_n + \beta^1 \text{Post-violence}_t \times \text{Italian}_n + \epsilon_{nmt} \tag{7}$$

As before, δ_m corresponds to municipal fixed effects. The dummy variable Post-violence$_t$ now corresponds to a dummy for the *1896 census*. Indeed, recall that we are now considering foreigners present *post*-violence. Italian$_n$ is an indicator variable equal to one if the nationality group we consider is Italian nationals. Our coefficient of interest is β^1, the regression equivalent for τ^1 (whose formula is given in Equation 2). We cluster standard errors at the municipal level. Further, as y_{nmt} is an average over the population of foreigners of group n in commune m, we also weight observations by its associated foreigner populations (using analytical weights).

5 Exposure to violence, exit, and naturalisation applications

Table 4 displays the results from Equation 6 for exit and applications to naturalisation in, respectively, columns (1) and (2), and the results for naturalisation applications from Equation 7 in column (3).

Three patterns are worth noting. First, exposure to violence increases both exit and applications for naturalisation. Second, the point estimates in columns (1) and (2) are small. Exposure to violence for foreigners present pre-treatment increases exit by 2.5 percentage points (pp) (after correction) and applications for naturalisation by 1 pp (after correction). There are three main reasons for this. First, the violence was short-lived, meaning that individuals did not have too much impetus to change their situation in one way or another. Second, recall that integration effort should be understood as integration effort *within a locality*. In our case, the locality is a commune, a very small geographical unit, meaning that many individuals who move within the department (e.g., to go to Lyon) are counted as zero even if they end up naturalizing. As such, our estimates can be understood as a lower bound on the impact of violence. Third, naturalisation only brings small benefits, and some costs in the form of military service obligation for them (if young enough) and their children, meaning that few individuals

Table 4: Effect of violence on exit and naturalisation application

	$\hat{\beta}^0_{pl}$ (Equation 6)		$\hat{\beta}^1$ (Equation 7)
	(1) DV: Exit	(2) DV: Naturalisation application	(3) DV: Naturalisation application
Post-violence	-0.017	-0.002	-0.002
	(0.005)	(0.000)	(0.004)
Italian	-0.017	0.002	0.001
	(0.006)	(0.000)	(0.002)
Post-violence × Italian	0.016	0.005	0.023
	(0.006)	(0.000)	(0.005)
Observations	30,203	30,203	349
# of municipalities (clusters)	129	129	113
Target Population	Pre-violence	Pre-violence	Post-violence
Mean DV for Italians pre-treatment	0.681	0.006	0.018
Corrected estimate	.025	.008	
Effect size (mean)	2.32%	86.45%	129.82%
Effect size (std)	3.38%	6.51%	121.46%

Note: In column (1), the dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t+5$. In column (2), the dependent variable is an indicator variable equal to one if a foreigner present in the census t is matched to a naturalisation applicant in the same municipality who applied for naturalisation between t and $t+5$. In both columns, the variable *Post-violence* takes the value one if the foreigner is present in the 1891 census. In column (3), the dependent variable is the number of foreigners in municipality m who applied between $t-5$ and t divided by the number of foreigners in municipality m at time t. In column (3), the variable *Post-violence* takes the value one for the 1896 census. In columns (1) and (2), the coefficient of *Post-violence* × *Italians* is an estimate of β^0_{pl} using Equation 6. In column (3), the coefficient of Post-violence × Italians is an estimate of β^1 using Equation 7, where we weight observations by the number of foreigners in the municipality. The mean of the DV is calculated for Italians pre-treatment: the proportion of Italians present in the 1886 census who exited by 1891 in column (1), who applied for naturalisation by 1891 in column (2) and the proportion of Italians who have applied to naturalisation between 1886 and 1891 in the 1891 census in column (3). The standard deviations are calculated on the same samples. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses. Results in columns (1) and (2) are presented in more detail in Tables F.2 and F.3.

have an incentive to apply for French nationality.[16] This last observation also implies that the impact of exposure to violence is actually quite large when we compare with the mean

[16] Even nowadays, the number of foreigners who apply for naturalisation is relatively low. For the UK, around 200,000 foreigners apply each year for naturalisation according to the UK government relative to 4.9 million 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 naturalisation according to Homeland Security compared to a foreign population of 25 million according to estimates from the Pew Research Center. These numbers arise in a context in which obtaining the nationality of the host country provides way more advantages (in terms of permanent residency and security) today than at the end of the 19th century.

of the dependent variable among the treated (Italians) pre-treatment. Indeed, our estimate corresponds to a 86% increase relative to the mean, around 6.5% of a standard deviation.

One last pattern worth noting regards the comparison of the estimates from columns 2 and 3 of Table 4. The approach using Equation 7 yields a much larger estimate (almost three times as large even after applying our correction) and a much larger effect, both relative to the mean or to the standard deviation. Even though our estimate from column (2) may be a lower bound on the effect of exposure to violence, the difference is striking. This raises the possibility, as we noted in section 1, that β^1 may yield an upwardly biased estimate of the ATT for the foreigners post-treatment. Overall, we believe that the results from column (3) should be interpreted with caution and analyses below will reinforce this conclusion.

Is the rise in exit and in applications for naturalisation due to the exposure to violence? Two ancillary tests in Supplemental Appendix G.1 suggest so. First, we compare the effects in places where we know violence occurred to the effects in place where we have no evidence of violence (which does not mean that violence did not happen there). We find that for exit, the point estimates are significantly higher (in a statistical sense) in localities where violence has happened (see Table F.4), we see no clear difference for naturalization (see Table F.5). Second, we provide suggestive evidence that our findings are not driven by anarchists being expelled from the country (to find potential anarchists in our sample, we use a list established in 1892 for the Rhône department, see Figure F.1 in the Online Appendix). While possible anarchists are slightly more likely to exit (though the difference is not statistically significant), they are much less likely to naturalize (see Table F.6 for details).

In Online Appendix G.2, we look at an additional measure of integration: having a French spouse. In Table G.3, we find that exposure to violence has no effect on the chances of an Italian present pre-treatment being in a partnership with a French. This contrasts dramatically with the estimate from the specification used in the literature which yields a significant and positive effect of exposure to violence on intermarriage.

6 Heterogeneous responses to exposure to violence

So far, we have considered that Italians and other foreigners are each a homogeneous group. This is unlikely to be the case. In our setting, Italians and other foreigners vary in multiple dimensions, which can be related to their propensity to integrate. In 1891, Italians living in the Rhône department were less well integrated socially (less likely to live in a mixed household) and economically (less likely to have domestics or employees, a sign of wealth, and more likely to be a worker) than other foreigners (Appendix Table B.2).

In this section, we study how two different individual characteristics — their occupation (workers or shopkeepers) and their wealth (having employees or not) — affect their responses to xenophobic violence. We identify shopkeepers/workers based on the occupation they declare in the census. We use the fact that all individuals in a household are included in the census, with their function clearly stated and each household clearly separated, to separate heads of household (heads and spouses) who have employees from those who do not.

The analysis is obviously important from a substantive angle as it provides a better understanding of the consequences of xenophobic violence. It also matters from an inference perspective, as we have noted in our theoretical framework how heterogeneity (i) poses a threat to the parallel trends assumption for the approach we recommend and (ii) can provide some idea of the sign of the bias in the research design employed in the literature.

We first look at exit. In columns (1) to (3) of Table 5, we consider heterogeneity by occupation. Column (1) reproduces column (1) of Table 4 for the sample of immigrants, which we can classify as shopkeeper or worker. Column (2) looks at the effect of exposure to violence for the subsample of workers, and column (3) for the subsample of shopkeepers. Workers, it appears, are more likely to exit following exposure to violence than shopkeepers who seem more likely to stay (the coefficient is negative coefficient). In Table F.7 in the Online Appendix, we show, however, that the difference in treatment effects is not statistically significant at the conventional 5% level between the two types of foreigners. In columns (4) to (6), we consider heterogeneity by wealth. Column (4) looks at all foreigners for all heads of household, column (5) looks at the subsample which does not have employees, and column (6) at the subsample of immigrants with employees. Here, we see that relatively wealthier individuals are the most likely to exit (Table F.8 in the Online Appendix indicates that the difference in treatment effects between wealthy and less wealthy individuals is statistically significant).

Table 5: Effect of violence on exit: Heterogeneity by occupation and wealth

	Heterogeneity by occupation			Heterogeneity by wealth		
	(1) All foreigners	(2) Among workers	(3) Among shopkeepers	(4) All foreigners	(5) With no employee	(6) Has employees
Post-violence	-0.018	-0.024	0.043	-0.016	-0.008	-0.063
	(0.012)	(0.014)	(0.009)	(0.006)	(0.005)	(0.015)
Italians	-0.006	-0.010	0.016	-0.018	-0.011	-0.080
	(0.015)	(0.015)	(0.020)	(0.006)	(0.005)	(0.030)
Post-violence × Italians	0.031	0.038	-0.015	0.029	0.020	0.099
	(0.014)	(0.016)	(0.015)	(0.009)	(0.008)	(0.036)
Observations	11,067	10,249	806	13,939	12,591	1,330
# of municipalities (clusters)	83	81	19	91	84	39
Population	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence
Mean DV for Italians pre-treatment	0.688	0.688	0.688	0.608	0.676	0.608
Effect size (mean)	4.56%	5.55%	-2.12%	4.80%	3.01%	16.27%
Effect size (std)	6.78%	8.25%	-3.15%	5.97%	4.35%	20.22%
Mean naturalization control gr. in 1891		0.001	0.000		0.003	0.000
p-value of = means			0.633			0.044

Note: In all columns, the dependent variable is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t+5$. The coefficient of *Post-violence* × *Italian* is an estimate of $\hat{\beta}^0_{pl}$ using Equation 6. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

The last two rows of Table 5 also display the proportion who naturalized in the control group between the 1891 census and the 1896 census (i.e., for other foreigners present in the 1891 census) to get an indication of the sign of the possible bias in the estimates of the average treatment effect on the treated for foreigners present post-treatment (ATT^1). As we noted in Section 1, the sign is likely to be positive (upward bias in real value) if the group that exits most following the violence is the group that has the lowest rate of integration absent violence (as proxied by the mean in the control group). This is what Table 5 indicates, at least when we look at the heterogeneity by wealth (columns (5) and (6)). Hence, the very large effect we found in column (3) of Table 4 using the approach commonly employed in the literature (very large compared to our approach in column (2)) may overestimate how foreigners react to exposure to violence.[17]

[17] In Table G.4, we also compare the mean in the control group in 1891 (other foreigners present in the 1891 census) when it comes to intermarriage. We observe that for this group, shopkeepers are more likely to be married to a French than workers, those who have no employees more likely than those who have employees (and those who are heads of household more than those who are simply employees). Again, we observe that the group that exits the less following treatment is more likely to be integrated in the control group, this time according to our intermarriage measure. This indicates again that the positive effect of the treatment on intermarriage we find using the usual approach from the literature (Table G.3) is likely upwardly biased.

Table 6 looks at differences in naturalisation applications according to foreigners' characteristics. The structure is the same as Table 5. Comparing the two tables, we see that when it comes to occupation, the type of Italians who exit the least following the treatment (shopkeepers) is the type who applies the most for naturalisation following exposure to violence (Table F.7 in the online appendix highlights that the difference in the effect of the treatment on applications to naturalization is statistically significant between workers and shopkeepers). We do not find a similar finding for heterogeneity based on wealth: wealthy individuals exhibit both a higher exit rate and a higher naturalization application rate following violence, though the difference in treatment effects for naturalization application is small (0.7pp) and not statistically significant (see Table F.8 in the Online Appendix).

Table 6: Effect of violence on naturalisation application: Heterogeneity by occupation and wealth

	Heterogeneity by occupation			Heterogeneity by wealth		
	(1) All foreigners	(2) Among workers	(3) Among shopkeepers	(4) All foreigners	(5) With no employee	(6) Has employees
Post-violence	-0.004	-0.003	-0.015	-0.003	-0.002	-0.007
	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)	(0.001)
Italian	0.004	0.005	0.005	0.004	0.005	-0.007
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Post-violence × Italian	0.009	0.007	0.022	0.008	0.008	0.016
	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)	(0.002)
Observations	11,067	10,249	806	13,939	12,591	1,330
# of municipalities (clusters)	83	81	19	91	84	39
Target population	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence
Mean DV for Italians pre-treatment	0.008	0.008	0.014	0.009	0.009	0.000
Corrected estimate	.014	.012	.035	.013	.012	.026
Effect size (mean)	107.98%	94.29%	155.94%	97.30%	80.02%	.%
Effect size (std)	9.66%	8.37%	18.64%	9.09%	7.81%	.%

Note: In all columns, the dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation applicant who applied for naturalisation between t and $t+5$. The coefficient of *Post-violence* × *Italian* is an estimate of β^0_{pl} using Equation 6. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

In Appendix C, we look at other dimensions of heterogeneity: whether an individual is a head of household ('head' or 'spouse') or an employee and whether an individual lives in a mixed household with both a French and a foreigner among the heads of household, based again on the information reported in the census. For these two dimensions of heterogeneity, we constantly find that the group that exits less in response to violence is also the group that tends to naturalize more (see Appendix Tables C.1 and C.2).

The presence of heterogeneity in the consequences of violence on both exit and naturalisation may present a threat to the parallel trend assumption, which does not hold when trends in integration effort for the treated group absent treatment vary by types as we noted above. To test for this, we first look at the increase in naturalization applications over time by types in the control group. In Table C.4, we observe slight differences between types (across all dimensions of heterogeneity), but the signs are not consistent over time. The absence of clear difference in trends suggests that our overall difference-in-differences estimate from Table 4 is unlikely to suffer from biases. To find some confirmation for this, in Table 7, we recompute our estimate $\hat{\beta}^0_{pl}$ using weighted average of our estimates from Table 6 (Columns (2) and (3) and Columns (5) and (6)), with the weight determined by the proportion of the different types among Italians in the 1886 census. The exercise suggests again that our overall estimates is unlikely to exhibit significant bias (Table C.3 confirms the findings using other dimensions of heterogeneity).

Table 7: Effect of violence on naturalisation application: Summary of results.

	Heterogeneity by occupation		Heterogeneity by wealth	
	(1)	(2)	(3)	(4)
	$\hat{\beta}^0_{pl}$	Weighted $\hat{\beta}^0_{pl}$	$\hat{\beta}^0_{pl}$	Weighted $\hat{\beta}^0_{pl}$
Post-violence × Italian	0.014	0.013	0.013	0.013
	(0.001)	(0.001)	(0.002)	(0.002)

Note: This table provides estimates of β^0_{pl} both using Equation 6 and by weighting estimates within subgroups using the proportion of Italians in each category in 1886 (see Table B.2 for more details on these proportions). In columns (1) and (2), the sample is restricted to foreigners with a known occupation. In columns (3) and (4), the sample restricted to foreigners who are heads of households. The estimates and standard errors based on occupations and wealth assume that the estimates for each group are independent.

7 Discussion and conclusion

How do foreigners react when exposed to xenophobic violence? We provide a framework to think about this question. We argue that the literature, in most cases, seeks to estimate the effect of violence on integration for treated immigrants present post-treatment. The problem is that a classical difference-in-differences design, used in most existing works, yields an unbiased estimate of this estimand only under some stringent assumptions, which go beyond the usual parallel trend and estimation assumptions. We offer an alternative approach: examining the consequences of exposure to violence on integration for foreigners present *pre*-treatment and explaining why a difference-in-differences research design yields unbiased estimates under the assumptions scholars commonly have to make. Why this difference? The key issue is that foreigners have multiple choices when deciding how to react after observing violence. They can integrate more, or they can simply leave. The option to exit can change the composition of the group of treated foreigners, which makes the control group a poor comparison if the target population is foreigners present post-violence. This issue is likely to arise when foreigners vary in important dimensions that shape their willingness to exit after violence and to integrate in the absence of violence.

Using the case of France in the 19th century and the anti-Italian riots in Lyon and around in June 1894, we apply the lessons from our theoretical framework. We document that Italians are more likely to exit *and* to apply for naturalisation following the violence. When it comes to our integration outcome, our approach, with the foreigners present pre-treatment as the target population, yields small, but statistically significant estimates. In contrast, the strategy employed by the literature, with foreigners present post-treatment as the target population, renders estimates almost three times as large as those we obtain with our preferred approach. One explanation for this large difference is that, in our setting, the usual strategy used in the literature is likely to yield upwardly biased estimates. Indeed, we document that the type of foreigners who exit the most following violence are those who tend to apply for naturalisation at a lower rate absent treatment (wealthy individuals in particular). On top of this, we also show that the types who are most likely to leave because of violence tend to be those who are least likely to exert integration effort following the riot (workers, employees, Italians not in mixed partnership).

We believe that taking the heterogeneity among foreigners seriously can help re-evaluate the consequences of violence. Violence is rarely random; it is seldom without purpose. In our historical case, French workers appear to have targeted their Italian competitors as we already noted (see also Berthoud, 1969, p.30), and our analysis suggests it may have worked. Italian workers were more likely to leave and less likely to apply for naturalisation because of the violence. More recently, in Ballymena, Northern Ireland, the July 2025 riots targeted one particular group, Roma Romanians, again with some success, to the satisfaction of most inhabitants (Guardian, 19 July 2025). Understanding the roots of the violence should serve as a guide to better comprehend its consequences.

While we look at violence in this paper, our framework can prove useful for contemporary immigration policies, a highly controversial topic nowadays. 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). Other measures have attempted to increase the benefits of assimilation. This includes, among others, language training and compulsory civic courses (Emeriau et al., 2025), 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). All those policies are likely to have a dual effect. They will encourage some immigrants to leave (or to stay) and others to integrate more into the host society. The responses of immigrants will probably depend on some underlying traits that would have affected foreigners' propensity to integrate absent policy and to stay post-treatment. As such, just like for violence, attempts to understand the impact of the policy on the population of foreigners present post-treatment with a difference-in-differences research design (like in Abdelgadir and Fouka, 2020; Emeriau et al., 2025; Govind and Sirugue, 2023) are likely to yield biased estimates, with the sign of the bias a function of the heterogeneous responses of immigrants. A more promising approach for future policy evaluation is to track foreigners present pre-policy and study their response, just like we did for violence.[18]

[18] An alternative approach would be to use a different research design, such as a regression discontinuity design, as in Dahl et al. (2022). Data limitations, however, make a difference-in-differences approach the preferred and often only available option for researchers.

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Appendix

A Summary of the literature

In the next table, we summarize the literature on the impact of violence on integration. The table should be read as our best understanding of existing works. The last column harmonizes the findings in the sense that all effects are relative to integration. Hence, a negative sign indicates that treated foreigners integrate less post-violence, a positive sign that they integrate more.

Table A.1: Summary of the literature

Paper	Data	Setting	Violence	Target population	Empirical strategy	Outcome	Effect of violence on integration
Aksoy et al. (2023)	Survey	Refugees in Germany in 2016-18	Local animosity (via Tweets)	Present post-treatment	OLS using random assignment	Being employed	- (sig.)
	"	"	"	"	"	Earnings	- (sig.)
	"	"	"	"	"	Integration index	- (sig.)
Ferrara and Fishback (2022)	Observational	Germans in 1880-1940 USA	Hostility (measured by casualty in the war)	Present post-treatment	DiD	German population	- (sig.)
	"	Germans in 1930	"	Present pre-treatment	OLS	Distance moved	- (sig.)
	"	"	"	"	"	Americanized name	- (sig.)
	"	"	"	"	"	Switch to agriculture job	- (sig.)
	"	"	"	"	"	Not being naturalized	- (sig.)
Fouka (2019)	Observational	Germans and other foreigners in 1930 US census	Entry of USA in WWI	Present post-treatment	DiD	Foreign name	+ (sig.)
	"	Germans and other foreigners 1911-25 in 4 US states	"	"	"	Applying for naturalisation	+ (sig.)
	"	"	"	"	"	Name changes	+ (sig.)
Fouka (2020)	Observational	Germans in 1930 USA	States banning teaching of German language	Stayers	DiD	German names	- (sig.)
	"	"	"	"	"	Intermarriage	- (sig.)
	"	Germans during WWII USA	"	Present post-treatment	"	WWII volunteering	- (sig.)
Gould and Klor (2016)	Observational	Muslims in USA in 2000-10	Anti-Muslim hate-crime post-9/11	Present post-treatment	DiD	Intra-marriage	- (sig.)
	"	"	"	"	"	Fertility	- (sig.)
	"	"	"	"	"	Women's labor force participation	- (sig.)
	"	"	"	"	"	Language proficiency	- (sig.)
Jaschke et al. (2022)	Survey	Refugees in Germany 2016-18	Index of hostility	Present post-treatment	OLS using random assignment	Cultural proximity index	+ (sig.)
	"	"	"	"	"	Views on gender equality	0
	"	"	"	"	"	Religiosity	+
	"	"	"	"	"	Employment (relative to natives)	- (sig.)
	"	"	"	"	"	Earnings relative to native	+
Saavedra (2021)	Observational	Japanese after WWII USA	Hostility due to Pearl Harbour	Present post-treatment	RDD	Americanized name	+ (sig.)
Steinhardt (2018)	Survey	Turkish and Southern European immigrants in 1991-93 Germany	Xenophobic violence against Turks	Present post-treatment	DiD	Life satisfaction	- (sig.)
	"	"	"	"	"	Return intentions	- (sig.)
	"	"	"	"	"	Language skills	- (sig.)
	"	"	"	Present-pre-treatment	"	Exit	- (sig.)

B Descriptive statistics

Table B.1: Exit: Probability estimates by subgroup (full table)

	1886			1891		
	N	Mean	S.d.	N	Mean	S.d.
Left the department by t+5						
Among a sample of French	197,007	0.373	0.484	196,109	0.378	0.485
Among all foreigners	15,016	0.619	0.486	15,221	0.607	0.488
Among Italians	8,510	0.608	0.488	8,170	0.600	0.490
Among other foreigners	6,506	0.633	0.482	7,051	0.616	0.486
Among Swiss	3,962	0.596	0.491	4,388	0.574	0.495
Left the commune by t+5						
Among a sample of French	197,007	0.544	0.498	196,109	0.539	0.498
Among all foreigners	15,016	0.686	0.464	15,221	0.673	0.469
Among Italians	8,510	0.681	0.466	8,170	0.675	0.468
Among other foreigners	6,506	0.693	0.461	7,051	0.670	0.470
Among Swiss	3,962	0.659	0.474	4,388	0.634	0.482

Table B.2: Summary statistics on integration proxies at baseline

	1886					1891				
	N	Mean	S.d.	Min	Max	N	Mean	S.d.	Min	Max
Among Italians										
Lives in a mixed household	2,700	0.217	0.412	0	1	2,785	0.172	0.377	0	1
Has employees	3,932	0.056	0.231	0	1	3,849	0.062	0.241	0	1
Head of household (HH)	7,261	0.875	0.331	0	1	7,332	0.895	0.307	0	1
Is a worker	3,578	1.000	0.000	1	1	3,260	1.000	0.000	1	1
Is a shopkeeper	7,042	0.030	0.171	0	1	7,117	0.033	0.179	0	1
Among other foreigners										
Lives in a mixed household	2,006	0.355	0.479	0	1	2,241	0.305	0.461	0	1
Has employees	2,994	0.150	0.357	0	1	3,196	0.139	0.346	0	1
Head of household (HH)	5,491	0.820	0.384	0	1	6,101	0.833	0.373	0	1
Is a worker	1,662	1.000	0.000	1	1	1,785	1.000	0.000	1	1
Is a shopkeeper	5,009	0.033	0.178	0	1	5,805	0.035	0.184	0	1

Note: This table provides summary statistics on the main dimension of heterogeneity used in the paper.

C Other results

Table C.1: Exit: Heterogeneity by position in household (HH) and household type

	Heterogeneity by position in household			Heterogeneity by household's type		
	(1) All foreigners	(2) Among employees	(3) Among heads of HH	(4) All foreigners	(5) In not mixed households	(6) In mixed households
Post-violence	-0.015	-0.035	-0.009	-0.024	-0.034	-0.001
	(0.005)	(0.006)	(0.005)	(0.006)	(0.004)	(0.014)
Italian	-0.016	-0.062	0.001	0.004	-0.001	0.020
	(0.006)	(0.009)	(0.006)	(0.011)	(0.008)	(0.017)
Post-violence × Italian	0.021	0.033	0.018	0.031	0.044	-0.029
	(0.006)	(0.013)	(0.007)	(0.013)	(0.009)	(0.033)
Observations	26,150	3,646	22,473	17,363	13,607	3,752
# of municipalities (clusters)	122	81	96	85	65	59
Population	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence
Mean DV for Italians pre-treatment	0.672	0.756	0.658	0.659	0.664	0.640
Effect size (mean)	3.16%	4.32%	2.77%	4.73%	6.66%	-4.51%
Effect size (std)	4.51%	7.60%	3.85%	6.58%	9.36%	-6.02%
Mean naturalization control gr. in 1891		0.001	0.002		0.002	0.002
p-value of = means			0.572			0.955

Note: The dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t+5$. All coefficients come from estimating Equation 6. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

Table C.2: Naturalisation application: Heterogeneity by position in household (HH) and household's type

	Heterogeneity by position in household			Heterogeneity by household's type		
	(1) All foreigners	(2) Among employees	(3) Among heads of HH	(4) All foreigners	(5) In not mixed households	(6) In mixed households
Post-violence	-0.002	-0.001	-0.002	-0.001	0.000	-0.006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Italian	0.003	0.002	0.003	0.004	0.004	0.005
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)
Post-violence × Italian	0.005	-0.001	0.005	0.005	0.004	0.009
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.005)
Observations	26,150	3,646	22,473	17,363	13,607	3,752
# of municipalities (clusters)	122	81	96	85	65	59
Target population	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence	Pre-violence
Mean DV for Italians pre-treatment	0.006	0.003	0.006	0.007	0.006	0.012
Corrected estimate	.007	-.001	.008	.008	.006	.014
Effect size (mean)	76.09%	-26.35%	82.04%	77.45%	63.50%	72.04%
Effect size (std)	5.88%	-1.52%	6.53%	6.30%	4.73%	8.07%

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation applicant who applied for naturalisation between t and $t + 5$. All coefficients come from estimating Equation 6. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

Table C.3: Applications for naturalisation: Summary of results

	Heterogeneity by position in household		Heterogeneity by household's type	
	(1)	(2)	(3)	(4)
	$\hat{\beta}^0_{pl}$	Weighted $\hat{\beta}^0_{pl}$	$\hat{\beta}^0_{pl}$	Weighted $\hat{\beta}^0_{pl}$
Post-violence × Italian	0.007	0.007	0.008	0.007
	(0.001)	(0.001)	(0.001)	(0.002)

Note: This table provides estimates of β^0_{pl} both using Equation 6 and by weighting estimates within subgroups using the proportion of Italians in each category in 1886 (see Table B.2 for more details on these proportions). In columns (1) and (2), the sample is restricted to foreigners with a known position in the household. In columns (3) and (4), the sample restricted to foreigners who live in multi-person households. The estimates and standard errors based on occupations and wealth assume that the estimates for each group are independent.

Table C.4: Applications for naturalisation among other foreigners: The effect of different dimensions of heterogeneity

	(1) Occupation	(2) Domestics	(3) Position	(4) Mixed partnerships
Shopkeeper	0.008			
	(0.001)			
Post-violence × Shopkeeper	-0.010			
	(0.001)			
Has domestics		0.003		
		(0.000)		
Post-violence × Has domestics		-0.006		
		(0.001)		
Head of household			0.001	
			(0.000)	
Post-violence × Head of household			-0.000	
			(0.000)	
In a mixed household				0.006
				(0.001)
Post-violence × In a mixed household				-0.006
				(0.001)
Observations	3,787	6,163	11,554	7,244
# of municipalities (clusters)	44	60	84	57
Target population	Pre-violence	Pre-violence	Pre-violence	Pre-violence

Note: We run the following regression for the sample of other foreigners: $Y_{imt} = \delta_m + \alpha_1 \text{Census } 1891_{imt} + \alpha_2 \theta_i + \tau \text{Census } 1891_{imt} \times \theta_i + \epsilon_{imt}$. The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation applicant who applied for naturalisation between t and $t+5$. δ_m is a municipality fixed effect, Census 1891_{imt} a dummy equal to one if individual i is present in locality m in census t, θ_i is a relevant heterogeneity dimensions (see column heads). Standard errors are clustered at the municipality level.

Supplementary Information For Online Publication

D Framework: Additional results

In this section, we present some additional results derived from our framework presented in Section 1. We first highlight the difficulty of measuring the effect of exposure to violence on the integration of foreigners not present pre-treatment and for foreigners who decided to stay. We then introduce heterogeneity in our framework. We use this extended framework to first formalize the possible bias in τ^1 (Equation 2) and the possible issues with the parallel trend assumptions for the approach we recommend. Throughout, we assume that researchers face no linking problems. That is, we assume that researchers observe $L_{i,t} \in \{0,1\}$ and $Y_{i,t} \in \{0,1\}$ for each individual in their sample with $L_{i,t} = L_{i,t}(0) + D_{i,t}(L_{i,t}(1) - L_{i,t}(0))$ and $Y_{i,t} = Y_{i,t}(0) + D_{i,t}(Y_{i,t}(1) - Y_{i,t}(0))$.

D.1 Foreigners not present pre-treatment

Here, we look at another possible quantity of interest: the effect of exposure to violence on the integration of treated foreigners who were not present pre-treatment and could have moved to the host country (i.e., to a location within the host country). We denote this estimand by ATT_{pl}^{out} to highlight that we focus on individuals living elsewhere pre-treatment. The formula for this estimand is:

$$\begin{aligned} ATT_{pl}^{out} =& E(Y_{i,1}(1)|D_{i,1}=1, L_{i,0}(0)=0) - E(Y_{i,1}(0)|D_{i,1}=1, L_{i,0}(0)=0) \\ =& E(Y_{i,1}(1)|D_{i,1}=1, L_{i,0}(0)=0, L_{i,1}(1)=1)Pr(L_{i,1}(1)=1|D_{i,1}=1, L_{i,0}(0)=0) \\ &- E(Y_{i,1}(0)|D_{i,1}=1, L_{i,0}(0)=0, L_{i,1}(0)=1)Pr(L_{i,1}(0)=1|D_{i,1}=1, L_{i,0}(0)=0) \end{aligned} \tag{D.1}$$

This estimand presents a clear difficulty to researchers, even if considering integration in a given location. Indeed, we do not have access to the relevant population. We never observe the potential pool of foreigners who could have migrated to the host country or to the locality, but did not. As such, $Pr(L_{i,t} = 1|L_{i,0} = 0)$ is never observed. Instead, with the data available (i.e., new arrivals in period $t = 0$ and $t = 1$ and their integration efforts), researchers can estimate with a difference-in-differences research design the following quantity:

$$\begin{aligned}\tau_{pl}^{out} =& E(Y_{i,1}(1)|G_i = 1, L_{i,0}(0) = 0, L_{i,1}(1) = 1) - E(Y_{i,0}(0)|G_i = 1, L_{i,-1}(0) = 0, L_{i,0}(0) = 1) \\ & - \big(E(Y_{i,1}(0)|G_i = 0, L_{i,0}(0) = 0, L_{i,1}(0) = 1) - E(Y_{i,0}(0)|G_i = 0, L_{i,-1}(0) = 0, L_{i,0}(0) = 1)\big)\end{aligned}$$

In other words, the main issue is that we can only estimate the effect of violence on integration for foreigners who were not present pre-treatment only conditional on those individuals arriving in the host country. The estimator τ^{out} is an unbiased estimate of ATT_{pl}^{out} only if all the migrants who could possibly immigrate to the host country actually do: $Pr(L_{i,t} = 1|L_{i,t-1} = 0) = 1$. This is a strong assumption unlikely to be satisfied in most contexts. Indeed, Friebel et al. (2013) documents that intentions to emigrate from Mozambique to South Africa are affected by xenophobic violence in the potential host country.

An alternative solution would be to estimate the effect of the treatment on the treated conditional on arriving. In the next subsection, looking at the mirror case (conditional on staying), we show that such approach also requires some specific identification assumptions to obtain an unbiased estimate of this conditional effect.

D.2 Conditional on staying: estimand and estimation assumptions

The estimand conditional on staying assumes the following form:

$$\begin{aligned}ATT_{pl}^{stay} =& E(Y_{i,1}(1)|L_{i,0}(0) = 1, D_{i,1} = 1, L_{i,1}(1) = 1) \\ & - E(Y_{i,1}(0)|L_{i,0}(0) = 1, D_{i,1} = 1, L_{i,1}(1) = 1)\end{aligned}$$

This quantity is equivalent to the natural direct effect in Acharya et al. (2016) when, here, we look at foreigners present in the locality after exposure to violence ($L_{i,1}(1) = 1$).

Using a difference-in-differences analysis where the sample is the foreigners present pre-treatment who stayed in the country, we recover the following estimator:

$$\begin{aligned}\tau_{pl}^{stay} =& \big(E(Y_{i,1}(1)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1) - E(Y_{i,0}(0)|G_i = 1, L_{i,-1}(0) = 1, L_{i,0}(0) = 1)\big) \\ & -\big(E(Y_{i,1}(0)|G_i = 0, L_{i,0}(0) = 1, L_{i,1}(0) = 1) - E(Y_{i,0}(0)|G_i = 0, L_{i,-1}(0) = 1, L_{i,0}(0) = 1)\big)\end{aligned}$$

After rearranging, this can be rewritten as:

$$\begin{aligned}
\tau_{pl}^{stay} =& E(Y_{i,1}(1)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1) - E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1) \\
&+ \big(E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 1) - E(Y_{i,0}(0)|G_i = 1, L_{i,-1}(0) = 1, L_{i,0}(0) = 1) \\
-& E(Y_{i,1}(0)|G_i = 0, L_{i,0}(0) = 1, L_{i,1}(0) = 1) - E(Y_{i,0}(0)|G_i = 0, L_{i,-1}(0) = 1, L_{i,0}(0) = 1)\big) \\
&+ E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1) - E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 1)
\end{aligned}$$

The assumptions needed for identification have a similar flavour as for the estimation of the effect of exposure to violence for foreigners present in the locality post-treatment. We need to assume that (i) we can estimate the average integration of the treated post-treatment, (ii) the parallel trend assumption holds, and importantly (iii) the average integration effort of the treated group absent treatment is the same in the sample who stayed after the treatment and the sample who would have stayed absent treatment ($E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(1) = 1) - E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, L_{i,1}(0) = 1)$). As we have noted in the main text, there are reasons to believe that this third assumption is unlikely to hold and researchers can also get an idea of the sign of the bias by performing the tests we recommended using heterogeneity among foreigners. Overall, the identification assumptions are very similar for τ^1 and τ_{pl}^{stay} because both suffer from the same issue. The estimand in both cases conditions on post-treatment variables which cannot be properly accounted for in the data.

D.3 Introducing heterogeneity

We know introduce heterogeneity in our setting. We suppose that each individual i is characterized by a type $\theta_i \in \{0, 1\}$. We assume that the type can affect all decisions: staying, arriving, and integration effort. Hence, we amend our notation such that the location decision is $L_{i,t}(D_{i,t}, \theta_i) \in \{0, 1\}$ and the integration effort is $Y_{i,t}(D_{i,t}, \theta_i) \in \{0, 1\}$. We give a natural ordering to types so that high-type individuals ($\theta_i = 1$) have a higher propensity to exert high integration effort than low-type individual: for any $D_{i,t} \in \{0, 1\}$, $E(Y_{i,t}(D_{i,t}, \theta_i)|\theta_i = 1) > E(Y_{i,t}(D_{i,t}, \theta_i)|\theta_i = 0)$ (with the same being true for other conditional expectations). We also suppose that the type of each individual is observable by the researchers in their dataset.

D.4 Bias in estimates of the ATT for foreigners present post-treatment

On top of the estimation and parallel trend assumptions, we noted in the main text that a difference-in-differences approach yield an unbiased estimate of ATT^1 only if $E(Y_{i,1}(0)|G_i = 1, L_{i,1}(1) = 1) = E(Y_{i,1}(0)|G_i = 1, L_{i,1}(0) = 1)$. Assuming that only types and treatment status affect integration effort, using the heterogeneity introduced above, we can write these two expectations as:

$$
\begin{aligned}
E(Y_{i,1}(0)|G_i = 1, L_{i,1}(1) = 1) =& Pr(\theta_i = 1|G_i = 1, L_{i,1}(1) = 1)E(Y_{i,1}(0, \theta_i)|\theta_i = 1) \\
&+ Pr(\theta_i = 0|G_i = 1, L_{i,1}(1) = 1)E(Y_{i,1}(0, \theta_i)|\theta_i = 0) \\
E(Y_{i,1}(0)|G_i = 1, L_{i,1}(0) = 1) =& Pr(\theta_i = 1|G_i = 1, L_{i,1}(0) = 1)E(Y_{i,1}(0, \theta_i)|\theta_i = 1) \\
&+ Pr(\theta_i = 0|G_i = 1, L_{i,1}(0) = 1)E(Y_{i,1}(0, \theta_i)|\theta_i = 0)
\end{aligned}
$$

The two expectations are equal if one of two conditions are met. Condition (i): among treated foreigners, the proportion of high type is the same when we look at foreigners present post-violence ($Pr(\theta_i = 1|G_i = 1, L_{i,1}(1) = 1)$) and those who would have been present absent violence ($Pr(\theta_i = 1|G_i = 1, L_{i,1}(0) = 1)$). Condition (ii): High and low types have the same propensity to integrate absent violence: $E(Y_{i,1}(0)|\theta_i = 1) = E(Y_{i,1}(0)|\theta_i = 1)$ (which we exclude by assumption). From the formulas above, it is clear that if there are more high types among treated foreigners after treatment ($Pr(\theta_i = 1|G_i = 1, L_{i,1}(1) = 1) > Pr(\theta_i = 1|G_i = 1, L_{i,1}(0) = 1)$), then $E(Y_{i,1}(0)|G_i = 1, L_{i,1}(1) = 1) > E(Y_{i,1}(0)|G_i = 1, L_{i,1}(0) = 1)$, so that the bias in the estimates of ATT^1 is positive as explained in the main text. The reverse holds true when $Pr(\theta_i = 1|G_i = 1, L_{i,1}(1) = 1) < Pr(\theta_i = 1|G_i = 1, L_{i,1}(0) = 1)$.

In general, condition (i) is unlikely to be met when the treatment effect on exit is higher for one of the two types. Further, it is generally the case that $Pr(\theta_i = 1|G_i = 1, L_{i,1}(1) = 1) <$ $(>) Pr(\theta_i = 1|G_i = 1, L_{i,1}(0) = 1)$ if high type exits more (less) than low type following the treatment. To see this, we need to introduce a few additional notation. Let $N_1^1(\theta, D)$ be the number of individuals of type $\theta \in \{0, 1\}$ who live in the locality in period 1 given treatment $D \in \{0, 1\}$. Let $N_0^1(\theta)$ and $N_0^0(\theta)$ be the number of individuals of type θ in period 0 who, respectively, live in the locality and do not live in the locality and could possibly immigrate to it. Let $e(\theta, D)$ be the exit rate of individuals of type θ given treatment D. Let ρ be the immigration rate of individuals outside the locality. We assume for simplicity that it is independent of the treatment and type, but results would remain the same if there is a small dependence or the effect is the same as for exit.

The proportion of high-type individuals living in the locality as a function of D is then:

$$\alpha_1(D) = \frac{(1-e(1,D))N_0^1(1) + \rho N_0^0(1)}{(1-e(1,D))N_0^1(1) + \rho N_0^0(1) + (1-e(0,D))N_0^1(0) + \rho N_0^0(0)} = \frac{1}{1 + \frac{(1-e(0,D))N_0^1(0)+\rho N_0^0(0)}{(1-e(1,D))N_0^1(1)+\rho N_0^0(1)}}$$

Hence, the proportion of high type is higher post-treatment ($\alpha_1(1) > \alpha_1(0)$) if and only if $\frac{(1-e(0,1))N_0^1(0)+\rho N_0^0(0)}{(1-e(1,1))N_0^1(1)+\rho N_0^0(1)} < \frac{(1-e(0,0))N_0^1(0)+\rho N_0^0(0)}{(1-e(1,0))N_0^1(1)+\rho N_0^0(1)}$. After rearranging, this yields:

$$\begin{aligned}&(1-e(0,1))N_0^1(0)(1-e(1,0))N_0^1(1) + \rho N_0^0(0)(e(1,1)-e(1,0))N_0^1(1)\\ &< (1-e(0,0))N_0^1(0)(1-e(1,1))N_0^1(1) + \rho N_0^0(1)(e(0,1)-e(0,0))N_0^1(0)\end{aligned}$$

Denote $e(0,1) = e(0,0) + \gamma(0)$ and $e(1,1) = e(1,0) + \gamma(1)$ so that $\gamma(\theta)$ is the effect of the treatment on the exit rate for individuals with type $\theta \in \{0,1\}$. We assume $\gamma(\theta) > 0$ which is usually the case in our data. We then can rearrange the inequality as:

$$\begin{aligned}&-\gamma(0)(1-e(1,0)) + \frac{\rho N_0^0(0)}{N_0^1(0)}\gamma(1) < -\gamma(1)(1-e(0,0)) + \frac{\rho N_0^0(1)}{N_0^1(1)}\gamma(0)\\ \Leftrightarrow &\frac{\gamma(0)}{\gamma(1)} > \frac{1-e(0,0)}{1-e(1,0)} + \frac{\rho N_0^0(0)}{N_0^1(0)(1-e(1,0))} - \frac{\rho N_0^0(1)}{N_0^1(1)(1-e(1,0))}\frac{\gamma(0)}{\gamma(1)}\end{aligned}$$

Under the assumption that exit rates are relatively similar across types absent treatment so that $\frac{1-e(0,0)}{1-e(1,0)} \approx 1$ and that most foreigners in a locality were present in the past so that $\frac{\rho N_0^0(1)}{N_0^1(1)(1-e(1,0))} \approx 0$ and $\frac{\rho N_0^0(0)}{N_0^1(0)(1-e(1,0))} \approx 0$, then the right-hand side of the inequality above is close to one. Then, we get that there are more high types in period 1 in the locality post-treatment than absent treatment when $\gamma(0) > \gamma(1)$ (i.e., the effect of exposure to violence on exit is higher for low-type individuals) and less high types in period 1 in the locality post-treatment than absent treatment when $\gamma(1) < \gamma(0)$ (i.e., the effect of exposure to violence on exit is higher for high-type individuals).

D.5 Heterogeneity and the parallel trend assumptions for foreigners present pre-treatment

With heterogeneity, the average treatment effect on the treated for the foreigners present pre-treatment becomes:

$$
\begin{aligned}
ATT^0_{pl} =& Pr(\theta_i = 1|D_{i,t} = 1, L_{i,0}(0) = 1)\Big(E(Y_{i,1}(1,\theta_i)|D_{i,1} = 1, L_{i,0}(0) = 1, \theta_i = 1) \\
& - E(Y_{i,1}(0,\theta_i)|D_{i,1} = 1, L_{i,0}(0) = 1, \theta_i = 1)\Big) \\
& + Pr(\theta_i = 0|D_{i,t} = 1, L_{i,0}(0) = 1)\Big(E(Y_{i,1}(1,\theta_i)|D_{i,1} = 1, L_{i,0}(0) = 1, \theta_i = 0) \\
& - E(Y_{i,1}(0,\theta_i)|D_{i,1} = 1, L_{i,0}(0) = 1, \theta_i = 0)\Big)
\end{aligned}
$$

Our difference-in-differences estimator on the sample of foreigners present in the previous period, in turn, can be rewritten as such:

$$
\begin{aligned}
\beta^0_{pl} = \sum_{j=0}^{1} \Bigg[& \Big(Pr(\theta_i = j|G_i = 1, L_{i,0}(0) = 1)E(Y_{i,1}(1)|G_i = 1, L_{i,0}(0) = 1, \theta_i = j) \\
& - Pr(\theta_i = j|G_i = 1, L_{i,-1}(0) = 1)E(Y_{i,0}(0)|G_i = 1, L_{i,-1}(0) = 1, \theta_i = j)\Big) \\
& - \Big(Pr(\theta_i = j|G_i = 0, L_{i,0}(0) = 1)E(Y_{i,1}(0)|G_i = 0, L_{i,0}(0) = 1, \theta_i = j) \\
& - Pr(\theta_i = j|G_i = 0, L_{i,-1}(0) = 1)E(Y_{i,0}(0)|G_i = 0, L_{i,-1}(0) = 1, \theta_i = j)\Big)\Bigg]
\end{aligned}
$$

The parallel trend assumption now can now be expressed as such: $\sum_{j=0}^{1}\Big[\Big(Pr(\theta_i = j|G_i = 1, L_{i,0}(0) = 1)E(Y_{i,1}(0)|G_i = 1, L_{i,0}(0) = 1, \theta_i = j) - Pr(\theta_i = j|G_i = 1, L_{i,-1}(0) = 1)E(Y_{i,0}(0)|G_i = 1, L_{i,-1}(0) = 1, \theta_i = j)\Big) - \Big(Pr(\theta_i = j|G_i = 0, L_{i,0}(0) = 1)E(Y_{i,1}(0)|G_i = 0, L_{i,0}(0) = 1, \theta_i = j) - Pr(\theta_i = j|G_i = 0, L_{i,-1}(0) = 1)E(Y_{i,0}(0)|G_i = 0, L_{i,-1}(0) = 1, \theta_i = j)\Big)\Big] = 0$. Even after supposing that the proportions of types is constant over time $Pr(\theta_i = j|G_i, L_{i,0}(0) = 1) = Pr(\theta_i = j|G_i, L_{i,-1}(0) = 1)$, the parallel trend assumption holds when one of two conditions are met: (i) the proportions of high type is the same in the treated and control groups or (ii) the change in integration between t and t+1 absent treatment does not depend on individuals' types: $E(Y_{i,1}(0)|G_i, L_{i,0}(0) = 1, \theta_i) - E(Y_{i,0}(0)|G_i, L_{i,-1}(0) = 1, \theta_i)$ is a constant for all $G_i, \theta_i \in \{0, 1\}^2$.

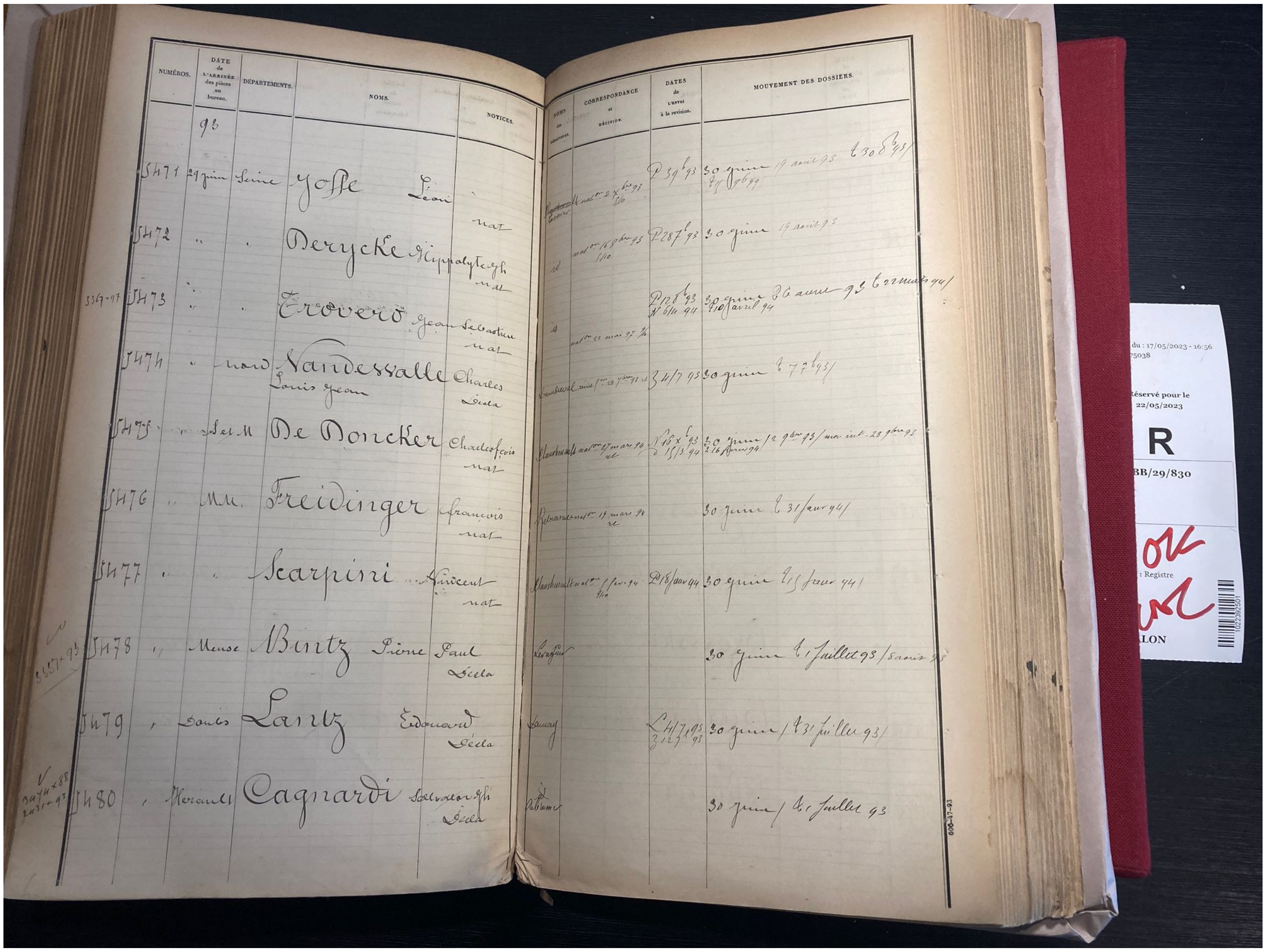
E Background information

E.1 Example of naturalisation decrees and registers

Figure E.1: Example of naturalisation decrees (indexed records)

Note: This figure is a screenshot of the naturalisation decrees published on January 4, 1887 as seen in the online reading room. Source: French national archives' online reading room (www.siv.archives-nationales.culture.gouv.fr)

Figure E.2: Example page of a naturalisation register



Note: Naturalisaion officers used naturalisation registers to record the date of application, the first and last name of applicants and the reference of the application file, which also features in the naturalisation decrees. We use this source to extract the date of naturalisation application. Source: Archives nationale de France, BB/29/829 to BB/29/837

E.2 Linking individuals

As explained in the main text, we construst our two main outcomes (exit and naturalisation application) by linking individuals either between census records and to naturalisation 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. Below, we describe the algorithm using by the `fastlink` R package, and provide more details on the strategy used to link individuals across records.

Description of the algorithm used in the `fastlink` R package

It proceeds in three steps. First, it computes an agreement vector γ_{ij} of length K (the number of linking variables) for each combination (i, j) of ith observation from dataset A and jth 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 λ, the probability of a match across all pairwise comparisons, and $2 \times K$ vectors π_{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 λ and π_{km}. We select the best match among matches with a probability greater than .85 to produce our main results.

Strategy used to Link between census records

When between linking census records, we link foreigners in the 1886 census to the full 1891 census and foreigners in the 1891 census to the full 1896 census. In both cases, we match foreigners to the full census in the next period to be able to track people who naturalized and might be counted as French in the next census period. We link individuals using four main variables: the year of birth, the first and last names, and the municipality of residence. In our main analyses, we use the municipality as a blocking variable, which means that we are forcing an exact match on the municipality. 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.

Year of birth: We allow 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.

First and last names: In the nominative censuses, first and last names are recorded in a single column. This complicates the identification of first and last names when more than 3 name elements are recorded, as it is the case in 11% of our observations for 1886, 1891 and 1896. We thus proceed as follows. We start by cleaning the name variable 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) removing all auxiliary information and particles from full names (for instance, we drop elements like "de", "mlle," "veuve") (3) removing all accents and other special characters. We then extract the first element of the list of name elements as the first name, and the last element of the list of names as the last name. Finally we replace both first and last names by missing values when the number of name elements is strictly smaller than 2 (.51% of observations).

Estimating match probabilities: Matching approximately 15,000 foreigners to 750,000 individual entries amounts to roughly 10 billion pairs making the linking computationally intensive. We simplify computation by first estimating the relevant probabilities using 20% of each census year (not blocking on municipality).

Strategy used to Link between census records and naturalisation decrees

We link foreigners in the 1886 census and the foreigners in the 1891 census to the naturalisation decrees using the same strategy as for exit. We link individuals using three main variables: the year of birth, the first and last names, while blocking on the municipality of residence.

Year of birth: This time, we match exactly on the year of birth.

First and last names: In the naturalisation decrees we can easily identify first and last names since they are seperated by a comma. However, in this case, all first names are listed on the decree, as this is an official administrative document, whereas in the census individuals tend to only report the first name they use in every day 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 reconcile records after the linking.

Table E.1: Difference in exit rate pre-treatment (between 1886 and 1891) between travellers and other occupations

	DV: Exit from municipality			DV: Exit from department		
	(1) Among French	(2) Among Italian	(3) Among Swiss	(4) Among French	(5) Among Italian	(6) Among Swiss
Traveller	0.065	0.295	0.175	0.108	0.377	0.252
	(0.069)	(0.054)	(0.230)	(0.057)	(0.011)	(0.185)
Observations	57,059	7,011	2,988	57,059	7,011	2,988
# of municipalities (clusters)	126	66	33	126	66	33

Note: In columns (1) to (3), the dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t+5$. In columns (4) to (6), the dependent variable (DV) is an indicator variable equal to one if a foreigner present in any municipality in census t is not found in any municipality in the census at $t+5$. The coefficient on *Traveller* is an estimate of the difference in means in exit between individuals who list travelling as part of their occupations and individuals who don't. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

F Additional analyses

F.1 Pre-trends for exit

As noted in the main text, the 1881 census does not include information on the nationality of individuals, making it difficult to test for pre-trends for exit. In this section, we describe our approach to test for pre-trends between Italians and Swiss. In short, we use the association between last names and nationality in the 1886 census to predict nationality based on last names in the 1881, 1886 and 1891 censuses.

Last names and name indexes using the 1886 census. Using data from the 1886 census, we calculate the Italian Name Index (INI$_{name}$) and the Swiss Name Index (SNI$_{name}$) using the methodology described in Fouka (2019, 2020). For each last name that appeared at least 5 times in the 1886 census, we first estimate the proportion of Italians who have this last name $P(\text{name}|\text{Italian})$ and the proportion of non-Italians with the same last name $P(\text{name}|\neg\ \text{Italian})$. We do the same for Swiss nationals to compute the the following two indices:

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

Identify Italians and Swiss in the 1881, 1886, and the 1891 census using last names. Matching last names in the 1881, 1886 and 1891 census to the Italian Name Index (INI$_{name}$) and the Swiss Name Index (SNI$_{name}$), we generate two indicator variables. Our dummy Italian equals one for individuals with an INI over 90 and zero for other individuals for whom the INI < 90 and the INI is not missing. Our dummy Swiss equals one for individuals with an SNI over 90 and zero for other individuals for whom the SNI < 90 and the SNI is not missing.

Testing for pre-trends. We restrict our sample to individuals in the 1881, 1886, and 1891 with an Italian name index (INI$_{name}$) or a Swiss name index (SNI$_{name}$) above 90. We link these individuals to the next census using the methodology described in section E.2 and generate an indicator variable equal to one if we cannot link an individual present in municipality m in census t to an individual in municipality m in the census at $t + 5$, for $t \in \{1881, 1886, 1891\}$. 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 F.1 shows the result of our regression using Equation 6 on the sample described above. The year 1886 is our reference year. We find that Italians were 1.7 percentage points more likely to leave their municipality than Swiss between 1886 and 1891. But we don't find evidence that this difference was larger or smaller than between 1881 and 1886 (coefficient: 2.6pp, s.e.: 2.7pp). This provides some evidence in favour of the parallel trend assumptions holding in our context for exit.

Table F.1: Exit: Testing for pre-trends

	DV: Exit
1881	-0.028
	(0.019)
1891	-0.027
	(0.006)
Italian (predicted)	0.017
	(0.008)
1881 × Italian (predicted)	0.026
	(0.027)
1891 × Italian (predicted)	0.019
	(0.007)
Constant	0.587
	(0.003)
Observations	24,325
# of municipalities (clusters)	230

Note: The dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t+5$. The sample consists in individuals in the 1881, 1886 and 1891 census with a last name that score higher than 90 on the INI_{name} or the SNI_{name}. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

F.2 Main results: Detailed difference-in-differences tables

We provide the detailed DiD tables for all the outcomes we analyze using our recommended empirical stategy (Equation 6).

Table F.2: Effect of violence on exit: Detailed results

	Among Italians	Among other foreigners	Comparing Italians to other foreigners	Among Swiss	Comparing Italians to Swiss
Post-violence	-0.001	-0.017	-0.017	-0.017	-0.019
	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)
Italian			-0.017		0.015
			(0.006)		(0.011)
Post-violence × Italian			0.016		0.019
			(0.006)		(0.009)
Observations	16,653	13,520	30,203	8,316	24,998
# of municipalities (clusters)	94	90	129	68	117
Mean DV in 1886 sample	0.680	0.692	0.686	0.659	0.673

Note: The dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in the same municipality m at $t+5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

Table F.3: Effect of violence on naturalisation application: Detailed results

	Among Italians	Among other foreigners	Comparing Italians to other foreigners	Among Swiss	Comparing Italians to Swiss
Post-violence	0.003	-0.002	-0.002	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Italian			0.002		0.003
			(0.000)		(0.000)
Post-violence × Italian			0.005		0.004
			(0.000)		(0.000)
Observations	16,653	13,520	30,203	8,316	24,998
# of municipalities (clusters)	94	90	129	68	117
Mean DV in 1886 sample	0.006	0.003	0.005	0.003	0.005

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t+5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

F.3 Heterogeneity by exposure to violence

Table F.4: Effect of violence on exit: Heterogeneity by exposure to violence

	High exposure	Low exposure	All
Post-violence	-0.040	-0.016	-0.016
	(0.002)	(0.005)	(0.005)
Italian	-0.148	-0.011	-0.011
	(0.003)	(0.006)	(0.006)
Post-violence × Italian	0.098	0.012	0.012
	(0.003)	(0.006)	(0.006)
High exposure			-0.000
			(0.005)
Post-violence × High exposure			-0.024
			(0.006)
Italian × High exposure			-0.125
			(0.013)
Post-violence × Italian × High exposure			0.072
			(0.014)
Observations	1,321	28,882	30,203
# of municipalities (clusters)	3	129	129

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t + 5$. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses. Individuals living in the areas of fons, vaise and la guillotière are coded as high exposure:

Table F.5: Effect of violence on naturalisation application: Heterogeneity by exposure to violence

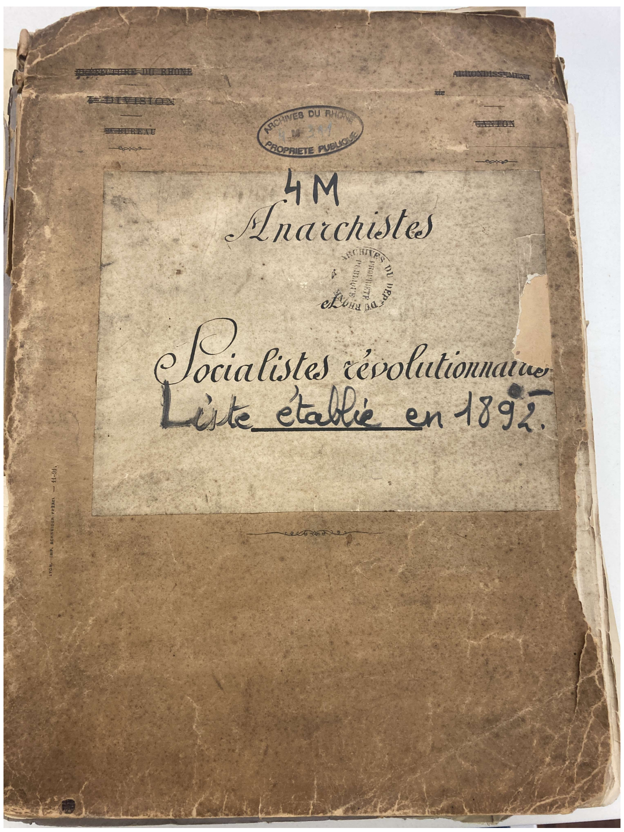
	High exposure	Low exposure	All
1891	-0.004	-0.002	-0.002
	(0.000)	(0.000)	(0.000)
Italian	0.003	0.002	0.002
	(0.000)	(0.000)	(0.000)
1891 × Italian	0.005	0.005	0.005
	(0.000)	(0.000)	(0.000)
High exposure			-0.000
			(0.000)
1891 × High exposure			-0.002
			(0.000)
Italian × High exposure			0.001
			(0.000)
1891 × Italian × High exposure			0.000
			(0.000)
Observations	1,321	28,882	30,203
# of municipalities (clusters)	3	129	129

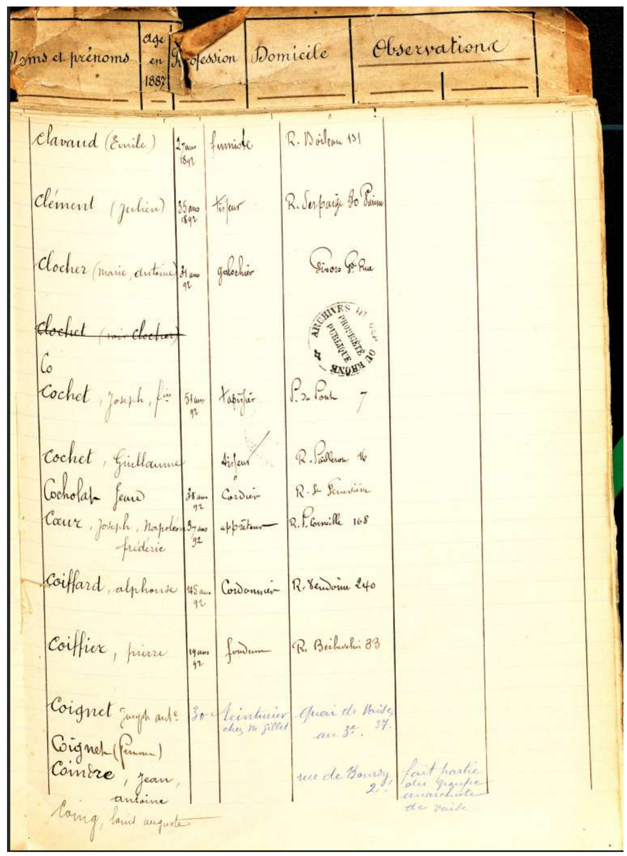
Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation applicant who applied for naturalisation between t and $t+5$. Individuals are coded as "High exposure" when they live in the area of Saint-Fons, Vaise, or la Guillotière. Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

F.4 Anarchists in the Rhône in 1892

We digitized the list of anarchists known to French security services in 1892 in the archives of the Rhône department (document 4M311 in the Rhône Archives Departementales). We identify individuals in our sample with the same last name as a potential anarchists, and compare the effect of exposure to violence on exit and naturalisation application among potential anarchists and others.

Figure F.1: Example of a page from the list of anarchists compiled by the Rhône prefecture in 1892





Note: This figure reproduces a page from the liste of anarchists known to the police department in 1892. Source: Archives departemental du Rhône

Table F.6: Exit and naturalisation application: Heterogeneity by anarchist status

	DV: Exit			DV: Naturalisation application		
	(1) Among potential anarchists	(2) Among others	(3) All foreigners	(4) Among potential anarchists	(5) Among others	(6) All foreigners
Post-violence	-0.057	-0.013	-0.014	-0.001	-0.002	-0.002
	(0.011)	(0.005)	(0.005)	(0.000)	(0.000)	(0.000)
Italian	-0.100	-0.018	-0.018	0.012	0.002	0.002
	(0.035)	(0.007)	(0.007)	(0.002)	(0.000)	(0.000)
Post-violence × Italian	0.036	0.014	0.014	-0.009	0.006	0.006
	(0.037)	(0.006)	(0.006)	(0.002)	(0.000)	(0.000)
Anarchist			-0.119			0.002
			(0.012)			(0.001)
Post-violence × Anarchist			-0.039			0.002
			(0.017)			(0.000)
Italian × Anarchist			-0.073			0.009
			(0.042)			(0.003)
Post-violence × Italian × Anarachist			0.026			-0.014
			(0.035)			(0.002)
Observations	1,794	28,385	30,203	1,794	28,385	30,203
# of municipalities (clusters)	29	126	129	29	126	129

Note: In columns (1) to (3), the dependent variable (DV) is an indicator variable equal to one if a foreigner present in municipality m in census t is not found in municipality m in the census at $t + 5$. In columns (4) to (6), the dependent variable (DV) is an indicator variable equal to one if a foreigner present in any municipality in census t is not found in any municipality in the census at $t+5$. Potential anarchists are individuals who share the same name as anarchists listed in the 1892 document in the Rhône Departmental Archives.

F.5 Heterogeneity: Detailed difference-in-differences tables

Table F.7: Heterogeneity by occupation: Detailed results

	DV: Exit			DV: naturalisation application		
	Among workers	Among shopkeepers	All foreigners	Among workers	Among shopkeepers	All foreigners
Post-violence	-0.024	0.043	-0.024	-0.003	-0.015	-0.003
	(0.014)	(0.009)	(0.013)	(0.001)	(0.002)	(0.000)
Italian	-0.010	0.016	-0.011	0.005	0.005	0.005
	(0.015)	(0.020)	(0.015)	(0.000)	(0.000)	(0.000)
Post-violence × Italian	0.038	-0.015	0.040	0.007	0.022	0.007
	(0.016)	(0.015)	(0.017)	(0.001)	(0.004)	(0.001)
Shopkeeper			-0.090			0.008
			(0.015)			(0.001)
Post-violence × Shopkeeper			0.074			-0.009
			(0.023)			(0.001)
Italian × Shopkeeper			0.025			-0.002
			(0.030)			(0.001)
Post-violence × Italian × Shopkeeper			-0.086			0.015
			(0.051)			(0.005)
Observations	10,249	806	11,067	10,249	806	11,067
# of municipalities (clusters)	81	19	83	81	19	83

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t+5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

Table F.8: Heterogeneity by wealth: Detailed results

	DV: Exit			DV: Naturalisation application		
	With no employee	With employee	All foreigners	With no employee	With employee	All foreigners
Post-violence	-0.008	-0.063	-0.009	-0.002	-0.007	-0.002
	(0.005)	(0.015)	(0.006)	(0.000)	(0.001)	(0.000)
Italian	-0.011	-0.080	-0.011	0.005	-0.007	0.005
	(0.005)	(0.030)	(0.005)	(0.001)	(0.001)	(0.001)
Post-violence × Italian	0.020	0.099	0.021	0.008	0.016	0.008
	(0.008)	(0.036)	(0.008)	(0.001)	(0.002)	(0.001)
Has employees			0.020			0.002
			(0.010)			(0.001)
Post-violence × Has employees			-0.051			-0.006
			(0.009)			(0.001)
Italian × Has employees			-0.088			-0.011
			(0.041)			(0.002)
Post-violence × Italian × Has employees			0.079			0.007
			(0.032)			(0.004)
Observations	12,591	1,330	13,939	12,591	1,330	13,939
# of municipalities (clusters)	84	39	91	84	39	91

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t + 5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

Table F.9: Heterogeneity by position in household: Detailed results

	DV: Exit			DV: Naturalisation application		
	Among employees	Among heads of HH	All foreigners	Among employees	Among heads of HH	All foreigners
Post-violence	-0.035	-0.009	-0.038	-0.001	-0.002	-0.002
	(0.006)	(0.005)	(0.009)	(0.000)	(0.000)	(0.000)
Italian	-0.062	0.001	-0.061	0.002	0.003	0.002
	(0.009)	(0.006)	(0.009)	(0.000)	(0.000)	(0.001)
Post-violence × Italian	0.033	0.018	0.037	-0.001	0.005	-0.000
	(0.013)	(0.007)	(0.013)	(0.000)	(0.000)	(0.001)
Head of HH			-0.152			0.001
			(0.012)			(0.000)
Post-violence × Head of HH			0.029			-0.000
			(0.008)			(0.000)
Italian × Head of HH			0.062			0.002
			(0.011)			(0.001)
Post-violence × Italian × Head of HH			-0.019			0.005
			(0.014)			(0.001)
Observations	3,646	22,473	26,150	3,646	22,473	26,150
# of municipalities (clusters)	81	96	122	81	96	122

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t + 5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

Table F.10: Heterogeneity by household's type: Detailed results

	DV: Exit			DV: Naturalisation application		
	In not mixed households	In mixed households	All foreigners	In not mixed households	In mixed households	All foreigners
Post-violence	-0.034	-0.001	-0.036	0.000	-0.006	0.000
	(0.004)	(0.014)	(0.007)	(0.000)	(0.001)	(0.000)
Italian	-0.001	0.020	-0.007	0.004	0.005	0.004
	(0.008)	(0.017)	(0.013)	(0.001)	(0.002)	(0.001)
Post-violence × Italian	0.044	-0.029	0.047	0.004	0.009	0.004
	(0.009)	(0.033)	(0.011)	(0.001)	(0.005)	(0.001)
In a mixed household			-0.053			0.006
			(0.016)			(0.001)
Post-violence × In a mixed household			0.038			-0.006
			(0.013)			(0.001)
Italian × In a mixed household			0.028			0.001
			(0.014)			(0.002)
Post-violence × Italian × In a mixed household			-0.071			0.006
			(0.031)			(0.005)
Observations	13,607	3,752	17,363	13,607	3,752	17,363
# of municipalities (clusters)	65	59	85	65	59	85

Note: The dependent variable is an indicator variable equal to one if a foreigner present in census t is matched to a naturalisation application by $t + 5$. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

G Additional outcomes

G.1 Exit from and relocating within the department

We look at two other dimensions of exit. First, we define an indicator variable equal to one if individual i is not found in the *Rhone department* in the census at $t+5$. To link individuals across censuses, 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). Unlike in the same text, we do not block on municipality. As before, 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. Summary statistics for exiting the department can be found in Table B.1. Second, we look at whether an individual has moved to another commune within the Rhone department. To do so, we look at our matches without blocking on municipality at t and create an indicator variable equal to one if an individual is found in another commune at $t+5$. Tables G.1 and G.2 indicate that our exit result in Table 4 is a mixture of leaving the department and relocating within the department with the former dominating slightly.

Table G.1: Effect of violence on exit: Looking at exit from the department rather than exit from the municipality

	Among Italians	Among other foreigners	Comparing Italians to other foreigners	Among Swiss	Comparing Italians to Swiss
1891	-0.007	-0.017	-0.016	-0.022	-0.021
	(0.006)	(0.003)	(0.003)	(0.005)	(0.004)
Italian			-0.024		0.013
			(0.004)		(0.005)
1891 × Italian			0.009		0.013
			(0.006)		(0.006)
Observations	16,653	13,520	30,203	8,316	24,998
# of municipalities (clusters)	94	90	129	68	117

Note: Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

Table G.2: Effect of violence on exit: Looking at relocating within the department

	Among Italians	Among other foreigners	Comparing Italians to other foreigners	Among Swiss	Comparing Italians to Swiss
1891	0.005	0.000	-0.001	0.005	0.001
	(0.006)	(0.003)	(0.003)	(0.003)	(0.005)
Italian			0.007		0.002
			(0.005)		(0.008)
1891 × Italian			0.007		0.006
			(0.008)		(0.010)
Observations	16,653	13,520	30,203	8,316	24,998
# of municipalities (clusters)	94	90	129	68	117

Note: Municipality fixed effects not shown. Standard errors clustered at the municipality level in parentheses.

G.2 Other integration outcome: Partnership with French

We also look at another integration outcome: Having a French spouse. In this case, our integration outcome takes the form of an indicator variable taking value one if an individual in census t in municipality m is (i) matched with an individual in the census at $t + 5$ in the same locality and (ii) the matched individual is a head or spouse living in partnership with a French national. The indicator takes value 0 otherwise. In Table G.3, we report estimates of the effect of violence on having a French spouse using our approach (Equation 6) in column (1) and from traditional approaches (Equation 7) in column (2). Our preferred approach estimates a null effect of violence, whereas the approach used in the literature find a statistically significant (and meaningful) positive effect. In Table G.4, we report the mean of our outcomes in 1891 among of foreigners in the control group (foreigners of an nationality other than Italian). We find that shopkeepers and heads of households are more likely to stay and be in a partnership with a French national than workers and employees. We already know that exposure to violence induces a higher exit rate among workers than shopkeepers (Table 5 in the main text) and among employees than heads of household (Table C.1 in the Appendix). Hence, those more likely to leave due to the treatment are those who were least likely to integrate absent treatment (according to our proxy test). This indicates that the estimate obtained from the literature (column (2) of Table G.3) is likely to be upwardly biased:

Table G.3: Effect of violence on having a French spouse

	DV: Has a French spouse	
	(1)	(2)
	$\hat{\beta}^0_{pl}$	$\hat{\beta}^1$
	(Equation 6)	(Equation 7)
Post-violence	0.004	-0.046
	(0.002)	(0.006)
Italian	-0.004	-0.035
	(0.002)	(0.002)
Post-violence × Italian	-0.001	0.021
	(0.002)	(0.003)
Observations	30,203	349
# of municipalities (clusters)	129	113
Target population	Pre-violence	Post-violence
Mean DV for Italians pre-treatment	0.073	0.058
Corrected estimate	-.001	
Effect size (mean)	-1.11%	36.88%
Effect size (std)	-0.31%	39.86%

Note: In column (1), the dependent variable is an indicator variable equal to one if a foreigner present in census t in municipality m is linked to an individuals in the same municipality at $t+5$ who has a French spouse. In column (2), the dependent variable is the number of foreigners with a French spouse divided by the number of foreigners at time t and the variable *Post-violence* takes value one for the 1896 census. We estimate β^1 using Equation 7 and weighting by the number of foreigners in the municipality. Municipality fixed effects are not shown. Standard errors clustered at the municipality level in parentheses.

Table G.4: Has a French spouse: Mean in the control group in 1891 for different dimensions of heterogeneity

	Mean	p-value of the difference
A. By occupation		
Among worker	0.112	
Among shopkeeper	0.152	0.092
B. By wealth		
Among heads with no employee	0.136	
Among heads with employees	0.115	0.226
C. By position in household		
Among employees	0.052	
Among heads of household	0.094	0.000

Note: This table shows estimates of the mean of the dependent variable *Has a French Spouse* in 1891 in the control group for different subgroup of the population, as well as the p-value of the difference bewteen sugroups. In each subgroup, the mean corresponds to the proportion of foreigners of nationality other than Italian in the 1891 census who were match to an individual with a French spouse in the 1896 census.