

Business as (Un)usual: Immigrant Entrepreneurship and Native Backlash*

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Abstract

A large literature has investigated the conditions under which exposure to immigrants leads to hostility or support for immigration in Western democracies. Yet, one of the most routine forms of exposure for natives is with immigrant-owned businesses. Millions of immigrant businesses exist in Europe and North America, where they generate jobs and tax revenue. Do their contributions foster support for immigration, or does their everyday presence heighten hostility? To answer this, I assemble a new dataset on more than 20 million geocoded French businesses, classified by the inferred origin of their owners' names and apply a shift-share instrumental variable strategy exploiting exogenous industry trends. I find that doubling the number of storefront immigrant businesses in a municipality over a decade increases support for the far right by five percentage points. The effect is concentrated among businesses owned by immigrants from Muslim-majority countries and persists despite evidence of positive economic contributions. A survey experiment with 1,400 French respondents further shows that elite rhetoric powerfully mediates perceptions of immigrant businesses as cultural and economic threats. This paper shifts the exposure debate from demographic presence to economic spaces as sites of symbolic politics: immigrants' economic choices, chief among which business creation, structure the cues that natives observe in daily life, with downstream consequences for attitudes and voting.

1 Introduction

Support for far-right populist parties in Western democracies has risen sharply over the past two decades. Far-right parties have taken power in Hungary, Poland, Italy, the United States,

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Israel, and Brazil and entered governing coalitions in Austria, Finland, and Slovakia. In many other countries, their popularity continues to grow. This trend has produced lasting policy consequences, including Brexit (Algan et al., 2017; Guriev and Papaioannou, 2022), while also accelerating the erosion of democratic norms (Levitsky and Ziblatt, 2019) and deepening partisan polarization (Bischof and Wagner, 2019). Few issues have become as closely tied to this rise, and central to contemporary Western politics, as immigration (Ford and Jennings, 2020; Cools et al., 2021; Pupaza and Wehner, 2023; Margalit et al., 2024; Alrababah et al., 2024).

Scholars have devoted substantial attention to explaining natives' attitudes toward immigrants (Hainmueller and Hopkins, 2014), emphasizing both economic (Scheve and Slaughter, 2001; Hanson et al., 2007; Dancygier and Donnelly, 2013) and cultural factors (Kinder and Kam, 2009; Hainmueller and Hangartner, 2013; Maxwell, 2019). These accounts typically focus on variation in the local presence of immigrants and on their passive characteristics, such as skills, wealth, race, language, or religion. However, conceptualizing immigrants solely as a statistic obscures the role played by the decisions they make as economic agents in host countries. Crucially, to earn an income, immigrants must choose between finding salaried employment and creating their own businesses.

Millions of immigrant-owned businesses operate in Western democracies, and immigrants establish them at significantly higher rates than natives (Azoulay et al., 2022), making their potential impact increasingly consequential over time. Unlike immigrant enclaves, which are often confined to peripheral areas, immigrant businesses are frequently located in native-dominated neighborhoods, where they represent a primary form of exposure. To natives, such businesses could appear as a best-case scenario: they provide goods and services, generate tax revenue, and do not directly compete with native workers in the labor market. Yet they may also provoke hostility by serving as visible cultural symbols and have become frequent targets of far-right rhetoric. When natives encounter immigrant businesses, do they recognize their economic contributions? Do they react against new markers of cultural difference? How does exposure to immigrant businesses shape support for the far right?

In this paper, I develop a theory of how immigrant-owned businesses shape natives' attitudes and political behavior by decomposing their impact into four channels: businesses act

as symbols, producers, employers, and spaces. These channels operate independently of the overall size of the immigrant population.

As a *symbol*, an immigrant-owned business signals the presence of a group and alters the physical environment of a community. Natives who prefer homogeneity may resent such changes. As a *producer*, a business provides goods and services that benefit native consumers but may also compete with native-owned firms. Even though business owners are a minority, natives may internalize the concerns of local producers, fueling backlash. As an *employer*, an immigrant business can create jobs and generate tax revenue, potentially improving attitudes toward immigrants. Yet if employers primarily hire co-ethnics, natives may perceive a threat to their own employment opportunities or local wages. As a *space*, a business becomes a site of social interaction. If it primarily serves an immigrant clientele, it may heighten natives' anxieties. If it attracts both immigrants and natives, it can foster intergroup contact and reduce prejudice.

These channels can shift support for the far right in several ways. Symbolic visibility may increase the salience of immigration, benefiting parties that have historically owned the issue (Meguid, 2005). If natives feel worse off and attribute this to immigrants, they may support restrictionist policies aligned with far-right platforms. Conversely, if they feel better off, they may resist such appeals. Finally, voters may punish local incumbents for immigrant business creation, which may benefit the far right especially when incumbents belong to mainstream parties. How these mechanisms play out is likely to be moderated by elite and media rhetoric.

In this paper, I estimate the net effect of immigrant-owned businesses on support for the far right using comprehensive business-level administrative data from France. Because a 1978 law prohibits direct data collection on ethnicity and origin, I employ a machine-learning algorithm to classify more than 20 million business owners by probable country of origin using their names. I then geo-code all businesses and time-stamp their opening and closing dates to construct a new spatial measure of local exposure to immigrant businesses, covering more than 33,000 municipalities over time. With this novel dataset, I test whether exposure to immigrant-owned storefront businesses affects support for the National Rally, France's main far-right party. To my knowledge, this is the first study to examine the political consequences

of immigration at the municipal level across the entire country, without relying on restrictive definitions of immigration (Edo et al., 2019; Vasilopoulos et al., 2022).

A central empirical challenge in studying the impact of businesses is the endogeneity of entrepreneurship. Immigrant businesses arise not only due to immigrant presence but also from local market conditions, such as demand, competition, and labor supply, all of which may themselves correlate with support for the far right. These factors risk confounding the relationship of interest. To address this challenge, I construct a shift-share instrumental variable (IV) that generates exogenous variation in the municipal growth of immigrant businesses between 2009 and 2019 (Bartik, 1991; Borusyak et al., 2022).

The instrument exploits exogenous trends in business creation among immigrants, who tend to specialize in industries distinct from those of natives¹. For each municipality, these trends are weighted by its pre-period industrial composition, reflecting the tendency of industries to agglomerate geographically. The resulting shift-share instrument generates exogenous variation in the number of immigrant businesses established in each municipality between 2009 and 2019. I provide evidence in support of the exclusion restriction: the instrument predicts neither the number of native businesses nor the local presence of immigrants, two key potential confounders.

Between 2009 and 2019, doubling the number of storefront immigrant businesses in and around a municipality, from 10 to 20 on average, increased support for the far right by five percentage points. This effect is comparable in size to that of a 15-point decline in the share of college-educated residents, a strong predictor of far-right voting. Results are robust to alternative constructions of both the instrument and the exposure measure. Estimates from a simpler two-way fixed effects specification are consistent with the IV results in sign and significance, though smaller in magnitude. Immigrant businesses do not produce a statistically significant shift in support for any other party, though the direction of the estimates suggests a broader move toward anti-immigration attitudes.

I then examine which of the four theoretical channels account for these results. Three main findings emerge. First, the *symbolic* role of immigrant businesses drives the net effect.

¹Exogeneity is generated by predicting trends using GDP growth in immigrants' countries of origin and by residualizing them to remove components driven by overall industrial trends not specific to immigrants.

Decomposing by business type, I find that the effect is concentrated in storefront businesses owned by immigrants from Muslim-majority countries, a group at the center of cultural conflict in France. By contrast, there is no significant effect for Muslim-owned businesses without storefronts, which lack symbolic visibility, nor for businesses owned by immigrants from other groups. These findings suggest that immigrant businesses provoke backlash when they serve as *symbols* of cultural heterogeneity, either by altering the physical environment valued by natives or by signaling the presence of groups with whom broader cultural conflict exists. Finally, I find no evidence that the *space* channel moderates the effect: even restaurants and other businesses facilitating interactions, which could most plausibly facilitate intergroup contact and therefore reduce prejudice, do not mitigate the backlash.

Second, I show that the overall negative effect persists despite the fact that immigrant businesses benefit natives as producers and employers. Using measures of local economic conditions derived from the Herfindahl-Hirschman index, I find that new immigrant businesses increase the diversity of goods and services available to consumers without raising average levels of market competition, consistent with prior research on the differentiation of migrant workers (Piore, 1979; Massey et al., 1993). These businesses therefore do not threaten native producers. Yet natives do not appear to credit immigrants for these economic benefits, or such attribution does not translate into voting behavior. Nor are they punished for negative outcomes: using exact locations and dates of openings and closures, I find that the displacement of native businesses by immigrant-owned businesses has no significant electoral effect. Likewise, the creation of new storefront businesses does not significantly affect local unemployment, perhaps because they tend to be small. Taken together, these results suggest that the *symbolic* role of immigrant businesses drives natives' responses, while their roles as *spaces*, *producers*, and *employers* are neither rewarded nor sanctioned.

Finally, I show that politicians respond to immigrant businesses and that elite cues shape how voters perceive them. To complement the IV analysis, I fielded a preregistered survey experiment with 1,400 French respondents, randomly exposing them to far-right rhetoric targeting immigrant businesses. Elite criticism significantly shifted public opinion: respondents who read the media article were more likely to view immigrant businesses as both an identity

and an economic threat, even though my observational results indicate no adverse economic effects. They also expressed more negative attitudes toward immigrants more broadly. In parallel, I find that growth in immigrant-owned businesses prompts local elected officials to raise business taxes. Together, these findings suggest that immigrant businesses are politically salient targets, and that both elites and local politicians can capitalize on their presence to mobilize constituents or justify redistribution.

This paper makes three substantive contributions. First, it advances the literature on the rise of far-right populist parties (Golder, 2016; Berman, 2021). Prior work shows that anxieties about immigrant presence and demographic shocks fuel far-right support (Hopkins, 2010; Cools et al., 2021; Pupaza and Wehner, 2023; Alrababah et al., 2024; Margalit et al., 2024). I show that immigrants' presence, skills, or ethnicity capture only part of their effect on native voting behavior. By examining immigrants' economic decisions, particularly the choice to start a business rather than seek salaried employment, I reveal new mechanisms linking immigration to far-right support. Natives respond not only to demographic change but also to the physical symbols of immigrant businesses, and, when primed by elite rhetoric, form judgments about the economic desirability of immigration based on these businesses. The magnitude of this effect is comparable to other well-established drivers of far-right support: whereas a one standard deviation in Colantone and Stanig's (2018a) import shock causes an increase in radical right support between a 0.5 (OLS) and 1.7 (IV) percentage points, an equivalent increase in exposure to immigrant businesses causes an increase between 0.3 (TWFE) and 5.5 (IV) percentage points.

Second, this paper contributes to the large literature (Hainmueller and Hopkins, 2014) on immigration attitudes. Prior work emphasizes economic concerns—labor market competition (Scheve and Slaughter, 2001; Mayda, 2006), fiscal burdens (Facchini and Mayda, 2009; Hanson et al., 2007; Cavaillé and Ferwerda, 2023), or broader sociotropic effects (Dancygier and Donnelly, 2013)—as well as cultural explanations linked to education (Hainmueller and Hiscox, 2007, 2010), identity and ethnocentrism (Kinder and Kam, 2009; Schildkraut, 2014), or cultural distance (Hainmueller and Hangartner, 2013; Tabellini, 2020). Examining immigrant entrepreneurship enriches our understanding of the intersection between cultural and eco-

nomic explanations. For example, an increase in the availability of local goods and services, though predicted by economic theory to increase consumer utility, can in fact make natives feel worse off if they attach a negative cultural valence to the goods. Results also suggest that if far-right policies promoting preferential employment for natives lead more immigrants to create businesses instead, they may trigger even stronger backlash among natives, fueling a nativist cycle.

Finally, this paper contributes to contact theory. Since Allport's (1954) seminal work, a large literature has examined when intergroup contact reduces prejudice and improves relations, particularly between natives and immigrants (Hopkins, 2010; Ellison et al., 2011; Enos, 2014; Paluck et al., 2019). Recent work has refined the concept of contact, emphasizing its duration and depth (Nathan and Sands, 2023). This article expands the scope of contact theory by showing that exposure need not occur through direct interpersonal interaction: encounters with immigrant-owned businesses, or other immigrant institutions, also shape natives' daily experiences and attitudes. Moreover, because such businesses are highly visible, they can lead natives to misperceive the broader size and presence of immigrant communities.

The rest of this paper is organized as follows. In Section 2, I introduce the theory and derive empirical predictions. In Section 3, I present the French context. In Section 4, I introduce the data sources and the measurement strategies. In Section 5, I present the identification strategy and the aggregate result. In Section 6, I present the survey experiment and its results. In Section 7, I present a more detailed analysis of mechanisms, and in Section 8 I conclude.

2 Theory

2.1 Far-Right Populism and Attitudes toward Immigration

The dramatic rise of far-right populist ideologies and leaders around the world has generated significant scholarly attention examining, among other factors, backlash against globalization (Rodrik, 2018; Colantone and Stanig, 2018b; Autor et al., 2020), status threat (Inglehart and Norris, 2016; Mutz, 2018; Dancygier et al., 2022; Oshri et al., 2023; Margalit et al., 2024), economic anxieties (Kavanagh et al., 2021), and the decline of rural and peripheral regions

(Margalit et al., 2024; Dancygier et al., 2025). However, few issues have become as synonymous with the rise of far-right populism and as central to contemporary Western politics as immigration (Ford and Jennings, 2020; Cools et al., 2021; Pupaza and Wehner, 2023; Margalit et al., 2024; Alrababah et al., 2024).

Researchers have accordingly devoted a great deal of attention to understanding native attitudes toward immigrants. Political economy theories focus on the skills (Scheve and Slaughter, 2001; Mayda, 2006), income (Hanson et al., 2007; Facchini and Mayda, 2009; Cavaillé and Ferwerda, 2023) and perceived aggregate economic impact (Dancygier and Donnelly, 2013; Hainmueller and Hopkins, 2014). Cultural arguments focus on the identity of immigrants (Hainmueller and Hangartner, 2013) and their cultural distance with natives (Kinder and Kam, 2009; Schildkraut, 2014). These theoretical mechanisms are moderated by the level of education of natives (Hainmueller and Hiscox, 2007; Cavaillé and Marshall, 2019; Maxwell, 2019) and their relative political powers compared to immigrant groups (Dancygier, 2010).

These theories explain attitudes toward immigration, and their impact on far-right support, largely based on variation in the numbers of immigrants present in a territory and the characteristics of these immigrants. However, conceptualizing immigrants as a statistic obscures the role played by the economic choices they make in their host countries. To earn an income, immigrants must decide whether to join the salaried labor force or to create their own business. Millions of immigrant-owned businesses exist in Western democracies, and immigrants create businesses at a significantly higher rate than natives (Azoulay et al., 2022). Immigrants' decision to create a business instead of finding employment has been studied by economists who suggest it is a function of their risk profile (Lucas, 1978; Fairlie and Lofstrom, 2015), the discrimination they face on the labor market (Aldrich and Waldinger, 1990; Fernandez and Fernandez-Mateo, 2006; Auer et al., 2019), and their social networks (Kerr and Mandorff, 2020).

Studies have consistently shown that immigrant entrepreneurship has a positive aggregate impact on economic activity and innovation in host economies (Portes et al., 2002; Hunt and Gauthier-Loiselle, 2010; Kerr, 2013; Sequeira et al., 2020; Azoulay et al., 2022). This runs counter to prevailing far-right narratives depicting immigrants as a burden on the national

economy. For natives, immigrant businesses are thus perhaps the most appealing form of economic activity: they create growth while releasing labor market competition. Yet, immigrant businesses are often the focus of aggressive discourse and behavior from far-right supporters and politicians: news reports reveal they are targeted by local campaigns in France (Le Monde, 2015), denounced by national politicians in Italy (CNN, 2018), singled out by British or German rioters (TIME, 2016; Al Jazeera, 2024), and threatened by the Trump administration's crackdown on immigration in the US (The New York Times, 2025).

Anecdotal evidence of strong backlash despite economic contributions raises the following question: when does the presence of immigrant-owned businesses lead to increased support for the far right?

2.2 A Political Theory of Immigrant Businesses

I conceptualize new businesses as serving four simultaneous roles in a community: new symbols, new producers, new employers, and new spaces. I describe below each of these channels, which can affect the attitudes and behavior of natives exposed to immigrant businesses in different ways. In the next section, I discuss the concept of *exposure* to businesses and the ways that changes in attitudes can be converted in voting behavior.

As a *new symbol*, a business can first provide a noisy signal of the presence of an immigrant group to natives if it includes markers of group identity. Some businesses are more likely to signal group identities: for example, a restaurant offering cuisine from a distinct country, or a hair salon tailored to a specific ethnic or racial group, may be identifiable by natives, while a hardware store may not². Regardless of the true underlying presence of immigrants, the literature suggests that the perception of in-migration can trigger backlash among natives (Green et al., 1998). As a symbol, a new business can also affect the physical environment of a community. This idea speaks to a broader literature showing that voters punish incumbent for undesirable changes to their environment, as shown by Stokes (2016) in the context of wind turbines. Some natives may prefer an aesthetically homogeneous space aligned with their perception of local or national identity (Schildkraut, 2014; Faury, 2024). Whether and

²This is not a one-to-one relationship: an immigrant business owner could display a symbol of group identity even if the business itself has no cultural valence, for example by hanging a flag on the wall.

to what extent natives are discontent with a new symbol should depend on predispositions toward the group whose presence is signaled, on natives' acceptance of multiculturalism (see Maxwell, 2019), and on the cultural distance between natives and the immigrant group.

Hypothesis 1 (Symbol) *Immigrant businesses are more likely to cause an increase in support for the far right when they are owned by members of groups with whom there exists a cultural conflict.*

A new business is also a *new producer* of goods and services. Basic utility theory indicates that this should benefit consumers: an increase in the diversity of goods and services available in a local market will meet a more diverse set of preferences. An increase in the supply of a good may also decrease its price³. Given the diminishing returns associated to both these factors, the extent to which a business benefits consumers economically should depend on pre-existing market conditions. A business providing a formerly unavailable product, or one that was dominated by a local monopoly, should benefit consumers more. Whether consumer benefits are internalized by voters and attributed to immigrant businesses and whether this attribution shifts attitudes, are empirical questions⁴. By providing a new product, a new business can also create competition for existing native businesses selling the same product. While business owners are a minority in any community, natives can sociotropically internalize the concerns of their community members (Dancygier and Donnelly, 2013), which could lead to backlash.

Hypothesis 2 (Producer) *Immigrant businesses are less likely to cause an increase in far right support if the product they offer improves the diversity of goods and services available locally.*

As a *new employer*, a new immigrant business can create employment opportunities for community members and generate tax revenue for local governments. Political economy theories of attitudes toward immigration suggest that observing this contribution might mitigate

³Note that downward price pressure will emerge even with differentiated goods. That is, even if immigrants are not perfect substitutes for native businesses but create specialized businesses (i.e., different cuisines, slightly different services, etc.), consumers should benefit.

⁴Evidence exists that voters reward larger ventures such as greenfield FDI projects (Owen, 2019) and that they vote on the basis of macroeconomic trends perceived locally (Healy and Lenz, 2017). However, to my knowledge evidence that voters attribute political credit for micro-level shocks to their consumer welfare, for example due to the business creation, is missing.

native voters' perception of a distributive conflict, which may improve their attitudes toward immigrants (Hanson et al., 2007; Scheve and Slaughter, 2001; Cavallé and Ferwerda, 2023). However, they may also believe that immigrants constitute a threat of displacement for native businesses. If immigrants tend to hire members of their own groups, natives may then predict that new immigrant businesses will ultimately reduce native labor demand, constituting a threat to their wages and employment opportunities. In general, Hainmueller and Hopkins (2014) have shown that only limited evidence supports the idea that attitudes are explained by direct economic impacts.

Hypothesis 3 (Employer) *Immigrant businesses are more likely to cause an increase in far right support if their creation coincides with the closure of native businesses, fostering a sense of displacement, in particular if they disproportionately hire immigrants.*

Finally, a new business constitutes a *new space* for individuals to gather and interact. If a business primarily serves a specific immigrant group, it may attract a relative concentration of individuals from this group that may trigger anxiety among natives (Enos, 2014). Examples of such businesses include ethnic-specific grocery stores, or hookah lounges. If the business serves both immigrants and natives, it can facilitate inter-group contact, both among customers and between customers and operators. Here, Nathan and Sands's (2023) framework is helpful: the longer and deeper contact between groups is, the more likely it is to lead to a reduction in prejudice. Following that logic, businesses that provide socialization spaces, like restaurants, could improve natives' perception of immigrants.

Hypothesis 4 (Space) *Immigrant businesses are less likely to cause an increase in far right support when they include socialization spaces that are not solely targeted at an in-group clientele.*

Importantly, I am interested in the effect of businesses holding constant the local presence of immigrants. In a community with a certain proportion of immigrant residents, what happens when new immigrant businesses open up? The counterfactual of interest is therefore a community with fewer immigrant-owned businesses but with the same number of immigrant residents⁵. The four functions of immigrant businesses laid out above do not make it

⁵Similarly, the counterfactual is one in which the number of native-owned businesses is held constant. This is therefore not primarily a displacement story.

possible to unambiguously predict whether immigrant businesses will affect native attitudes positively or negatively, which remains an empirical question. In this paper, I therefore begin by estimating the net effect of exposure to immigrant businesses on support for the far right.

Hypothesis 5 (Net Effect) *In aggregate, exposure to immigrant businesses may either increase or decrease native support for the far right.*

Decomposing the roles of new businesses helps specify scope conditions for the expected impact of an immigrant business on native attitudes. In the US, Vietnamese-owned nail salons do not display *symbols* of group identity and serve both natives and immigrants. They are therefore unlikely to trigger backlash and be conducive to far-right voting. By contrast, Mexican grocery stores that display symbols of group identity and primarily serve Latino immigrants may be expected to provoke backlash. In France, Chinese restaurants may not trigger a strong backlash because Chinese immigration has not been a salient political topic, and because cultural anxieties have not been directed at Chinese individuals. However, this could change when such anxieties are activated, for example during the Covid-19 pandemic (Huang et al., 2023). In contrast, halal butchers, associated with immigration from Arab or Middle-Eastern countries and primarily serving members of those groups, may trigger backlash due to the salience of political debates around Islam⁶.

2.3 Exposure and Vote for the Far Right

Since Allport's (1954) seminal "contact hypothesis", scholars of political psychology have devoted substantial attention to contact between members of different groups. Allport argued that under certain conditions, contact will lead to reduction in prejudice about out-group members. Researchers have applied this to contact between natives and immigrants, with ambiguous conclusions Hopkins (2010); Enos (2014); Hangartner et al. (2019); Cools et al. (2021); Alrababah et al. (2024). Recent work has refined the concept of contact: for example, Nathan and Sands (2023) characterize it by its duration and its depth, with different implications for between-group prejudice. Broadening our conception of contact to include *exposure* to immigrant establishments or institutions, of which businesses are a central example, brings this

⁶See Abdelgadir and Fouka (2020) for a discussion of this topic.

line of scholarship into the sphere of political economy and helps understand how immigrants' agency and choices determine their political impact.

Converting the impact of the four roles of immigrant businesses introduced in the previous section into voting behavior requires natives to be *exposed* to these business, that is, to become aware of their existence and activity.

Identifiability by natives

To make the connection between businesses and immigration, and by extension immigration policy and the far right, natives must first consider the owners of these businesses as *immigrants*. My theory therefore relies not on legal definitions of who is an immigrant, but on native perceptions⁷. While second- or third-generation immigrants might be born in the host country and not considered legal immigrants, natives may perceive them as such because of ascriptive characteristics such as race or religion. Conversely, natives may not perceive natives of neighboring countries with shared cultural or linguistic traits as immigrants.

Ownership of a business may not systematically be accurately inferred by natives if employees with whom customers interact are not immigrant themselves. While this may attenuate the relationship between immigrant entrepreneurship and far-right vote in some cases, natives are likely to correctly assume ownership in many cases, including businesses that display visible identity signals, small businesses where owners directly interact with customers, and businesses where employees are hired in-network, and therefore likely to be immigrants as well. Sociological work shows extensive evidence that minorities are more likely to find jobs via their networks than non-minorities (see e.g., Elliott, 2001; Fernandez and Fernandez-Mateo, 2006).

Conversion into Far-Right Vote

I have shown how immigrant businesses can affect natives through four channels. This can in turn affect voting behavior and support for the far right in at least three ways. First, natives ex-

⁷For example, France's *Insee* defines an immigrant as "a person who is born a foreigner and abroad, and resides in France." In contrast, The US defines as an immigrant "any person lawfully in the United States who is not a US citizen." Some formal definitions can therefore consider a naturalized citizen an immigrant (France) and others not (US).

posed to immigrant businesses may seek to reward or sanction their local incumbent government, if they respectively feel better or worse off and attribute the development of immigrant businesses to local policies (Burnett and Kogan, 2017; Larsen et al., 2019; Benedictis-Kessner and Warshaw, 2020). Because mainstream parties historically dominate local governments, if the net effect of immigrant businesses is negative, far-right parties may mechanically reap the benefits of native discontent. This is particularly true as trust in these mainstream parties erodes (Algan et al., 2017).

Second, exposure to immigrant businesses may directly shift the preferences of native voters. For example, natives may have previously been supportive of inclusive immigration policies because they did not feel directly affected by the presence of immigrants. This is supported by existing work finding that the direct effects of economic competition are limited (Tingley, 2013; Hainmueller and Hopkins, 2014). However, natives exposed to immigrant businesses may dislike symbols of cultural heterogeneity or worry about the displacement of businesses owned by members of their network. This may shift their preferences about immigration policy. If their updated preferences are better aligned with far-right parties promoting restrictionist policies, they will be more likely to support these parties electorally (Key, 1955; Ansell et al., 2022; Gidron, 2022).

Finally, exposure to immigrant businesses can increase the salience of immigration. Native voters supportive of restrictionist immigration policies because of their pre-existing preferences for cultural homogeneity (Hainmueller and Hiscox, 2007; Maxwell, 2019) may not have previously voted on that basis despite the local presence of immigrants. However, their immigration-based votes may be activated by exposure to immigrant businesses due to one of the channels presented above (Hopkins, 2010; Iversen and Goplerud, 2018). Far-right parties will likely benefit from the increased salience of immigration because of their historical ownership of the issue (Meguid, 2005). The effect of exposure to businesses on the salience of immigration can itself be mediated by elite cues if politicians and the news media explicitly connect immigrant-owned businesses to immigration-related cultural or economic anxieties (Lenz, 2012).

3 Context

To test these hypotheses, I focus on contemporary France, a country with a long history of immigration marked by its colonial history. Over the twentieth century, several waves of immigration saw residents of French colonies move to Metropolitan France, in particular from North and West Africa, and from Southeast Asia. This trend continued after independence into the twenty-first century and has been complemented by immigration from other regions, in particular since the mid-2010s refugee crisis.

The state has established progressively more restrictionist policies and institutions, including major restrictive laws in 1993, 2011, and 2024 and a dedicated Immigration Ministry in 2007 under President Sarkozy. The integration or assimilation of immigrants, in particular from Muslim-majority countries, has become a salient focus of political debate and a favored talking point of conservative politicians. In 2004, the right-wing government passed a headscarf ban in public schools (Abdelgadir and Fouka, 2020), and bills attempting to expand it have been introduced regularly since. The National Rally (RN, formerly National Front), France's main far-right party, has achieved increasingly strong electoral performance over the period (see Appendix B). It received the most votes of any single party in the 2024 European and legislative elections, and is widely considered a serious contender for the upcoming 2027 presidential election.

Immigrant businesses have been a particularly salient issue over the last decade. In 2015, Robert Ménard, one of the first far-right mayors in a major French town, declared that he wanted to rid his municipality of kebab shops (Le Monde, 2015). Several more instances of such political behavior have been observed and immigrant businesses are frequently mentioned as cultural disturbances by voters in the news media (Le Monde, 2022; France 24, 2024).

Note that in France, businesses do not generally require approval from local governments to open a business⁸. This is a significant scope condition: if native voters react negatively to the opening of an immigrant business, they cannot directly blame their municipal government for it. Similarly, if local leaders believe it is in their political interest to criticize immigrant

⁸They usually only need to formally register with the National Registry of Enterprises, and only need local approval when significant construction is required. The process of creating a company in France is outlined in Appendix C.

businesses, they can emphasize the fact that this was done against their own preference.

France is a major economy with a long history of immigration and a rapidly rising far-right party, making it an interesting setting to study the impact of immigrant businesses. However, the theory discussed above should be relevant in a broad set of contexts in which economic and cultural tensions related to immigration are salient, as is currently the case across Western democracies.

4 Data and Measurement

4.1 Data Sources

Electoral Outcomes

To measure electoral support for the far right, I use the vote share of the National Rally (RN) in elections at the municipal level⁹. I calculate the vote share as the proportion of all votes going to the RN and ignore variation in turnout, but results do not change if I use the proportion of all registered voters instead. I do not include other far-right parties who have consistently scored under 1% of the vote. For two-round elections, I only include results from the first round, even if the RN is present in the runoff, to avoid having to account for vote transfers. In the main instrumental variable specification described in Section 5, I use change in the RN vote share between the 2009 and 2019 European elections¹⁰. In the OLS specification used for some of the mechanism in Section 7, I use every election between 2008 and 2021. Appendix B shows the evolution of the RN vote share over time and its distribution across space. The figures show a steady national increase as well as a significant amount of heterogeneity between regions.

⁹Appendix A displays all the French elections held since 2000.

¹⁰I use these European elections to leverage a long between-election period while maximizing the availability of control variables. However, European elections have been described as “second-order” elections with different logics of participation, potentially encouraging protest votes (Reif and Schmitt, 1980; Hix and Marsh, 2007; Hobolt and Wittrock, 2011). I show that the results are robust to using the 2007 and 2017 Presidential elections instead in Appendix O.

Location of Businesses

I extract data on all individual businesses available as of July 2024 using the Application Programming Interface (API) of the French National Institute of Industrial Property, providing access to every entry of the National Registry of Enterprises (RNE). RNE servers are updated automatically every time companies formally register corporate milestones, including incorporation, changes in ownership structure, and cessation of activities. All private commercial, artisanal, agricultural, and independent companies are listed¹¹. To each company is associated a list of *establishments*, as illustrated in Appendix C. An establishment is an economic entity operated by the company. Establishments operationalize the concept of “business”. For a small shop owner, the company and the establishment may be one and the same. However, many larger companies operate several establishments—from a few local stores owned by a family company to hundreds of gas stations owned by a major oil company. For each establishment, RNE data records the days activities started and ended, the exact address, and a code referencing the industry of economic activity. I geocode all businesses using the National Address Bank API of the French government. Figure 1 illustrates the detailed data for one municipality.

Industries

The activity of each business is identified by an activity code (EPA code). There are a total of 732 unique EPA codes. I distinguish visible and non-visible businesses by classifying as “storefront” all retail and hospitality (restaurants, hotels, etc.) businesses using their EPA code. “Hidden” businesses include back-end economic activities such as production, wholesale trading, and non-storefront services. Because visibility is key in the theoretical framework introduced above, I focus on storefront businesses in the main specification. I also produce estimates for hidden businesses in Section 7.

¹¹Omitted entities include public-sector companies and non-profit organizations.

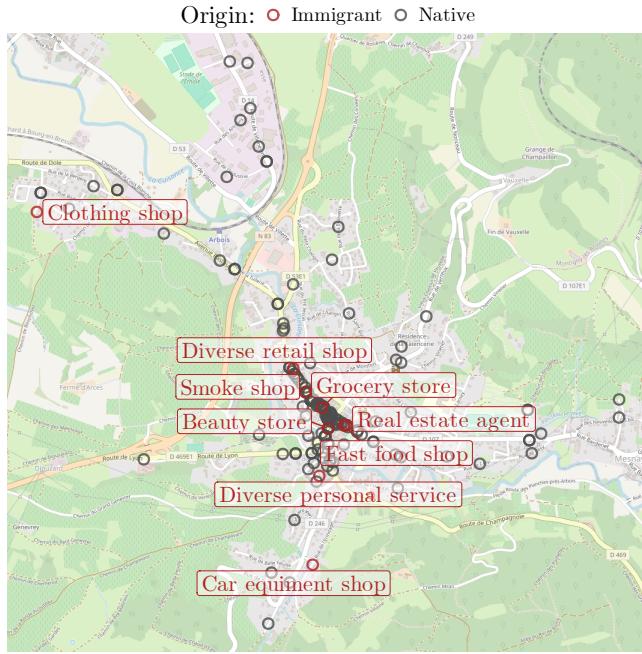


Figure 1: Detailed Map of Businesses in One Municipality

Detail of the 152 businesses open in a small rural town (Arbois, Jura). The 9 immigrant-owned businesses (see classification approach below) appear in red, and a label indicates their occupation as specified by the EPA industry codes registered in the RNE. Their exact location is obtained using the National Address Bank API of the French government.

Origins of Business Owners

Identifying which businesses are owned by immigrants presents a central challenge in the context of France. Citizenship is reported for a small subset of entrepreneurs in the RNE but is non-systematic. Moreover, citizenship is not the appropriate construct to characterize individuals that natives consider to be immigrants. Second-generation citizens and their descendants may still be perceived as immigrants by natives and are regularly targeted by anti-immigration rhetoric (Le Monde, 2023). A more general measure is therefore needed. However, a 1978 law, in conjunction with a 2007 Constitutional Council decision, prohibits the systematic collection of data on race, ethnicity, and religion. Appendix D provides more context on this law.

To overcome this, I predict the origin of every business owner using the proprietary algorithm of a partner organization, *Namsor*, classifying individuals by geographic origin based on their first and last names. The algorithm uses machine learning and linguistic analysis, has been used in previous academic research, and has been shown to perform well against

standard benchmarks (Bursztyn et al., 2022; Sebo, 2022)¹². The algorithm takes into account the host country’s naming patterns. For example, a second-generation immigrant in the US or France may be more likely to be named John or Jean, respectively, than in their country of origin. However, they will likely keep their last name. Namsor’s “diaspora” model accounts for this specificity and weighs first names accordingly. The following analysis relies on the assumption that the classification error is not systematically related to the treatment or outcome variables of interest (Egami et al., 2024). I categorize as an immigrant any business owner whose predicted origin is non-European, because the national debate on immigration has focused on immigrants from outside Europe. This also alleviates potential concerns over within-Europe classification error. Appendix E provides more details on the algorithm and reports validation tests. Figure 2 shows all the immigrant businesses in France.

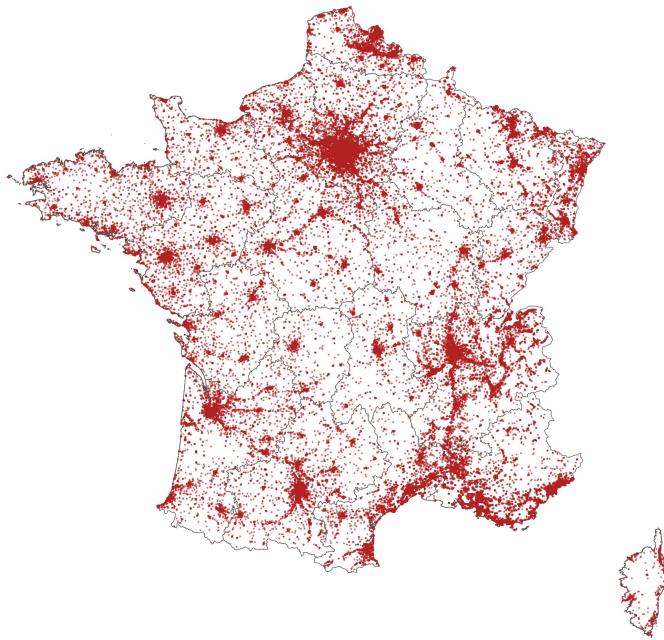


Figure 2: Immigrant-owned businesses in France

This figure displays the more than 360,000 immigrant-owned businesses open in France as of January 2024. Each dot represents a business.

Figure 3 displays the ten industries that show the largest difference between immigrant and native ownership. At one end of the spectrum, 15% of immigrant storefront businesses are fast food shops, for only 4.5% of native businesses. At the other end, 9% of native businesses

¹²Namsor’s algorithm is available for testing on the organization’s website.

are real-estate agencies, compared to 3.5% of immigrant businesses. Appendix F provides more detail on the industries most represented among them (Figure 16) and on the prevalent origins predicted among immigrant business owners (Figure 17). These figures show that a majority of immigrant business owners come from Muslim-majority countries, reflecting French colonial history, and that only about a quarter of all businesses owned by immigrants are in the food industry.

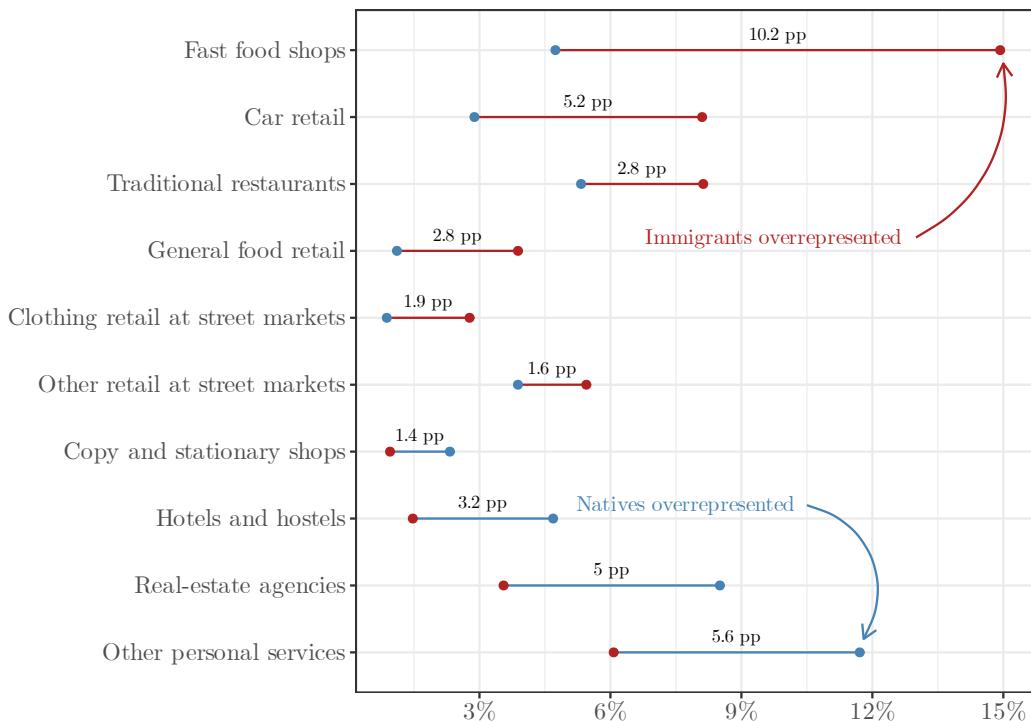


Figure 3: 10 Largest Industry Differences between Immigrants and Natives, January 2024

Blue points represent the proportion of all native storefront businesses operating in a given industry in January 2024. Red points represent the proportion of immigrant businesses in that same industry. The segment between the two points illustrates the difference in specialization. Only storefront industries are displayed.

A fundamental premise of this paper is that the presence of immigrant-owned businesses is not a perfect function of the presence of immigrants among residents. Figure 18 in Appendix G shows that while the proportion of businesses owned by immigrants is strongly explained by the foreign-born share of the population, significant variation exists: at every level of immigrant presence, some municipalities have very small proportions of businesses owned by immigrants while others have very large ones¹³. Moreover, Figure 19 in the appendix

¹³The literature on ethnic entrepreneurship suggests that while immigrant businesses may find larger markets

shows that the share of immigrants among businesses has grown at a much faster rate than the share of immigrants among residents.

Other Variables

Data for additional municipal-level covariates including employment, education, population, and the share of foreign-born residents, come from the French National Institute for Statistics and Economic Studies (Insee). Geographic data on infrastructure and roads come from the French National Institute of Geographic and Forest Information (IGN).

4.2 Measure of Exposure

Contact with immigrant-owned businesses is the key treatment of interest. Natives update their attitudes by *being exposed to*—seeing, purchasing from, hearing about—businesses in their community. I conceptualize a municipality’s exposure to immigrant-owned businesses as the sum of businesses in and around that municipality that are owned by immigrants, weighted by the inverse distance to the center of the municipality within a radius of 10 km. I use the log of this sum to account for the right skew of the distribution, and control for the equivalent weighted sum of native businesses in the analysis. Throughout the analysis, I also adjust for the logged population in the corresponding areas. For all businesses $i \in 1, \dots, K$ within distance $d_{m,i} \leq 10$ (km) of municipality m , where $G_i = 1$ if a business is immigrant owned, exposure is thus defined at the level of municipality m as:

$$\text{Exp}_{m,t} = \log \left[\sum_{i=1}^{K_m} w_{m,i} \mathbb{1} \{G_i = 1\} \right] \quad \text{where} \quad w_{m,i} = \frac{1}{1 + d_{m,i}}$$

I do not restrict K to the businesses located within a municipality’s border, because residents are not confined to their municipality of residence and often buy from businesses in neighboring municipalities, in particular when few or no businesses are operating in their own municipality. However, the weights capture the idea that being exposed to businesses

in ethnic enclaves, they may also find fiercer competition (Fairlie and Lofstrom, 2015; Kerr and Mandorff, 2020). Moreover, entrepreneurs in the most represented storefront industries are likely to seek markets beyond their own enclaves, given native demand for these goods and services.

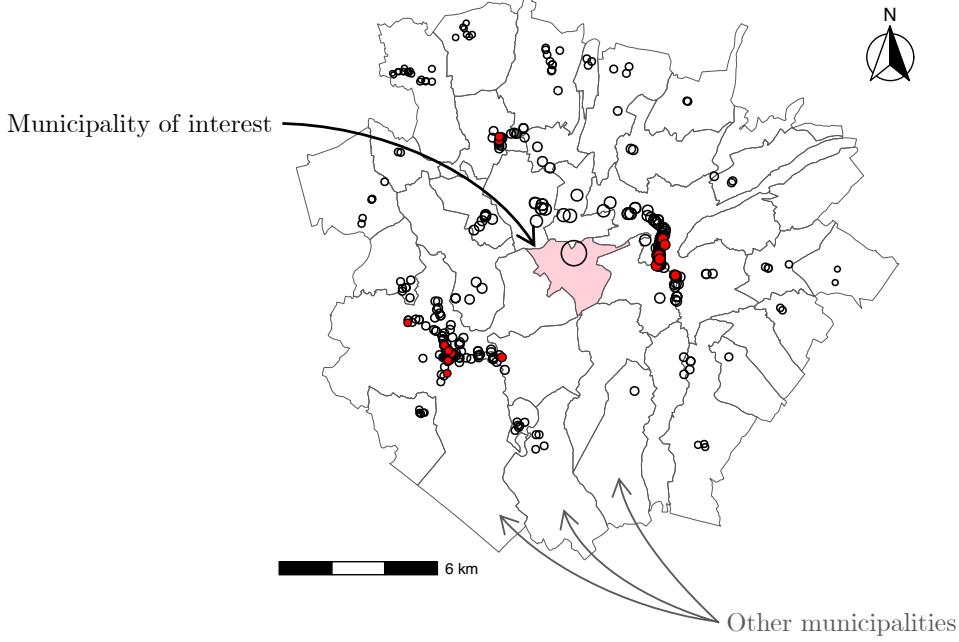


Figure 4: contribution of each business to the measure of exposure

Contribution of each business to the exposure measure for one municipality (Pretein, Jura), shaded in pink, in 2024. The borders of surrounding municipalities are displayed around it. Each point or circle represents a business. Their size is proportional to the weight $w_{m,i}$ and thus smaller for businesses farther away from the municipality. Black circles represent native-owned businesses, and red dots represent immigrant-owned businesses.

that are closer to a municipality matters more to this municipality's residents¹⁴. I compute the measure of exposure every year between 2009 and 2022 to create a municipality-year dataset for over 31,000 municipalities and over 20 million businesses. Figure 4 illustrates how businesses are accounted for in the calculation of Exposure_m for a given municipality: $\text{Exp}_{m,t}$ is equal to the logged sum of the red weights. The average municipality in the data is exposed to 18 immigrant businesses, out of a total of 86 businesses. Bear in mind that this includes large urban municipalities.

¹⁴ d is computed as the distance from the municipality's downtown area, estimated with density-based clustering using the municipality's buildings. This ensures that while every business in the municipality proper is equally weighted, businesses located in the outskirts of a municipality away from residential buildings receive lower weights.

5 Identification of Aggregate Effect

The main empirical goal of this paper is to estimate the net effect of an increase in exposure to immigrant businesses on vote for the far right at the municipal level, without capturing the effect of the presence of immigrants or of general economic conditions. A key empirical challenge is that the location of immigrant businesses is endogenous to a whole host of factors including the presence of immigrants and local market conditions such as demand, competition, and labor. To address this, since no policy change provides a clear discontinuity that disproportionately affected immigrants, I use an instrumental variable approach¹⁵.

The estimand can be understood intuitively as a Local Average Treatment Effect (LATE): the average treatment effect of the change in exposure to immigrant businesses on the change in vote for the far right, between 2009 and 2019, among municipalities where the number of businesses increased *because* of the instrument (Angrist and Pischke, 2009)¹⁶. The LATE is identified under the standard IV assumptions: instrument exogeneity, exclusion restriction, monotonicity, and relevance. I discuss potential violations of these assumptions in the following section.

Substantively, the LATE here is the effect of an exogenous increase in exposure due to the instrument, as opposed to an increase in exposure that may be due to confounders such as the number of immigrants residing in the municipalities or the local growth in economic activity more broadly. The estimate can thus be interpreted as the net effect of the theoretical channels introduced in Section 2, which are specific to immigrant businesses, and not as a proxy for other immigration-related variables.

¹⁵Intuitively, instrumental variable approaches identify an exogenous variable (the instrument) that causes variation in the endogenous treatment. Under some assumptions, the variation generated in the treatment can then be interpreted as exogenous, and used to estimate the causal effect of the treatment on the outcome.

¹⁶Note that the output of a two-stage least squares estimation with a continuous instrument cannot be formally interpreted as the classic LATE from Angrist and Pischke (2009), which requires a binary instrument. Instead, Alvarez and Toneto (2024) recommend interpreting it as a weighted average of treatment effects for *marginal compliance groups*. In my case, these groups can be understood for municipalities as defined by the minimum value of the instrument required for their immigrant business exposure to increase. This “weighted LATE” approach gives more weight to units that have more variation explained by the instrument, i.e., units that provide more information on the relationship between the treatment and the outcome.

5.1 Shift-Share Instrument

First developed by Bartik (1991), a shift-share instrument is the combination of a set of exogenous shocks common to all units (the *shift*) with unit-specific characteristics (the *share*) determining how the different shocks are weighted for each unit¹⁷. In this application, the units are municipalities. The *shift* is a set of exogenous shocks common to all municipalities across France: for each industry, the rate at which the number of immigrant businesses grows, adjusted for the rate at which native businesses grow in that same industry. This adjustment ensures that the instrument is predictive of immigrant business creation but not native business creation, as this would violate the exclusion restriction. Finally, the *share* is the industrial distribution of the businesses present in a municipality in 2005. This reflects the idea that a municipality with a higher concentration of businesses in industries in which immigrants tend to specialize will receive more of the immigrant businesses created over the period. For each municipality, the instrument is therefore a *weighted average* of this set of common shocks in which the weights are defined by the municipality's pre-existing industrial composition. Before introducing the instrument formally, I briefly introduce its substantive logic.

Substantive Logic of the Instrument

This new shift-share instrument leverages two theoretical observations. First, immigrants tend to specialize in different industries than natives nationally, as illustrated on Figure 3. This is in part due to historical factors: Bonacich (1973) argues that minorities are pushed out of desirable occupations and industries because of discrimination, and tend to fill economic gaps that social structures in the host economy leave vacant¹⁸. It is also due to network effects: vertical integration between wholesale suppliers and retailers (Bonacich, 1973), as well as in-group sharing of knowledge, skills, and connections (Kerr and Mandorff, 2020), provide immigrants with comparative advantages in some industries.

Second, industries tend to agglomerate in specific locations. This is first because the presence of one business lowers the cost of entry for another business in the same industry be-

¹⁷More formally, Borusyak et al. (2022) define shift-share instrumental variables as “*weighted averages of a common set of shocks, with weights reflecting heterogeneous shock exposure*”.

¹⁸In his *Theory of Middleman Minorities*, Bonacich famously uses the examples of Jews in Europe who were unwelcome in some industries and instead entered financial trades that Catholics rejected for religious reasons.

cause it decreases the marginal cost for suppliers or transportation (Ellison and Glaeser, 1997; Glaeser et al., 2010). This is a self-perpetuating cycle as concentration of businesses attract their suppliers, which may further reduce costs. Local concentrations of industry-specific labor (with particular skills) may also make a location more attractive to businesses in certain sectors. Finally, exogenous geographic determinants may make specific location more attractive to entrepreneurs in specific industries (Ellison and Glaeser, 1997): car repair shops may be located closer to highways, fast-food shops in areas where individuals work, and hotels in scenic locations attracting tourists.

This may appear counterintuitive at first: entrepreneurs could instead create businesses perceived as missing in a location. However, such absence often reveals local comparative disadvantage, or insufficient local demand for goods and services (Davis and Weinstein, 2003; Jaravel, 2019). In Appendix I, I show evidence of industrial persistence by showing that the municipal composition of industries in 2000 predicts the composition of the businesses locally created over the following decade.

The logic of the instrument can be understood through the following two stylized examples. Take a municipality that has historically had a greater concentration of car dealerships in and around its municipal boundaries. New entrants into the car dealership industry may be more likely to establish their footprint in this area to benefit from local demand and suppliers. Assume that nationally, immigrants have disproportionately specialized in the creation of car dealerships from 2009 to 2019, perhaps due to network effects as explained by Kerr and Mandorff (2020). In this instance, the number of immigrant businesses in and around that municipality is more likely to increase between 2009 and 2019, as immigrant entrepreneurs will strategically choose this municipality. This is independent of other political or immigration-related characteristics of the municipality. Take another municipality that has historically had a greater concentration of real estate agencies. Assume that nationally, natives have disproportionately specialized in the creation of real estate agencies. In this instance, the number of immigrant businesses in and around that municipality is not likely to increase between 2009 and 2019, because increasing demand or turnover primarily lead to the creation of native businesses.

I use the 2009-2019 time frame for several reasons. First, this allows me to capture slow changes in the industrial specialization of immigrants. One documented example is the slow specialization of US Vietnamese communities in nail salons in the 1970s and 1980s (Kerr and Mandorff, 2020). Using a shorter time frame does not capture patterns of specialization sufficiently to power the instrument, creating weak-instrument limitations. Second, the data availability of most covariates is limited before 2009, making it challenging to select an earlier start year. Finally, the idiosyncratic shocks associated to the Covid-19 pandemic in 2020 make the interpretation of the effect cleaner if the period ends in 2019. I therefore use, as outcome variables, change in support for the far right between the 2009 and 2019 European elections at the municipal level. Using the same election type also ensures some degree of consistency in the logic of voter behavior.

The instrument thus estimates the effect of change in exposure to immigrant businesses due to the strategic choice of location made by immigrant entrepreneurs, given the exogenous industry in which they tend to create their businesses. Exogeneity relies on variation in immigrant specialization explained by GDP growth in immigrants' countries of origin. The key assumption is that the instrument is independent of municipal characteristics that may themselves influence support for the right.

Formal Construction of the Instrument

The instrument Z_m is formally defined as follows, where m indexes the municipalities

$$Z_m = \sum_{j=1}^J S_{m,j} \tilde{G}_j^{immig} \quad (1)$$

Equation 1 defines the instrument as a weighted average of residual immigrant business growth across industries, weighted by municipalities' pre-existing industrial composition. $S_{m,j}$ is the share of municipality m 's businesses that operate in industry j in 2005, and \tilde{G}_j^{immig} is the *residual* growth of immigrant businesses in industry j nationally between 2009 and 2019. This residual growth isolates industry-level trends in immigrant entrepreneurship from the general growth of an industry. I operationalize this by regressing national immigrant busi-

ness growth on native business growth across industries, and using the residuals to strip out industry-wide demand shocks that affect both groups:

$$\tilde{G}_j^{immig} = G_j^{immig} - \hat{G}_j^{immig} \quad \text{where} \quad \hat{G}_j^{immig} = \alpha + \hat{\beta}G_j^{native} \quad (2)$$

In Appendix J, I provide descriptive information about the instrument, including the industries most represented in \tilde{G}_j^{immig} and a map of the municipalities with the largest share of these industry in their local distribution of businesses $S_{m,j}$.

The identification of the estimand requires the instrument to be exogenous. Borusyak et al. (2022) decomposes this into two primary assumptions: 1) quasi-random shock assignment and 2) many uncorrelated shocks¹⁹. The key assumption, quasi-random shock assignment, requires no correlation between industry-specific growth and other national factors affecting support for the far right differentially for different municipality-level industry compositions. To enhance confidence in this assumption, I use the value of \tilde{G}_j^{immig} predicted by the average GDP growth in countries most represented among immigrants in each industry j in 2005 instead of the observed value. I elaborate on this approach in Appendix K, but the findings are robust to using the observed value of \tilde{G}_j^{immig} directly²⁰.

Identifying the LATE also requires satisfying the exclusion restriction: $Z_m^{industry}$ must not affect the outcome through channels other than the increase in immigrant businesses. The residualization step introduced in Equation 2 is meant to improve the plausibility of this assumption. To further assess its credibility, I show in the next section that the instrument does not predict variation in two other major variables at the municipal level: the number of foreign-born residents and the number of native businesses.

I estimate the LATE using two-stage least squares (2SLS). The first stage is estimated using

¹⁹In this case, Assumption 2) requires sufficient variation in the distribution of industries across municipalities. Following their recommendation, I estimate the distribution's inverse Herfindahl–Hirschman index $1/\sum_{j,m} s_j^2$ and obtain an effective sample size of 10.7, which can be interpreted as a shock-level regression with 11 equal-sized industries. Borusyak et al.'s (2022) simulation studies suggest that the estimator performs well asymptotically with an effective sample size of 20: 10.7 is at the lower end of the distribution of reasonable values.

²⁰In Appendix K, I also replicate the analysis with a nonparametric bootstrap-based standard error estimator allowing me to propagate the uncertainty contained in this “stage 0” in which I predict the value of immigrant industry development. I sample municipalities with replacement to construct a bootstrap distribution and show that results for both first and second stages are robust to propagating this uncertainty explicitly.

the following regression:

$$\Delta Exp_m = \delta_m + \pi Z_m + X'_m \lambda + v_m \quad (3)$$

Where ΔExp_m stands for the difference in exposure to immigrant-owned businesses between 2009 and 2019, as defined in Section 4.2. δ_m are department fixed effects and X_m is a vector of municipality-level covariates at baseline (2009) that includes the logged population, the logged number of native-owned businesses, and the logged number of foreign-born residents in and around the municipality, to ensure that changes in exposure are not merely reflecting demographic shocks, the local unemployment rate to account for economic conditions, and the proportions of college-educated and secondary-school-educated residents. I also include the logged distance to the closest highway, which has been shown to influence business development. The second stage is estimated as follows:

$$\Delta RN_m = \eta_m + \beta \widehat{\Delta Exp}_m + X'_m \theta + \varepsilon_m \quad (4)$$

Where ΔRN_m is the change in the vote share of the National Rally between 2009 and 2019 and η_m are department fixed effects.

I estimate all the parameters using the `fixest` package in R, with a Conley standard error estimator allowing for spatial dependencies (Conley, 1999). I weigh the observations by municipality population at baseline, so the estimates can be interpreted to reflect the average voter's municipality.

5.2 Results

First Stage

The first column in Table 1 displays the estimate from the first-stage regression. It shows a strong positive relationship between the instrument and exposure to immigrant businesses, with a first-stage F statistic of 45.4 (reported in Table 2). To assess the plausibility of the exclusion restriction, I test whether the instrument predicts change in two other variables

that could plausibly affect the outcome. The second and third columns of Table 1 show that the value of the instrument is not correlated with change in the number of native businesses, nor with change in the foreign population of a municipality²¹.

Table 1: First stage and Placebo Checks

	First Stage		Placebo
	Δ Exposure (log)	Δ Nat. Bus. (log)	Δ Foreign Pop. (log)
Instrument	0.57*** (0.15)	-0.06 (0.04)	0.35 (0.21)
Dist. to highway (log)	-0.02*** (0.01)	-0.01*** (0.00)	0.00 (0.01)
Native businesses (log, 2009)	-0.09*** (0.02)	-0.15*** (0.01)	0.00 (0.03)
Foreign pop. in mun. (log, 2009)	0.01** (0.00)	-0.00 (0.00)	-0.68*** (0.01)
Foreign pop. around (log, 2009)	-0.01 (0.01)	0.01*** (0.00)	0.03 (0.02)
Population (log, 2009)	0.03* (0.02)	0.09*** (0.01)	0.91*** (0.04)
Population around (log, 2009)	0.03 (0.02)	0.00 (0.01)	0.07 (0.05)
Prop. unemployed (2009)	-1.25*** (0.37)	-0.20** (0.08)	5.10*** (0.66)
Prop. college graduate (2009)	-0.16 (0.11)	0.43*** (0.07)	-0.51** (0.21)
Prop. HS graduate (2009)	0.23 (0.28)	0.81*** (0.09)	-0.23 (0.54)
Urban/Rural controls	Yes	Yes	Yes
Department FE	Yes	Yes	Yes
Num. obs.	33721	33721	33721
R ²	0.06	0.47	0.33
Adj. R ²	0.06	0.47	0.32

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

The instrument is described in Equation 1. Column 1 shows the first-stage estimates from Equation 3. Columns 2 and 3 show placebo first-stages supporting the plausibility of the exclusion restriction.

²¹While the coefficient for change in foreign population is not significant, its magnitude is non-negligible. In Appendix M.1, I show that controlling for this variable in the second stage directly does not affect the results.

Second Stage

Table 2 displays the results of the second-stage regression. The third column reports the coefficient of interest and indicates that a one-unit change in the difference in log exposure to immigrant businesses between 2009 and 2019 causes a 7 percentage-point increase in National Rally (RN) vote share. The difference in logs can be interpreted as follows: a doubling of the number of immigrant businesses to which a municipality is exposed causes a five percentage-point increase in the RN vote share. For context, in 2009, the average municipality in the data was exposed to six immigrant businesses only. This coefficient is of the opposite sign to the coefficient for the share of the population that is foreign-born, which includes voters unlikely to support the far right.

The magnitude of the effect can further be contextualized using other coefficients from Table 2. Using the coefficient on the proportion of college graduates at baseline (-0.35), the five percentage-point effect caused by a doubling in exposure to immigrant businesses corresponds to a 15-percentage-point decrease in the baseline proportion of municipality residents that are college-educated. In Appendix N, I show that the results are driven by new voter mobilization: the increase in support for the RN as a proportion of all registered voters is similar in magnitude to the effect on increased exposure on turnout.

In the framework introduced in Section 2, immigrant businesses are conceptualized as symbols, producers, employers, and spaces. The present results show that in aggregate, an increase in exposure to immigrant businesses causes native backlash, measured as an increase in support for the far right. This suggests that the mechanisms leading to backlash, on average, may dominate. Natives may resent symbols of cultural heterogeneity, may worry about the presence of immigrants revealed by the existence of a businesses with cultural markers, may internalize the economic threat to native businesses presented by this new competition, and may believe that businesses will attract a concentration of unwelcome immigrants in their neighborhood. I test these mechanisms in Section 7.

Table 2: Reduced Form and Second Stage

	Δ RN Vote Share		
	Reduced Form	OLS	2SLS
Instrument	0.041*** (0.010)		
Δ Exposure (log)		0.001*** (0.000)	0.073*** (0.026)
Foreign pop. (log, 2009)	-0.001*** (0.000)	-0.001*** (0.000)	-0.002*** (0.001)
Foreign pop. around (log, 2009)	-0.002** (0.001)	-0.002** (0.001)	-0.002 (0.001)
Population (log, 2009)	0.002* (0.001)	0.003** (0.001)	-0.000 (0.002)
Population around (log, 2009)	0.004*** (0.002)	0.004*** (0.002)	0.003 (0.002)
Prop. unemployed (2009)	0.181* (0.094)	0.179* (0.095)	0.272*** (0.099)
Prop. college graduate (2009)	-0.349*** (0.049)	-0.350*** (0.049)	-0.338*** (0.054)
Prop. HS graduate (2009)	-0.166*** (0.044)	-0.167*** (0.044)	-0.183*** (0.054)
Native businesses (log, 2009)	-0.011*** (0.002)	-0.013*** (0.002)	-0.005 (0.003)
Dist. to highway (log)	-0.001** (0.001)	-0.001** (0.001)	-0.000 (0.001)
Urban/Rural controls	Yes	Yes	Yes
Department FE	Yes	Yes	Yes
F (first stage)			45.4
Outcome Mean	0.210	0.210	0.210
Num. obs.	33720	33720	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

Column 1 shows the reduced form regression of the outcome on the instrument. Column 2 shows the naive OLS regression of the outcome on the endogenous treatment (exposure to immigrant-businesses). Column 3 reports the two-stage least squares estimate.

Robustness Checks

In Appendix K.3 I show that estimates from Tables 1 and 2 are robust to using the realized value of national industry-specific growth in immigrant businesses instead of the one predicted by GDP growth in origin countries. In Appendix L, I show that the results are robust to a version of the instrument constructed with leave-one-out jackknife sampling, to ensure that the value

of the instrument in each municipality is not explained by endogenous business creation in that municipality.

In Appendix M.1, I show that the instrument is balanced on the baseline values of most other variables of interest, and that second-stage results are not affected by the inclusion of unbalanced covariates. As a further test of exogeneity, I show in Appendix M.2 that the instrument is not predictive of change in exposure for a 2001-2009 placebo period. In Appendix M.3, I discuss the monotonicity assumption and show that the results are robust to the exclusion of some relevant subsets of municipalities.

In appendix O, I show that the results are robust to using the 2007 and 2017 presidential elections instead of the 2009 and 2019 European elections. In appendix In Appendix P, I show that the results are robust to using a measure of exposure with different weights for distance to the municipality.

As a sanity check, Appendix Q reports estimates of the second stage for other major political parties. None of the coefficients for the treatment are statistically significant at the 5% level. Moreover, the estimates all point in the expected directions: the coefficients for left-wing parties, who promote less inclusionary policies, are negative. The coefficient for the mainstream conservative party—the Republicans—is positive but smaller in magnitude than the coefficient for the far right. This is not surprising considering that the Republicans have increasingly promoted anti-immigration policies in an effort to compete with the far right.

Note that the simple OLS coefficient, while significant and positive, is very weak in magnitude. While fully explaining why the OLS result may be biased in the opposite direction is not straightforward, the difference in estimand and related principal stratification help to do so. 2SLS estimates the LATE, which is the average treatment effect *for compliers*: those municipalities where immigrant businesses were created due to the strategic entrepreneurial considerations captured by the instrument. In contrast, OLS estimates an average including the LATE (for compliers) as well as the unidentified effects for other compliance strata including *always-takers* and *never-takers*. Here, always-takers are municipalities that will receive new immigrant businesses regardless of their baseline industrial composition. This may be because they have a large population of immigrant residents, making them unlikely to

support the far right even with many new immigrant businesses. Similarly, never-takers may include municipalities where the far right is notoriously powerful and growing, making immigrants reluctant to opening shops. Accounting for these may therefore bias the results in the opposite direction.

5.3 Alternative Identification Approaches

I show in this section that the results are consistent when I explore two alternative identification approaches exploiting the panel structure of the data using estimators from the difference-in-differences (DID) family. The structure of the data does not lend itself well to modern DID estimators (e.g., De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Callaway et al., 2024) because the treatment (exposure to immigrant-owned businesses) is continuous and fluctuates up and down across units (municipalities), making it impossible to approach estimation as a standard case of staggered adoption.

I instead estimate two quantities of interest. First, I report classic two-way fixed effects (TWFE) estimates. Recent work has criticized the use of TWFE estimators in panel analysis in particular due to the implausibility of assumptions of strict exogeneity and treatment effect homogeneity (Athey and Imbens, 2022; Borusyak et al., 2024; Callaway and Sant'Anna, 2021). However, TWFE estimates constitute a useful benchmark and Chiu et al. (2025) have shown that they are often not qualitatively different from more robust estimators. Moreover, the causal interpretation of the estimates still holds under an albeit stronger conditional ignorability assumption with the fixed effects. The ATE is estimated using the following OLS regression:

$$RN_m = \alpha_m + \gamma_t + \beta Exp_{mt}^{\text{storefront}} + \delta Exp_{mt}^{\text{hidden}} + X'_{mt}\theta + e_{mt} \quad (5)$$

Where α_m and γ_t are municipality and election fixed effects respectively and X' is a vector of controls identical to the one described in Section 4.1, except for distance to the closest highway, which is captured by the municipality fixed effects. Exp_{mt} is the exposure to immigrant businesses described in Section 4.2. I distinguish between storefront businesses and hidden

businesses.

Second, I adapt the approach introduced by Imai et al. (2023), which estimates an average treatment effect relying on a conditional parallel trends assumption by matching treated units to control units with similar covariate and treatment histories (which can include switches in and out of treatment). However, this approach does not lend itself directly to continuous treatments. I therefore adapt it by discretizing exposure to immigrant businesses into ordered quantiles to represent increasing levels of intensity.

I estimate the effect of moving from one treatment level to the next (e.g., from quantile 3 to quantile 4) by comparing units that experience this change to similar units that remain at the lower level, while adjusting for their prior treatment and outcome histories. By repeating this process across all adjacent treatment “levels”, I obtain a set of stepwise treatment effect estimates, which I combine using inverse-variance weighting to summarize the average effect of a one-bin increase. I replicate this estimation procedure across different quantile sizes, from two halves to ten deciles.

The results from both approaches are reported in Appendix R, along with more details on the second procedure. Estimates are consistent in sign and suggest that an increase in exposure to immigrant-owned businesses at the municipality level causes an increase in municipal support for the far right. The magnitudes are significantly smaller, though they are both likely to be lower bounds. Given that the IV estimates target a specific LATE, which is not generalizable, one way to think about the magnitude difference is that the general effect of exposure to immigrant businesses exists between these two bounds.

6 Survey Experiment

To further understand the aggregate effect and test some of the mechanisms presented in Section 7, I analyze the results of a preregistered online survey experiment conducted in France in August 2025. I briefly describe its design here and provide more information in Appendix S. I surveyed a sample of 1,400 French respondents using the Prime Panels platform provided by Cloud Research (Chandler et al., 2019)²².

²²The preregistration is accessible at <https://osf.io/3ydxk/>.

6.1 Treatment Conditions

Respondents were randomly allocated to one of four conditions by receiving none, one, or both of the following treatments.

Media Treatment

For the first treatment, respondents were asked to read a one-page newspaper article about local economic development in the town of Perpignan. The article shown to subjects in the control condition described the closure of a local bakery, and its upcoming replacement by a classic French brasserie. The article shown to subjects in the treatment condition described the upcoming opening of “halal restaurants”, and briefly reported on opposition shown by Perpignan’s mayor to these restaurants. The mayor of Perpignan, Louis Aliot, was also vice president of the National Rally from 2011 to 2018 and remains one of its recognizable faces. The two articles are shown in Appendix S.1.

Businesses Treatment

For the second treatment, respondents were presented with five restaurants in or around their municipality²³. They were asked whether they knew each restaurant and, if they did, whether they would recommend it to friends or family members. The population of restaurants was established by scraping data from restaurants with a Google Maps presence in France. To each Google Maps restaurant is associated one or several categories, for example: “Lebanese Takeout”, or “Japanese Restaurant”. Respondents in the control condition were shown five stereotypically French restaurants whose Google Maps labels included categories like “French Brasserie” or “French Bistrot”. Respondents in the treatment condition were shown restaurants typically associated to immigration from Muslim-majority countries, with labels such as “Kebab Shop” or “Halal Restaurant”. An example of business from each condition is included in Appendix S.2. Regardless of the condition, the five restaurants displayed were those closest to the center of the respondent’s municipality of residence. Each restaurant was shown with the first photo provided for it by the Google Maps API.

²³Respondents were asked to identify their place of residence earlier in the survey.

Crucially, I sought to estimate a business-specific treatment effect. One worry in interpreting the results could be that the presence of immigrant businesses is simply interpreted as a proxy for the presence of immigrants. To partial this out, I informed each respondent, before treatment, of the evolution in the number of foreign-born residents in their municipality between 2011 and 2021, according to national statistics, and of the share and number of immigrants 2021 present in their municipality in 2021. This helps interpret the estimated treatment effects as specific to businesses while holding constant perceptions of the local presence of immigrants.

6.2 Experimental Design

Respondents were block-randomized to ensure balance within blocks defined by whether their municipality of residence is rural or urban, and whether they supported Emmanuel Macron, Marine Le Pen, or neither in the 2022 Presidential Election. All respondents were shown the pre-treatment vignette revealing immigrant presence in their municipalities.

Several outcomes were collected: for the closest outcome to the observational study, respondents were asked about their trust in the National Rally. To capture the local political consequences of exposure, respondents also had to indicate with what probability they would vote again for their incumbent mayor. Finally, respondents were asked two series of questions using seven-point Likert scales: one about their attitudes toward immigrant-owned businesses in particular and one about their broader attitudes toward immigration and immigrants. Both of these were condensed into indexes where higher values mean less immigrant-friendly attitudes. I pre-registered hypotheses only about these four outcomes (trust in RN, support for mayor, and two indexes), but also tested exploratory hypotheses, as described below.

My estimands are the Average Treatment Effect (ATE) of each treatment as well as their interaction. For each outcome and each treatment (or interaction), I estimate a simple OLS regression of the outcome on the treatment and a model with demographic controls²⁴.

²⁴The covariates controlled for are: pre-treatment trust in Marine Le Pen, self-location on a left-right scale, change in the number of foreign-born residents in the respondent's municipality between 2011 and 2021, years of education, age, gender, income, and whether the respondent identifies as an immigrant themselves.

6.3 Main Results

I report here a subset of estimates, and discuss other outcomes in Section 7's discussion of mechanisms. Figure 5 reports the ATE on four outcomes: 1) respondents' trust in the National Rally (pre-registered), 2) the level of support for their incumbent mayor (pre-registered), 3) whether respondents select immigration as one of the top four topics they find concerning (not pre-registered), and 4) post-treatment belief on the recent trend in immigrant presence (on a one-has decreased a lot-to five-has increased a lot-scale, not pre-registered).

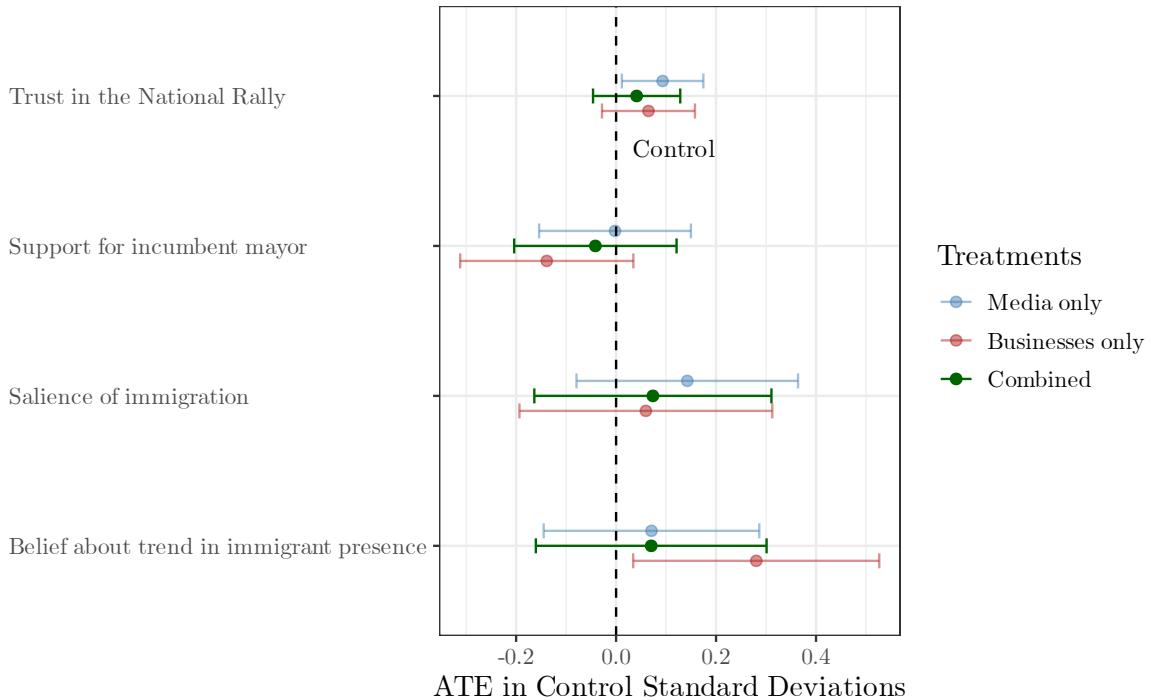


Figure 5: Main Experimental Estimates

This figure displays the average treatment effect (ATE) for the two treatments (media and businesses) and for their interaction, for four different outcomes.

Though they are not precisely estimated, the estimates for the first outcome support the results from Section 5. Respondents in both treatment conditions report higher trust in the National Rally. Both of these effects may shift voters toward the far right. The lack of precision may be due to the weakness of the vignette treatment: simple images embedded in a short survey, as opposed to repeated exposure, may not be able to shift strong priors on partisan trust. In addition, respondents in the control group may already have been aware of the existence of local immigrant businesses: on average, treated respondents indicated that they

already knew 1.27 of the immigrant businesses presented to them. Furthermore, because all respondents are given information about true immigrant presence, the true causal effect of exposure to businesses on support for the RN may be dampened if updating about immigrant presence is a mediator.

Estimates for the second outcome show that respondents exposed to the businesses treatment report lower support for their incumbent government. Because a majority of mayors come from mainstream parties (only a handful are RN members at the time of writing), this may benefit the far right electorally at both local and national levels. The third outcome suggests that the salience of immigration itself is not affected? The third outcome shows no significant effect, perhaps also because all respondents are already primed to think about immigration.

Finally, estimates for the last outcome indicate that respondents shown nearby immigrant businesses report believing that local immigrant presence has increased more. Note that this is true even though all respondents were shown the same facts about immigrant presence before treatment. One likely interpretation is that even knowing the truth, exposure to visual symbols of immigration distorts perceptions of immigrant presence. The coefficient on interacted treatments is in the same direction but not significant, perhaps because respondents shown the far-right article are more cautious in their responses.

7 Mechanisms

I combine observational tests with results from the survey experiment to explore the mechanisms that determine why and when exposure to immigrant businesses causes native backlash. This analysis is based on the framework and the hypotheses developed in Section 2.

7.1 Cultural: Evidence of Backlash against Businesses as Symbols

Hypothesis 1 predicts that immigrant businesses are more likely to lead to native backlash when they display symbols of group identity, and when they are owned by members of groups with whom there exists a more salient cultural conflict. In France, the immigration

debate has largely been centered on immigration from Muslim-majority countries²⁵. If this hypothesis is true, the relationship between businesses and native backlash should be stronger for businesses that are *visible* and owned by immigrants from Muslim-majority countries. If Hypothesis 4 is true, immigrant businesses should be less likely to lead to native backlash when they include socialization spaces that are not solely targeted at an in-group clientele. The businesses most conducive to socialization are plausibly food establishments. However, culturally-distinct food businesses may also carry the strongest symbolic valence. Their effect is therefore ambiguous.

To test these hypotheses, I re-estimate the two-way fixed effects model introduced in Section 5.3. The estimand, an average treatment effect of exposure on support for the far right, is identified under conditional ignorability²⁶. In Table 3, I present the estimates from Equation 5 but breaking down Exp_{mt} into Muslim-majority-owned businesses and other businesses in column A. In column B, I further break down Muslim-owned businesses into food businesses and non-food storefront businesses.

Column A of Table 3 suggests that the relationship between immigrant businesses and RN vote is driven by businesses owned by immigrants from Muslim-majority countries. The coefficient for this group is significant: a doubling of the number of businesses from Muslim-majority countries increases support for the far right by a quarter of a percentage point. As for the two-way fixed effects estimate of the aggregate effect presented in Section 5.3, this is likely to be an underestimate of the true effect. In contrast, no significant relationship exists between other groups and the outcome: hidden businesses from Muslim majority-country immigrants, and hidden businesses from others groups, are not associated with support for the far right at the conventional significance threshold.

Column B of Table 3 suggests that Muslim-owned food establishments, in particular, may drive the effect. However, the estimate on non-food businesses, although not precisely estimated, is also positive. The positive coefficients are consistent with Hypothesis 1: immigrant-

²⁵This has manifested in several ways, including debates over the right of Muslim women and girls to wear a hijab in public spaces (Abdelgadir and Fouka, 2020) or arguments about an “Islamо-Leftism” trend of research and activism in Universities.

²⁶A replication of the instrumental variable approach broken down by groups of businesses either does not satisfy the relevance assumption, or violates the exclusion restriction by predicting variation in exposure to both groups of businesses. No policy discontinuity exists that clearly affects one group of immigrants but not another.

Table 3: Linear Models: Businesses as Symbols

	RN Vote Share	
	A. By origin	B. By origin and type
Exposure - Muslim (storefront, log)	0.003** (0.001)	
Exposure - Muslim, food (storefront, log)		0.003*** (0.001)
Exposure - Muslim, non-food (storefront, log)		0.001 (0.002)
Exposure - Muslim (hidden, log)	-0.002 (0.002)	-0.003 (0.003)
Exposure - other (storefront, log)	-0.001 (0.001)	-0.001 (0.001)
Exposure - other (hidden, log)	-0.003* (0.002)	-0.003* (0.002)
Foreign pop. (log)	-0.001*** (0.000)	-0.001*** (0.000)
Foreign pop. around (log)	-0.009 (0.008)	-0.009 (0.008)
Population (log)	0.027** (0.012)	0.027** (0.012)
Population around (log)	0.038 (0.059)	0.036 (0.059)
Prop. unemployed	0.158 (0.132)	0.157 (0.133)
Prop. college graduate	-0.241*** (0.027)	-0.240*** (0.027)
Prop. HS graduate	0.430*** (0.090)	0.429*** (0.089)
Native businesses (storefront log)	0.022** (0.010)	0.023** (0.009)
Native businesses (hidden, log)	-0.104** (0.043)	-0.105** (0.042)
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Num. obs.	434048	434048
Adj. R ² (full model)	0.847	0.847
Adj. R ² (proj model)	0.059	0.060

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

owned restaurants, when they offer region-specific cuisines, carry the most identifiable symbols of group identity. In contrast, the positive sign is inconsistent with Hypothesis 4. Spaces where natives and immigrants can interact do not seem to mitigate the backlash associated

to exposure to immigrant businesses more broadly²⁷. As discussed in Section 2, it may be the case that natives believe that these socialization spaces attract a concentration of immigrants. If they perceive this to be undesirable, these spaces may in fact further backlash.

Overall, the estimates presented in Table 3 suggest results that are consistent with Hypothesis 1: immigrant businesses may trigger backlash because they are symbols of cultural heterogeneity, either because their presence changes the physical environment in a way that does not align with native preferences, or because they signal the presence of immigrant groups with whom there exists a broader cultural conflict. These results are not consistent with Hypothesis 4: the contact between natives and immigrants facilitated by immigrant businesses may not produce a reduction of prejudice or a positive effect on between-group interactions like broad applications of the contact hypothesis would predict. Allport's (1954) and Nathan and Sands's (2023) work helps understand this: customers in a common business do not interact in the pursuit of a goal, and the nature of their interaction is one that is relatively short and shallow.

In Appendix T, I show that these results are robust to an additional control for the presence of immigrants that includes French-born individuals and is constructed by predicting the origin of deceased individuals using publically available files. Given that the coefficient on the log number of *native* storefront businesses is also positive and significant, I replicate the results in Appendix U by redefining exposure as the proportion of storefront businesses that are immigrant-owned instead of their number, and show that the main results hold.

Experimental Results

Results from the survey experiment are consistent with this interpretation, and suggest that a large part of the symbolic effect of immigrant businesses may be driven by elite cues. Figure 6 shows the treatment effects for the attitudinal items related to identity and culture. The two treatment effects are systematically in the direction of worse attitudes toward immigrants and immigrant businesses, even though the businesses treatment is not significant. As explained above, this is perhaps because respondents in the control condition already knew of local

²⁷Section 7.3 and Appendix X show that natives constitute the main group of consumers on average even in immigrant-owned businesses.

immigrant businesses. The media treatment has a significant effect on respondents' tendency to perceive immigrants as an identity threat, as less integrated in French society, and to prefer typically French businesses. One interpretation of these results is that elite rhetoric activates a reaction to immigrant businesses when latent prejudice against certain groups exists.

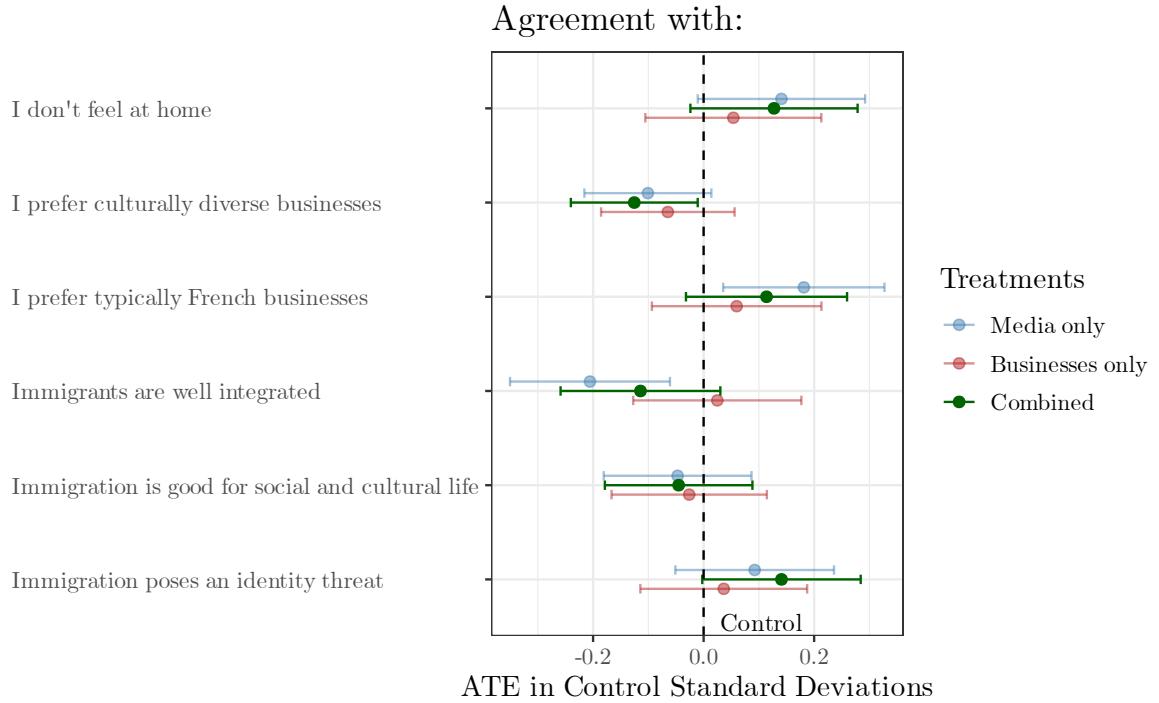


Figure 6: Survey Experiment: Treatment Effect on Cultural Attitudes

This figure displays the ATE for index components relevant to the symbolic mechanisms.

7.2 Economic: Limited Evidence of Negative Impact

If Hypothesis 2 is true, immigrant businesses should be less likely to lead to backlash when the product they offer improves the diversity of goods and services available locally. Hypothesis 3 suggests that immigrant businesses should be more likely to lead to backlash when their creation coincides with the closure of native businesses, fostering a sense of displacement. To test these hypotheses, I first re-estimate the instrumental variable specification of the main aggregate estimate, but with economic outcomes. I propose novel ways to measure these economic variables below.

Measures of Market Richness and Average Industry Competition

I propose measures of *market richness*, i.e., the variety of goods and services that a set of businesses provides to a community, and of *competition* among businesses within industries.

I measure *market richness* (MR) in community m as the sum of the squared, weighted proportion that each industry makes up of all businesses, adapting the Herfindahl–Hirschman Index, with businesses in municipality m indexed as $i \in \{1, \dots, K_m\}$, across all industries $j \in \{1, \dots, J\}$:

$$MR_m = 1 - \sum_{j=1}^J \left(\frac{\sum_{i \in j} w_{m,i}}{\sum_i^{K_m} w_{m,i}} \right)^2$$

The communities m correspond to the 10 km-radius areas around each municipality used to measure exposure, and the businesses are weighted by inverse distance to the municipality. Businesses are classified into industries using their EPA code as described in Section 4.1.

Market richness is a decreasing function of the concentration of businesses into fewer industries. A business reduces market richness if it belongs to a relatively more represented industry, and increases market richness if it belongs to a new or underrepresented industry. Hypothesis 2 suggests that natives should welcome immigrant businesses that increase market richness.

I measure average industry competition (AIC) among the businesses in a given area m by calculating the average of the Herfindahl–Hirschman Index of competition for each industry j , weighted by the proportion W_j of all businesses in community m operating in industry j . I assume that within industry, every business has the same market share $\frac{1}{n_j}$. Competition is therefore a weighted average of the squared inverse number of businesses in each industry. I consider the assumption of equal market share reasonable given that the EPA coding is specific: small produce shops are classified differently from grocery stores, and the assumption only needs to hold within each category.

$$AIC_m = 1 - \left\{ \sum_{j=1}^J W_{jm} \times \frac{1}{n_j^2} \right\}$$

AIC is a concave, increasing function of the number of businesses active in each industry. When new businesses are created in industries already represented, they increase competition. The more businesses already present in a given industry, the smaller the marginal increase in competition will be. Note that the community m often does not match the actual market served by a given business and is therefore not exactly analogous to other existing measures in the economics literature²⁸.

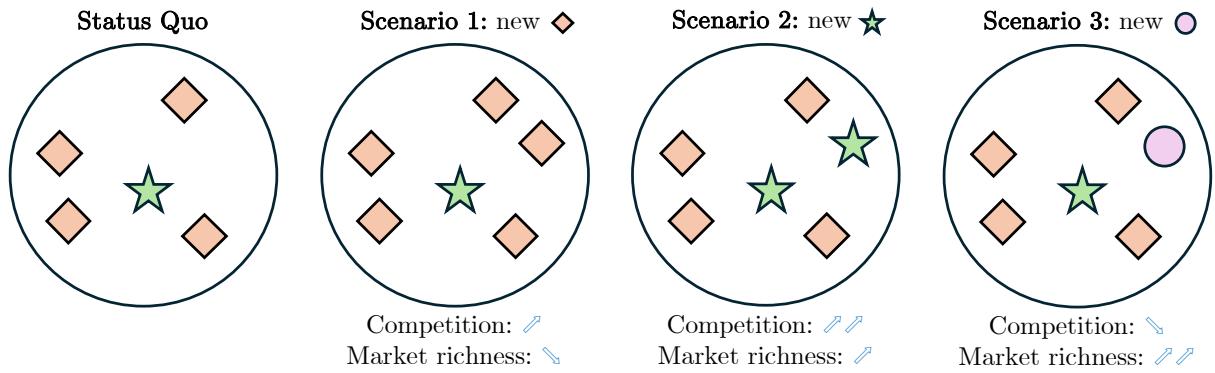


Figure 7: Illustration of the Competition and Market Richness Measures

Illustration of the concepts of Average Industry Competition (AIC) and Market Richness (MR). Each scenario considers a new business, varying the industry in which the business operates.

Figure 7 illustrates how different businesses affect the measures of competition and market richness. Different icons represent businesses with different activities (industries), and the circle represent a given community. In Scenario 1, the new business is in an already saturated industry, which increases competition at the margin, but decreases market richness²⁹. In Scenario 2, the new business is in an existing but previously monopolistic industry, which increases competition significantly and increases market richness. In Scenario 3, the new business is in an entirely new industry. This creates a new monopoly, which decreases average competition, but increases market richness significantly. Appendix H discusses these measures in further detail.

²⁸Instead, it is meant to capture natives' perception of competition within their community. According to Hypothesis 3, if natives believe that immigrant-owned businesses bring undesirable competition to businesses owned by native community members, exposure to such businesses may increase their support for the far right.

²⁹Note that market richness decreases, even though a new business is added and none is taken away, which may seem counter-intuitive. One way to think about market richness is as an opposite to market concentration. Market richness will increase more quickly if new industries business are introduced (as the numerator increases) than decrease if saturated ones increase (which only marginally increases the denominator). So in practice, the straightforward "richness" interpretation holds.

Results

I test whether change in exposure to immigrant businesses causes a change in Market Richness (MC) and Average Industry Composition (AIC) as well as in the rates of native and foreign-born unemployment. Results are displayed in Table 4.

Table 4: Second-Stage Stage Estimates with Economic Outcomes

	2SLS			
	ΔMC	ΔAIC	$\Delta \text{Nat. unemp.}$	$\Delta \text{Immig. unemp.}$
Δ Exposure (log)	0.03** (0.01)	-0.04** (0.02)	-0.01* (0.01)	0.03 (0.02)
Foreign pop. (log, 2009)	-0.00 (0.00)	0.00 (0.00)	0.00*** (0.00)	-0.00*** (0.00)
Foreign pop. around (log, 2009)	-0.00** (0.00)	-0.01*** (0.00)	0.00 (0.00)	0.00 (0.00)
Population (log, 2009)	-0.00** (0.00)	0.00 (0.00)	0.00*** (0.00)	0.00 (0.00)
Population around (log, 2009)	-0.00 (0.00)	0.01*** (0.00)	-0.00 (0.00)	-0.00 (0.00)
Prop. unemployed (2009)	0.06*** (0.02)	0.02 (0.03)	-0.36*** (0.03)	-0.23*** (0.05)
Prop. college graduate (2009)	-0.00 (0.00)	-0.01 (0.01)	-0.04*** (0.00)	-0.01 (0.01)
Prop. HS graduate (2009)	0.00 (0.01)	-0.05*** (0.02)	-0.05*** (0.01)	-0.04* (0.03)
Native businesses (log, 2009)	0.00* (0.00)	-0.01*** (0.00)	-0.00 (0.00)	0.00 (0.00)
Dist. to highway (log)	0.00** (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Urban/Rural controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
F (first stage)	45.4	45.4	48.0	41.6
Outcome Mean	0.01	0.03	0.01	0.01
Num. obs.	33721	33721	33721	31122

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

This table reports 2SLS estimates with economic outcomes: MC and AIC respectively stand for Market Richness and Average Industry Competition, as introduced earlier. None of the estimates suggests that an increase in exposure to immigrant businesses causes negative economic outcomes for natives.

The first column shows that an increase in exposure causes an increase in market richness, while the second column shows a decrease in average industry competition. This confirms

that immigrants tend to specialize in different industries from natives, and suggest that they contribute to local economies in ways that benefit consumers, by increasing the diversity of goods and services available, without threatening producers. The third and fourth columns show that immigrant businesses may cause a slight decrease in native unemployment, without significantly affecting immigrant unemployment. This suggests that native labor is not threatened by immigrant entrepreneurship.

Note that the effect on Average Industry Competition, while suggesting that immigrant businesses do not threaten existing producers, does not rule out that immigrant businesses the possibility that immigrant businesses may be displacing native businesses. The instrumental variable approach is design to partial out the effect on native businesses and therefore cannot directly speak to it, but the experimental results below suggest that natives are concerned by this possibility. In Appendix V, I combine the addresses of businesses with their closing and opening dates in the data to infer displacement to test the relationship between displacements and support for the far right. The sign suggests that they may trigger backlash, but the estimate is not significant.

Taken together, these results confirm that immigrant-owned businesses contribute positively to local economies. They enrich local consumer offering, do not pose a significant threat to native businesses, and marginally reduce native unemployment. In light of this, the negative aggregate effect shown in Section 5 can be understood in different ways. First, it could mean that even though natives do appreciate the economic contribution of immigrant businesses, this effect does not compensate for their negative symbolic effect. Second, it could mean that even though natives perceive the benefits of immigrant businesses, this does not affect their attitudes toward immigrant more broadly. Finally, it could be that natives misperceive the local economic impact of immigrant businesses.

Experimental Results

Results from the survey experiment displayed in Figure 8 are consistent with the latter interpretation. Respondents exposed to the media treatment report agreeing more with statements characterizing immigrant businesses as an economic threat. In particular, treated respondents

are more likely to believe that immigrant businesses pose a risk to other businesses and to the value of real estate. This is despite the fact that the far-right politician quoted in the media treatment article does not characterize immigrant businesses as an economic threat but as an identity threat. Here again, the businesses treatment alone does not significant shift attitudes. However, the sign indicates that priming about local immigrant-owned businesses may also have a similar negative effect on beliefs about their economic impact.

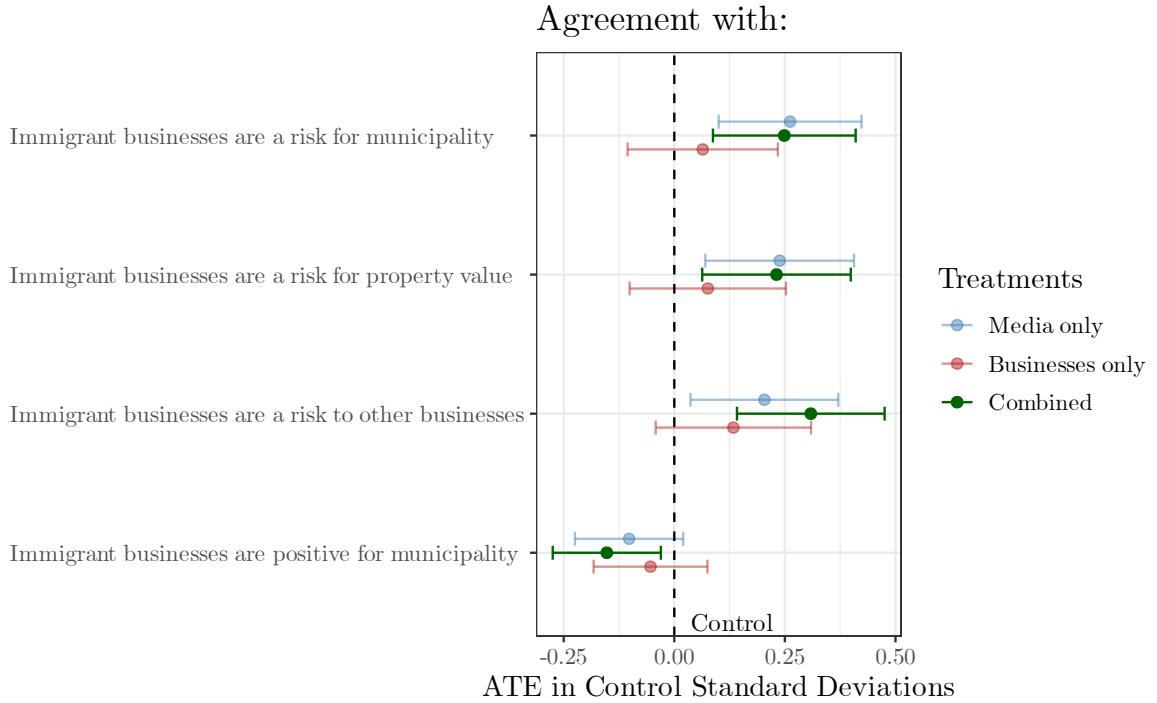


Figure 8: Survey Experiment: Treatment Effect on Economic Attitudes

This figure displays the ATE for index components relevant to the economic mechanisms.

In Appendix V, I run additional observational tests of the real-estate value argument. Although the sign is consistent with respondents' perceptions, suggesting that increased exposure to immigrant businesses may negatively affect property prices, the estimates are not significant.

7.3 Discussion of the Diffusion of Information about Businesses

My theory of the impact of immigrant businesses on native attitudes is built on the premise that natives become aware of these businesses. Qualitative work from sociologists suggests that such information is diffused between individuals: Faury (2024) describes native backlash

spreading easily when immigrant entrepreneurs create businesses in an area. A native voter can learn about the existence of a local immigrant business by directly seeing it or by hearing about it from members of their network. They can learn more about the characteristics and impact of businesses by purchasing goods and services directly from them, by word of mouth, or from elite cues. In this section, I examine three channels that can facilitate or complicate the diffusion of information about these businesses among natives.

The first diffusion channel is the first hand experience of natives as customers of immigrant businesses. While immigrants may in some cases create businesses specifically targeted at their own community (Fairlie and Lofstrom, 2015), they often offer products and services aimed for native customers as well. This is not only true for businesses without cultural valence, such as hardware shops, but also for businesses serving as symbols of group identity, such as restaurants. To examine the extent to which natives are active customers at immigrant-owned restaurants, I collected data from the Google Maps API on the universe of restaurants located in France. I then randomly sampled 4,800 of these restaurants and collected up to 300 reviews left by customers on the Google Maps page of the businesses. I classified the restaurants by cuisine, to infer which country of origin they are symbols of, and predicted the origin of the reviewers using Namsor, the algorithm introduced in Section 4.1. I present the descriptive analysis in more detail in Appendix X. Note that native customers at minority restaurants are a nonrandom from native residents across the board. This is even more true for those native customers who select into leaving a review on Google Maps. The following results are therefore only suggestive and exploratory.

Figure 9 shows that native reviewers constitute a majority of reviewers, and therefore likely customers, across restaurant types. Individuals classified as natives represent 93% of the reviewers for culturally French restaurants, 81% for East Asian restaurants, and 65% for restaurants from Muslim-majority countries. By contrast, individuals with East Asian names constitute only 12% of reviewers in East Asian restaurants and individuals with a name predicted to have a Muslim-majority country origin constitute 32% of the reviewers in the associated restaurants. While restaurants associated to immigrant groups do disproportionately welcome members of their own groups, native customers do remain a majority across restau-

rant types.

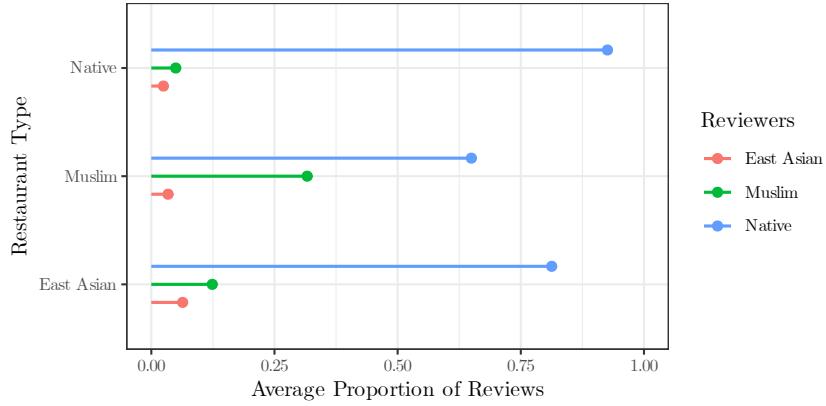


Figure 9: Proportion of Different Groups among Google Maps Reviewers

Figure 10 shows that natives do not tend to rate restaurants from other groups worse. This supports the idea that natives do not dislike the products offered by immigrant businesses³⁰. Instead, their symbolic role may be sufficient to trigger native backlash, consistent with Hypothesis 1. Taken together, this descriptive analysis of Google Maps data confirms that natives directly engage with immigrant businesses. Diffusion to their broader network can follow from word-of-mouth. These observations also suggest that the business-customer relationship is unlikely to be the source of backlash.

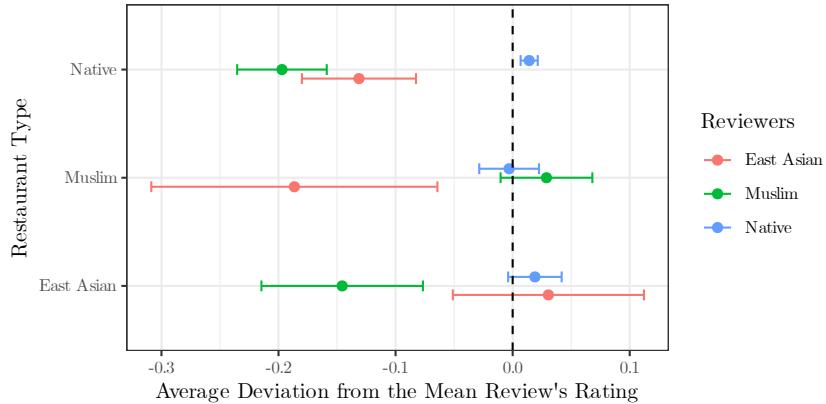


Figure 10: Average Google Maps Rating of Restaurants by Different Groups

Despite these results, some natives may belong to networks in which no individual will opt to purchase goods and services from immigrant businesses. Another diffusion channel is simple visual exposure to store fronts. The frequency and intensity of this type of exposure

³⁰At least natives who select into being customers at these businesses.

depends on how immigrant businesses are arranged in space. In Appendix Y, I re-estimate the OLS model but interact the main exposure with two measures of spatial segregation: the spatial dissimilarity index and the spatial Relative diversity index (Reardon and O’Sullivan, 2004; Hong et al., 2014). I find a significant and positive coefficient on the interaction term: in more segregated areas, where immigrant businesses are more clustered relative to other businesses, increased exposure is associated to a stronger increase in support for the far right. This suggests a more complicated picture of how space influences the diffusion of information and attitudes. If segregation acted primarily as a diffusion mechanism, we should expect a negative coefficient: more segregation would mean fewer natives exposed, and therefore a weaker effect. Instead, the positive coefficient suggests that segregation may moderate attitudes in another way. Natives may perceive immigrants to be less well integrated into the host country if their institutions are perceived to be enclaves (Aldrich and Waldinger, 1990; Fairlie and Lofstrom, 2015). In interviews with RN voters, Faury (2024) reports that some voters complain about a part of their town now looking like a different country. Concentration of businesses may increase such perceptions and facilitate backlash.

Finally, results from the survey experiment suggest that elite discourse about immigrant businesses affects beliefs about the impact of economic businesses and attitudes toward immigration. The central role of elite cues is not new in the political behavior literature (Boudreau, 2009; Lenz, 2012; Nicholson, 2012; Bullock, 2011) and this result is consistent with anecdotal evidence of elites directing voters’ attention toward immigrant businesses (e.g., Le Monde, 2015; CNN, 2018; actu.fr, 2024a,b). Politician discourse is one instance here that likely reflects a broader category of elite cues. This echoes work by Hopkins (2010) who established that local backlash against immigrants is activated when the salience of immigration is high due to national news or discourse. In France, even though mayoral approval is not formally required for the opening of a business (with the exception of businesses requiring construction), municipal governments may attempt to use policies in their control to directly show their stance on immigrant businesses. In Appendix W, I show that IV estimates suggest that an increase in the number of immigrant-owned businesses causes an increase in the local business tax, which is set by locally elected governments. The interpretation of such a result is

ambiguous, since the business tax also affects native businesses. However, it is suggestive of some systematic reaction by elites, who may perhaps use these taxes to signal their stance on immigrant businesses, or use immigrant businesses as a justification for tax increases.

This analysis of potential diffusion mechanisms indicates that daily visual exposure to immigrant businesses may not be the primary driver of native backlash. Instead, the presence of businesses somewhere in the community, along with word-of-mouth and elite cues, may suffice to trigger backlash against immigration and strengthen support for the far right.

8 Conclusion

Millions of immigrant-owned businesses exist across Western democracies. In this paper, I show that when natives are exposed to immigrant businesses, they become more likely to support far-right populist parties opposed to immigration. My findings in the context of France suggest that this effect is driven by businesses' role as symbols: the effect is pronounced only for *visible* businesses owned by immigrants from Muslim-majority countries, the minority with which cultural conflict is most salient. In contrast, I find no evidence that immigrant-owned businesses negatively affect local economies: if anything, my results suggest that they increase the diversity of goods and services available in local markets, and marginally reduce native unemployment.

These results have several implications. First, they complement existing research on how local demographic shocks shape immigration attitudes and support for the far right (Hopkins, 2010; Hangartner et al., 2019; Cools et al., 2021; Alrababah et al., 2024): even when the proportion of immigrants in the population remains fixed, changes in the visibility of immigration, through symbols to which natives are exposed on a regular basis, may affect voters. Second, the backlash against businesses suggests a potential vicious cycle of anti-immigration attitudes: immigrants may create businesses because they are unable to find salaried employment due to discrimination. However, if these businesses further fuel negative attitudes toward immigrants, they may in turn worsen labor market prospects, perpetuating the cycle. This cycle makes anti-immigrant-business rhetoric a prime choice for far-right populist polit-

ical entrepreneurs. Third, these conclusions contribute to our understanding of the tensions of globalization (Algan et al., 2017; Colantone and Stanig, 2018b; Autor et al., 2020), as pro-business policies and open borders may both successfully foster local economic growth and trigger native backlash because of cultural conflict. In this sense, this paper is aligned with findings from the literature on immigration attitudes suggesting that cultural factors play a predominant role (Hainmueller and Hopkins, 2014).

This paper has several limitations: first, the identification strategy does not make it possible to test heterogeneity causally. Experimental work may be a promising way to differentiate between exposure to different types of businesses and different immigrant origins. Second, the IV approach partials out the effect of the treatment on native businesses, but local market equilibrium effects may still play a mediating role—particularly if perceived displacement contributes to backlash, as suggested by my experimental results. This may be approached through structural modeling of local business entry and exit. Finally, replicating this study in other countries will help assess the external validity of the findings.

This paper suggests at least three promising areas for future research. First, researchers should seek to confirm the reasons why businesses owned by immigrants from Muslim-majority countries trigger backlash among voters. One possible reason is simply that natives update their priors about the presence of an out-group in their community toward whom they hold preexisting prejudice. Another possibility is that these businesses more visibly signal cultural differences, and natives attach high value to a physical environment aligned with their perception of national identity. One way to disentangle these factors will be to extend this research across contexts where the types of businesses and salient immigration debates differ.

Second, researchers should investigate the effects of immigrant entrepreneurship on the broader political equilibrium, including at the elite level. My results suggest that elite rhetoric influences perceptions of businesses, but businesses may in turn contribute to the success of political entrepreneurs who make them focal points of their campaigns, as has been the case not only in France (Le Monde, 2015) but also in Italy where a movement of mayors set off a “Yes to Polenta, No to Couscous” movement in 2009 (Hill, 2025). They

Finally, researchers should examine the political determinants of immigrant entrepreneurship. Scholars have already begun to show that immigrants' residential decisions are affected by political attitudes (Lueders, 2023). Future research might reveal that negative attitudes toward immigrants prevent entrepreneurship and hence hinder local economic activity, further fueling sentiments of economic decline and in turn nativism. Alternatively, it might reveal that immigrants open businesses in areas already affected by economic decline due to factors such as lower rents and operating costs. This may suggest that immigrant businesses are one of the main sources of contact between natives and immigrants in rural and otherwise declining areas.

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Appendix

A Election Data

Cantons are approximately equivalent to departments (an intermediate administrative unit between regions and municipalities). Cantonal elections used to be held for a rotating subset of territories every three years. Starting in 2015, they were replaced by departmental elections, conducted every six years country-wide. Senate elections are not included because they are indirect: the voters are other elected officials at the municipal, departmental, regional, and national levels.

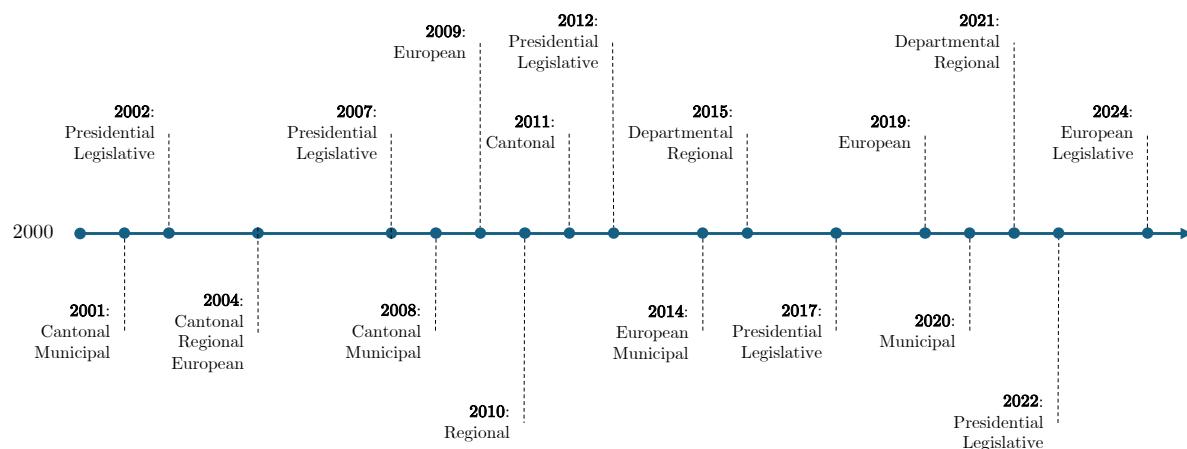


Figure 11: Electoral Results Available since 2000

B Far Right Vote

C French Business: Creation and Data Structure

Figure 14 shows the steps generally required to create a business in France. Note that each business structure type (SARL, SA, “microentrepreneurship”, etc.) requires specific steps. For example, micro-entrepreneurs, a category often selected by small businesses, including in the gig economy, do not need to deposit capital or formally declare the legal existence of the company. However, every new company must be registered with the *Registre National des Entreprises* (RNE), which will assign them a SIREN id and a SIRET id.

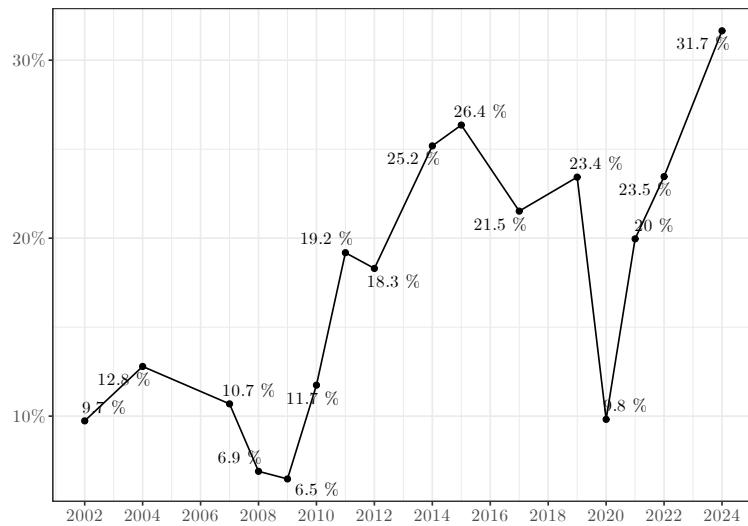


Figure 12: Electoral Support for the Far Right, 2002-2024

National average of the RN vote share over time. Municipal elections only include municipalities with over 3,500 residents. The 2020 drop can be explained by the subsample of municipalities whose results are available for municipal elections: only results for municipalities with over 3,500 residents are reported, and these more urban municipalities tend to show lower rates of support for the far right.

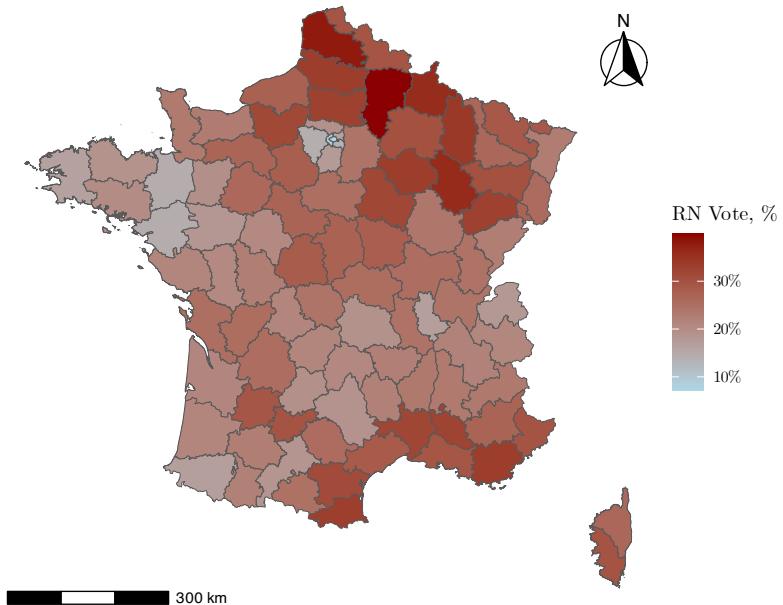


Figure 13: RN Vote by Department, 2019

My data relies on the information stored in the Registre National des Entreprises and is of the form described in Figure 15. I predict the probable origin of business owners at the company level, while I geo-code and time-code each business.

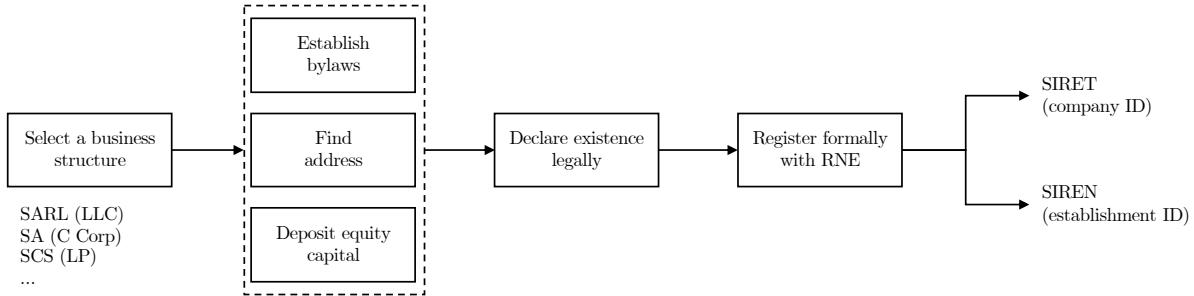


Figure 14: Business Creation Process in France

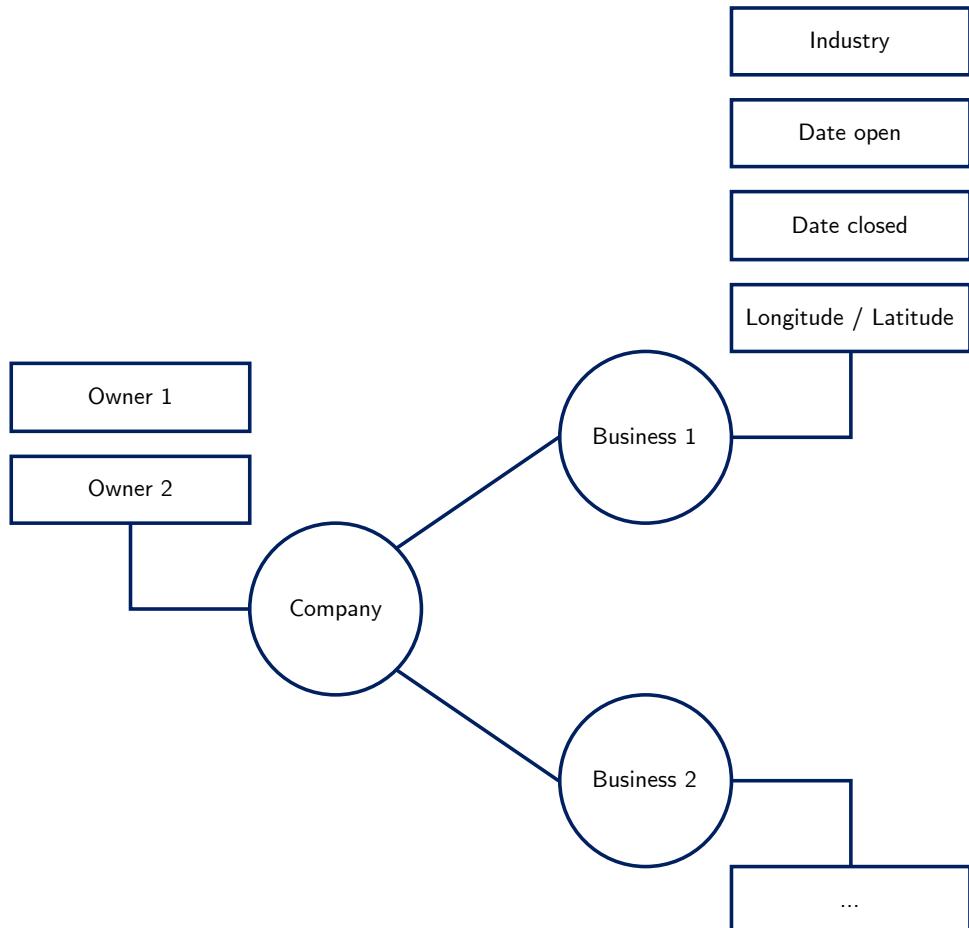


Figure 15: Nested Structure of the Individual Business Data

D Context on the 1978 Law

Law 78-17 from January 6th, 1978, known as the French Data Protection Act (*Loi Protection et Libertés*) includes, in Article 8: “*It is prohibited to collect or process personal data which reveal, directly or indirectly, racial or ethnic origins, political, philosophical or religious opinions or trade union membership of individuals, or which relate to their health or sexual life.*”

The law originally reflected public concern born in the wake of the Second World War,

during which citizens of Jewish origins were forced to wear a yellow star to be identified as such under Nazi occupation. The article mentioned above was challenged in 2007 to facilitate studies of diversity, discrimination, and integration. However, the amendment was shut down by the Constitutional Council—the equivalent of the US Supreme Court—under the premise that Article 1 of the 1958 French Constitution states that the country “*is an indivisible, secular, democratic, and social Republic [that] ensures equality before the law for all citizens without distinction of origin, race, or religion.*”

The 1978 law has been regularly challenged since then, most notably in 2009 (see e.g., Chrisafis (2009)). Proponents of a change typically argue that allowing “ethnic data” collection will facilitate a better understanding of systemic inequalities based on race and ethnicity, and in turn inform policy design. Opponents typically remind the initial rationale behind the law and argue that policy is sufficiently well informed by economic and geographic data. In the census, country of origin is recorded for some individuals born abroad, but no existing, systematic administrative data is available to research the impact of immigration if the definition of immigration extends to second-generation individuals.

E Namsor Classification

Table 5 illustrates the output of the Namsor algorithm for ten randomly chosen names from the RNE data. Last names are removed to preserve anonymity. I use the *Origin* variable to classify the origin of each business.

Table 5: Example of Output from the Namsor Algorithm

First names	Last name	Origin	Score	Alternative origins
Yannick	R.	French	0.94	Flemish,Jewish,Walloon,...
Florent	P.	French	0.95	Flemish,Swiss,Italian,...
Edwige; M.; D.	P.	French	0.96	Flemish,Jewish,German,...
Georges	J. D. F.	Portuguese	0.81	Jewish,French,German,...
Niyazi	K.	Turkish	0.96	Armenian,Azerbaijani,Georgian,...
Clement; M.; Y.	C.	French	0.96	Flemish,Jewish,British,...
Patrick	C.	French	0.96	Jewish,Flemish,British,...
Cyril	R.	Filipino	0.43	French,Hispanic,Jewish,...
Olivier; S. ;J.	D.	French	0.91	Flemish,Jewish,British,...
Thierry	G.	Hispanic	0.75	Italian,French,Jewish,...

While the performance of Namsor has been evaluated in broader context (Bursztyn et al., 2022; Sebo, 2022), validating the classification in the context of France presents a challenge because data on the true origin of individuals is not available. To circumvent this, I use comprehensive Insee data on deceased individuals. This dataset includes the place of birth of every deceased person–municipality for French-born individuals and country for others. Country of birth is an imprecise variable: individuals can be born in countries they are only temporarily residing in, or where they have immigrated before migrating again. I therefore use it as the target prediction knowing it only describes the country of origin of individuals with error. I only include individuals in or after 1962, the year in which Algeria gained its independence from France. Before this a large number of native French people lived and were born in Algeria, increasing the imprecision issue³¹.

I first find that the country of origin predicted by Namsor is the same as the country of birth in the Insee data for only 44% of individuals. This number increases to 55% if I exclude individuals born in Europe or North America. This figure is low but not surprising: given within-region migration and the propagation of foreign names within broader regions, the exact country of birth is difficult to predict. The classification of interest here, predicting origins from outside of Europe and North American and from Muslim-majority countries, does not rely on the exact country but on the broader region.

I find that Namsor predicts the region corresponding to the country of birth for 82% of the individuals, and that it correctly predicts whether individuals come from outside of Europe and North America in 87% of cases. Finally, I find that Namsor correctly predicts whether individuals are from Muslim-majority countries in 94% of the cases.

Keep in mind that this is likely to be a lower bound since for some individuals who are second-generation immigrants in their country of birth, Namsor may correctly predict the origin country, but this will still show up as incorrect in this validation exercise.

³¹In 1962, 800,000 native French people migrated back from Algeria to France.

F Description of Immigrant Business

Figure 16 shows the 15 storefront industries for which the proportion of businesses owned by immigrants was larger than 25% in January of 2024. This list includes many industries that native residents are exposed to on a daily basis and shows a diversity that contradicts the far-right rhetoric depicting immigrants as concentrated in only a few industries like fast-food shops.

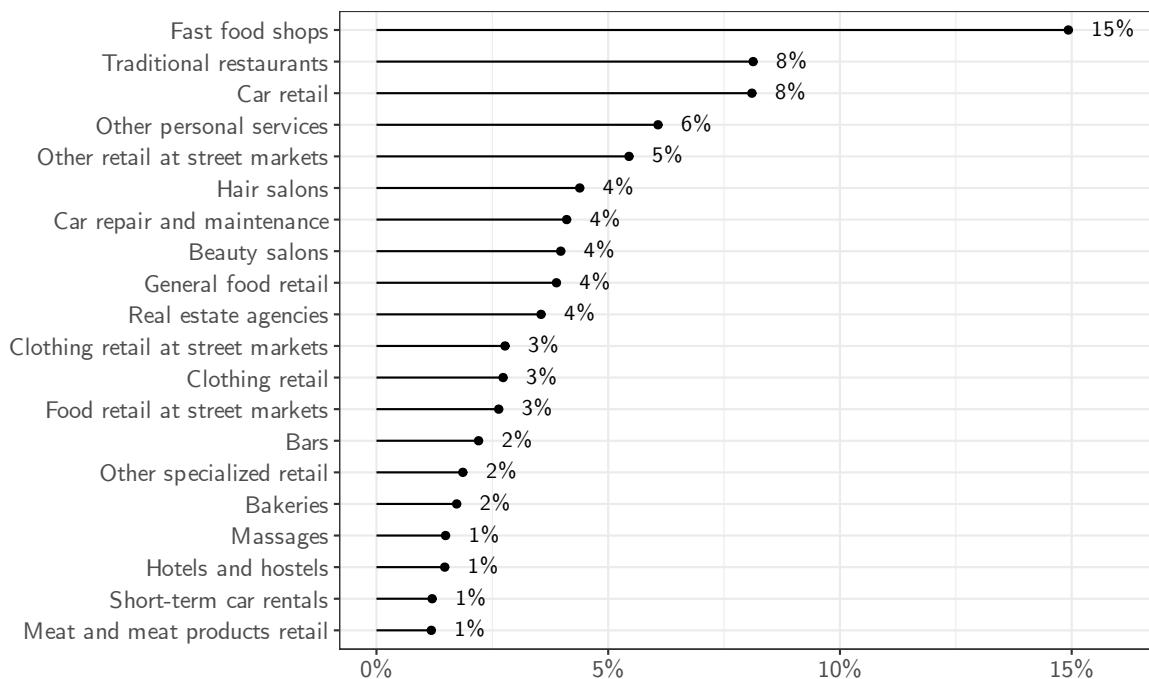


Figure 16: Industries Most Represented among Immigrant Businesses, January 2024

Each point represents the proportion of all businesses classified as immigrant-owned in a given industry in January 2024.

Figure 17 finally displays the twenty most represented countries of origin of immigrant business owners, in January of 2024, based on the classification algorithm. Immigrants from Northern and Sub-Saharan Africa are strongly represented, reflecting French colonial history. Chinese and Turkish immigrants are representative of more recent waves of immigration.

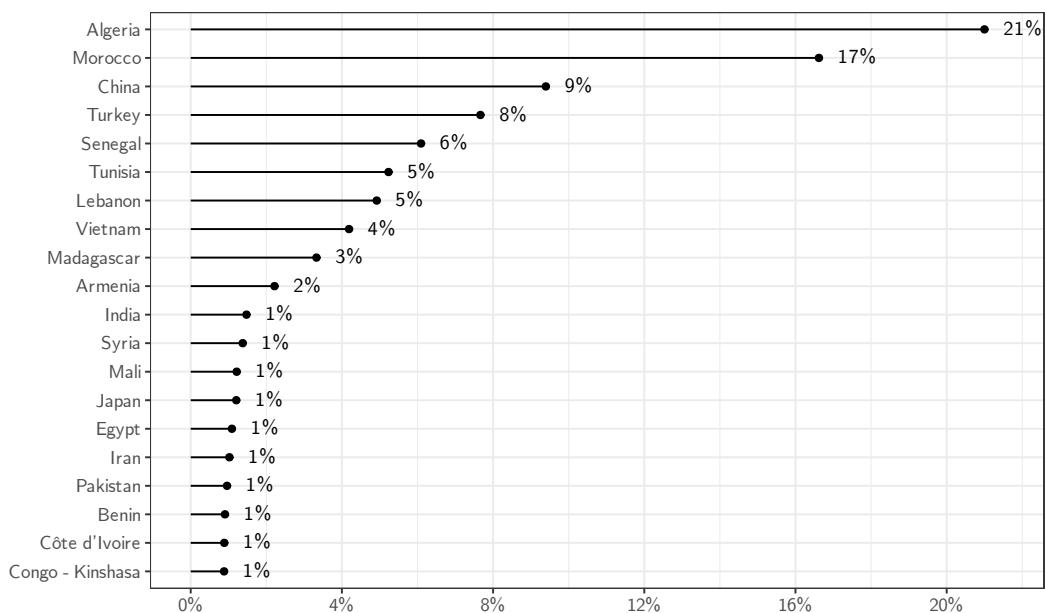


Figure 17: 20 Most Frequent Origins of Immigrant Business Owners, January 2024

Each point represents the proportion of businesses owned by immigrants of a given origin, based on the algorithm's output, out of all immigrant businesses in January 2024.

G Immigrant Businesses and Immigrant Residents

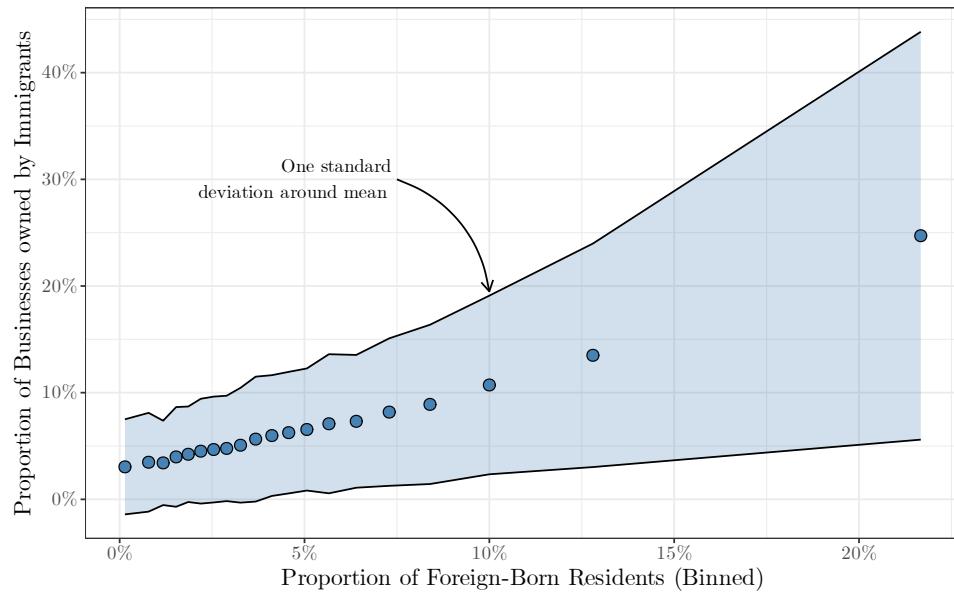


Figure 18: Immigrants among residents and business owners, January 2022

Each dot represents a municipality in January 2022. The scales are logged for clarity because the mass of the distribution is concentrated at lower values for both axes.

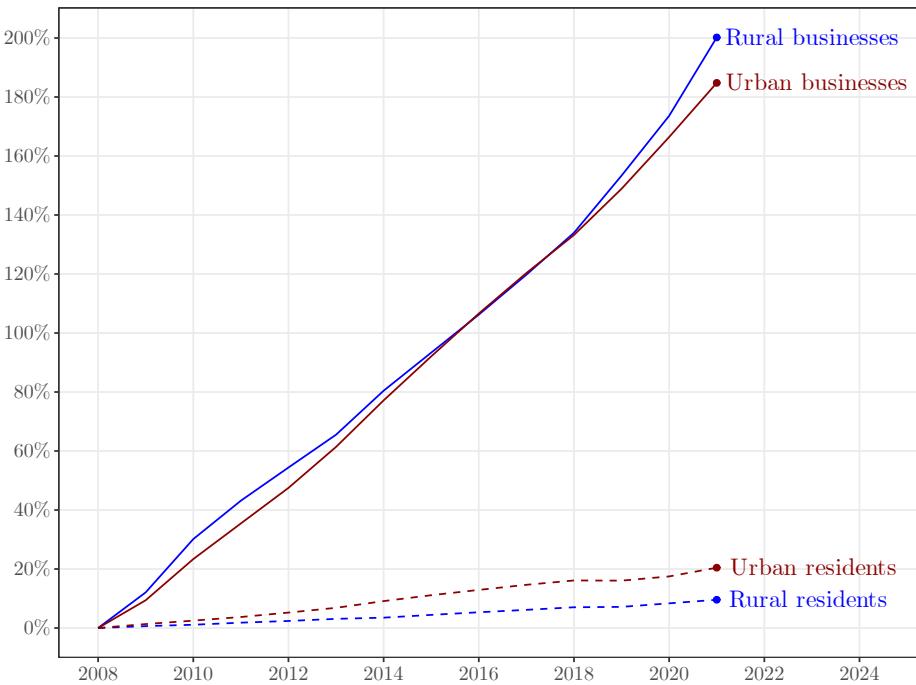


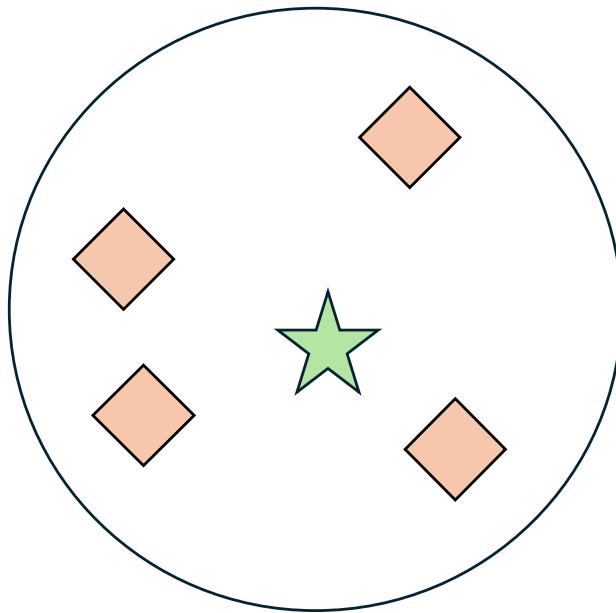
Figure 19: Growth in the Proportion of Immigrants

H Illustration of Competition and Market Richness

The four figures below illustrate the operationalization of the concepts of average industry competition and market richness, introduced in Section 7.2. The formulas are displayed here as a reminder.

$$AIC_m = 1 - \left\{ \sum_{j=1}^{J_m} W_j \times \frac{1}{n_j^2} \right\} \quad MR_m = 1 - \sum_{j=1}^{J_m} \left(\frac{\sum_{i \in j} w_{m,i}}{\sum_i^K w_{m,i}} \right)^2$$

In the figures, all businesses are assumed equidistant from the center of the municipality of interest. The sum of weights are therefore simple counts. The first figure (*Status Quo*) represents the economy in and around a municipality. Each item is a business, whose shape and color represent its industry. The orange diamond industry is more competitive (four businesses) than the green star industry.



Status quo: competition and diversity

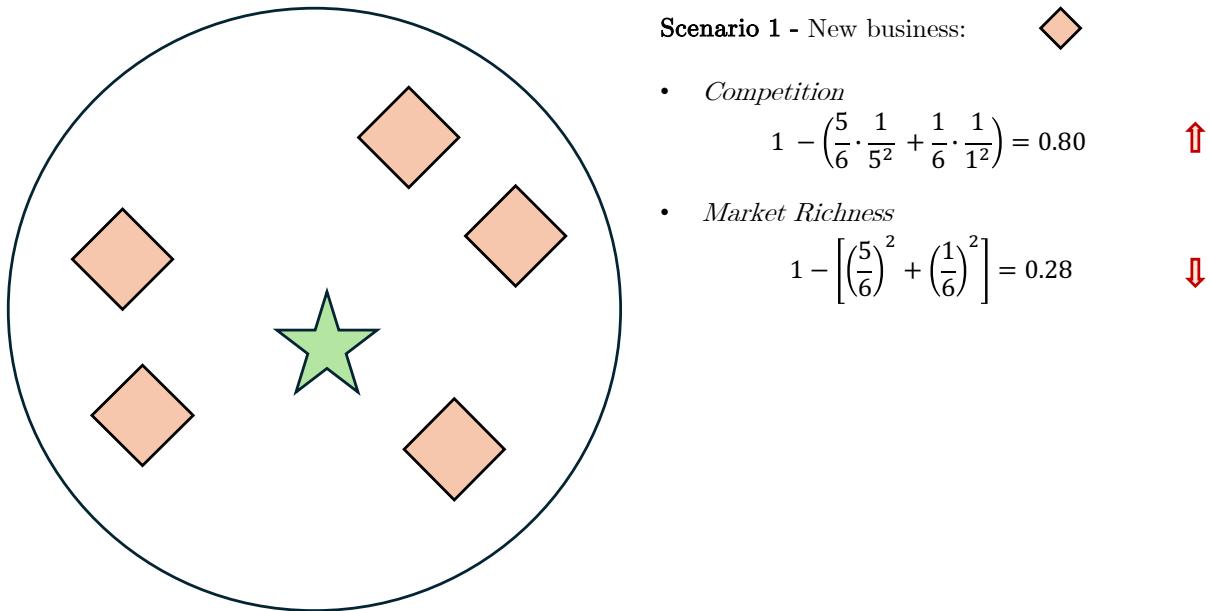
- *Competition*

$$1 - \left(\frac{4}{5} \cdot \frac{1}{4^2} + \frac{1}{5} \cdot \frac{1}{1^2} \right) = 0.75$$
- *Market Richness*

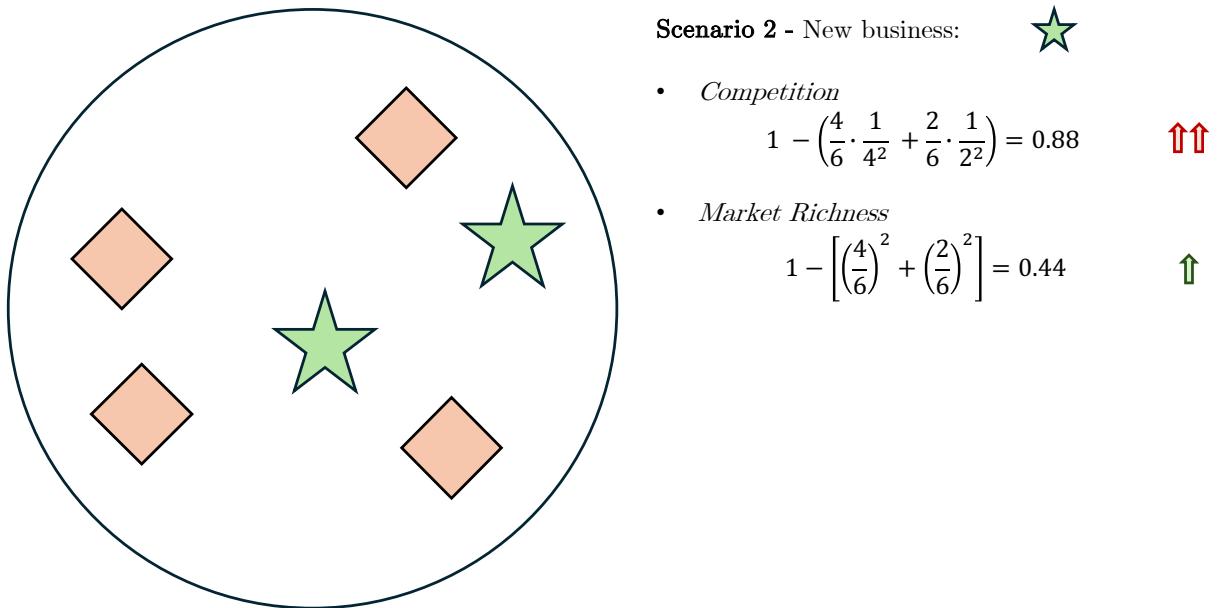
$$1 - \left[\left(\frac{4}{5} \right)^2 + \left(\frac{1}{5} \right)^2 \right] = 0.32$$

The next three figures show how new businesses from different industries affect the measures of competition and market richness. In Scenario 1, a new business is created in the already very competitive orange diamond industry. The marginal addition increases a little the competition score and decreases a little the market richness score, as the industry concentration increases.

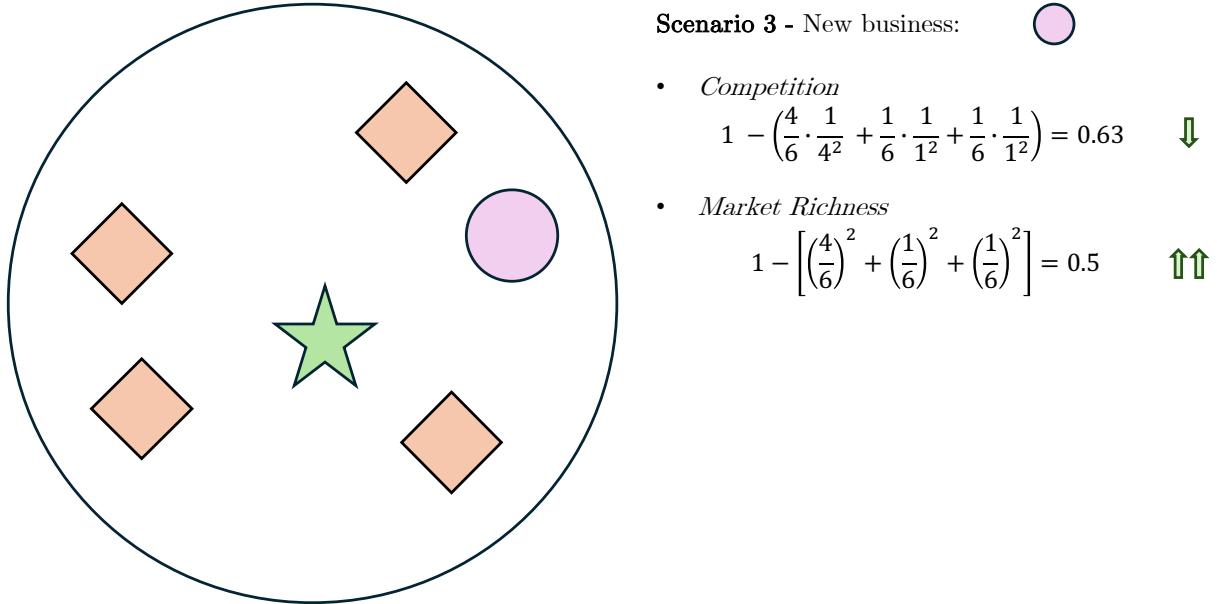
In Scenario 2, a new business is created in the low-competition green star industry. The marginal addition increases the competition score significantly, as the green star industry was



previously a monopoly, and increases market richness a little, as the industry concentration decreases.



Finally, in Scenario 3, a new business is created in the purple circle industry, previously unrepresented in the community. The marginal addition increases the competition score significantly: since the new industry is a monopoly, the denominator in the weighted average increases but not the numerator. The market richness score increases, as industry concentration decreases.



I Historical Industrial Persistence

I empirically test whether industrial agglomeration effects predicted by the literature (e.g., Ellison and Glaeser, 1997; Glaeser et al., 2010) lead to persistence in industrial composition at the municipality level. To do so, I test whether the municipal industry composition in 2000 predicts the composition of the new business created between 2000 and 2010. I do so by calculating the cosine similarity between the distribution of existing 2000 industries and the distribution of 2000-2010 new businesses' industries.

To test the persistence hypothesis, I test whether the average cosine similarity (across municipalities) between existing distribution and new businesses is closer than if new businesses were instead, under a null hypothesis, drawn from the industries of similar municipalities at random. “Similar municipalities” are those in the same category of rurality or urbanity (out of seven categories) and within these categories, in the same decile for number of businesses. Results are displayed in Figure 20.

I find that strong evidence that the observed similarity between new businesses and existing businesses in a specific municipality is stronger than it would be if businesses were just created in similar ways for similar municipalities. I can reject the null hypothesis with a p-value of 0.036.

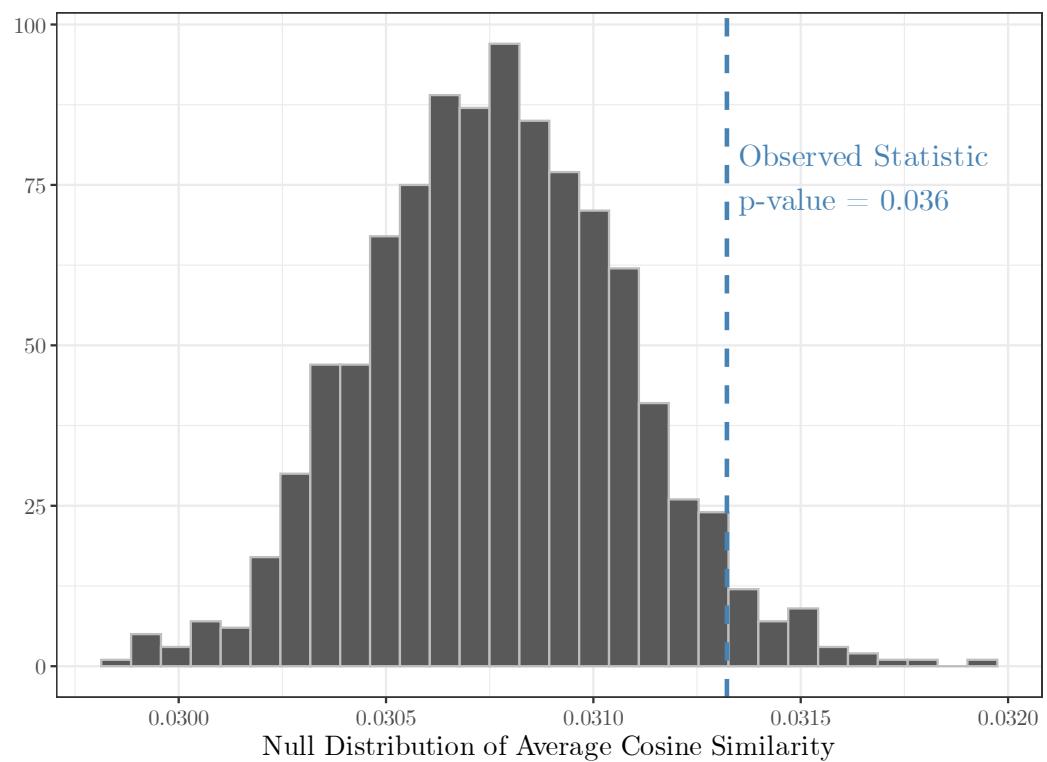


Figure 20: Test of Historical Industrial Persistence

J Description of the Instrument

The instrument $Z_m^{industry}$ is defined as follows, where m indexes the municipalities:

$$Z_m^{industry} = \sum_{j=1}^J S_{m,j} \tilde{G}_j^{immig} \quad (6)$$

$S_{m,j}$ is the share of municipality m 's businesses that operate in industry j in 2005, and \tilde{G}_j^{immig} is the *residual* growth of immigrant businesses in industry j nationally between 2009 and 2019. As described in Appendix K, \tilde{G}_j^{immig} is a plug-in value predicted by GDP growth in the countries of origin of the immigrants involved in each industry in 2005. Figure 21 displays the 20 industries with the highest value of \tilde{G}_j^{immig} .



Figure 21: 20 industries with highest values of \tilde{G}_j^{immig}

Figure 22 shows a map of France where the municipalities are shaded depending on the proportion of all the businesses they are exposed to who are among the businesses listed in Figure 21. The visible variation does not follow any clear pattern in terms of urbanicity, proximity to the coast, or proximity to the borders. Naturally, the true value of the instrument presents even more variation because of 1) the variation in the some other 60 industries and 2)

the variation in proportions within this set of 20 industries with the highest value of \tilde{G}_j^{immig} .

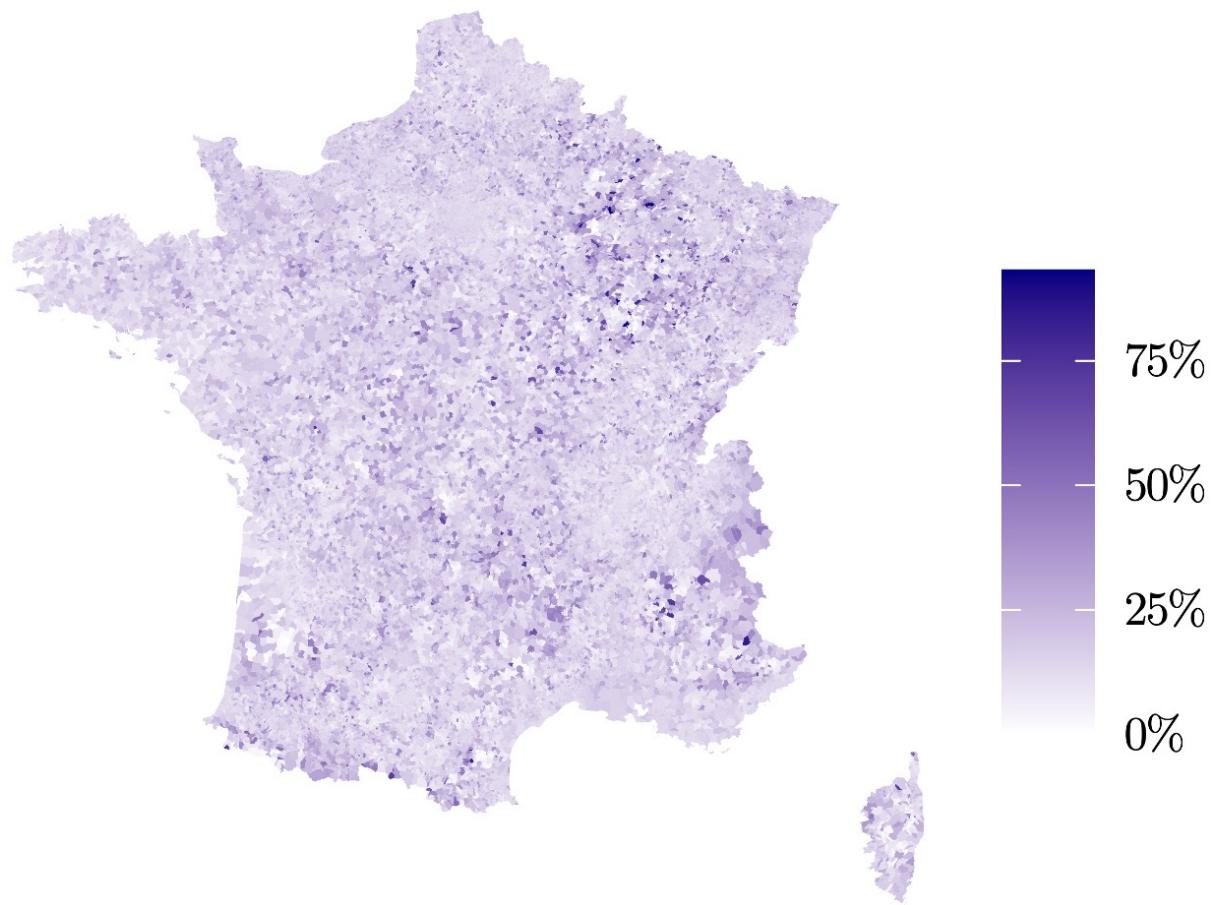


Figure 22: Municipalities where these industries are most represented

K Shift-Share Instrument and “Stage Zero”

K.1 Stage Zero: GDP Growth and Immigrant Business Creation

To ensure that the national shocks \tilde{G}_j^{immig} are exogenous, I introduce a “stage zero” regression predicting \tilde{G}_j^{immig} based on GDP growth in the countries of origin of each industry’s immigrants. The logic is the following: without this step, it could be that the degree to which immigrants create new businesses in different industries is endogenous to broader features of the French economy. Instead, just like in 2SLS, the second stage is a regression of the outcome on the values of the treatment predicted by the instrument in the first stage, I predict the values of the instrument using GDP growth in the countries of origin in a zero-th stage. This has been routinely done in economic applications (see for example Tabellini, 2020), but I also show in Section K.3 that the results are robust to using the instrument as *observed*, instead of using it as *predicted* in this stage 0.

For each industry j , I calculate the *growth mix*:

$$GM_j = \sum_{o \in j} \omega_o \text{growth}_o$$

Where ω_o is the proportion of businesses in industry j owned by immigrants from origin o in 2005, and growth_o is the GDP growth in country o between 2009 and 2019. Stage zero is therefore:

$$\tilde{G}_j^{immig} = \zeta GM_j + \xi_j \quad (7)$$

Where ξ_j is an error term. I report the estimate of ζ in Table 6. The coefficient is negative: fewer immigrant businesses are created between 2009 and 2019 in industries with many immigrants from high-GDP-growth countries. While the sign is not *ex ante* evident, one possible explanation is the following: immigrants face the decision to invest in France, by creating a business, or in their home country, either by creating a company there or by allocating funds to ventures of their network members. When GDP growth is high in their home country, the return on investment in the home country is higher. This makes the opportunity cost of

creating a business in France larger, and therefore decreases its probability.

Table 6: Stage Zero

Residual Growth in Im. Businesses (2009-2019)	
Constant	1.31** (0.59)
Weighted Average of GDP Growth	-2.69** (1.16)
R ²	0.06
Adj. R ²	0.05
Num. obs.	87

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

This table shows the estimate show the “Stage Zero” model introduced in Equation 7. The coefficient of interest shows that the number of businesses created by immigrants in a given industry in France is negatively associated by the average GDP growth in the countries of origin of the immigrant business-owners of that industry at baseline.

In the main specification of the paper, I plug the predicted value of \tilde{G}_j^{immig} from Equation 7 into Equation 1 to calculate the instrument. Using this plug-in shock based on the GDP growth of other countries reinforces the credibility of the exogeneity assumption. However, I show in Section K.3 below that results are robust to using the realized value of industry-specific growth instead of this predicted value. Before doing so, I show in Section K.2 that results hold when using propagating the uncertainty coming from this Stage Zero regression using a nonparametric bootstrap approach.

K.2 Propagation of Uncertainty from Stage Zero

In the main specification, when plugging in the value \tilde{G}_j^{immig} predicted by the Stage Zero regression, I do not propagate uncertainty in the estimate to the later stages of the instrumental variable approach. To correct this, I show here the results of the 2SLS estimation with inference conducted through a nonparametric bootstrap approach (Efron, 1979).

Starting with Stage Zero (prediction of immigrant-specific industry growth), I draw 1,000 bootstrap samples of French municipalities with replacement, allowing municipalities to appear multiple times in a sample. For each bootstrap sample, I re-estimate Stage Zero, followed by the first and second stages. The empirical distribution of these bootstrapped estimates ap-

proximates the sampling distribution of each estimator. Table 7 reports the resulting bootstrap estimates for the first and second stages. The estimates remain precisely estimated and of the same magnitude.

Table 7: Bootstrap Estimates

	First Stage Δ Exposure (log)	Second Stage Δ RN Vote Share
Instrument	0.53*** (0.15)	
Δ Exposure (log)		0.08*** (0.02)
Dist. to highway (log)	-0.02*** (0.00)	-0.00 (0.00)
Native businesses (log, 2009)	-0.09*** (0.01)	-0.00 (0.00)
Foreign pop. in mun. (log, 2009)	0.01*** (0.00)	-0.00*** (0.00)
Foreign pop. around (log, 2009)	0.00 (0.00)	-0.00 (0.00)
Population (log, 2009)	0.05*** (0.01)	0.00 (0.00)
Prop. unemployed (2009)	-1.24*** (0.31)	0.29*** (0.07)
Prop. college graduate (2009)	-0.14** (0.07)	-0.34*** (0.01)
Prop. HS graduate (2009)	0.19 (0.21)	-0.18*** (0.03)
Urban/Rural controls	Yes	Yes
Department FE	Yes	Yes
Num. obs.	33756	33756

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Bootstrap Standard Errors.

The estimates show first- and second-stage results estimated with nonparametric bootstrap standard errors. The coefficients are the averages of the bootstrap distribution of the estimates, where municipalities are sampled with replacement 1,000 times. The standard errors are the standard deviations of the bootstrap distributions.

K.3 Replication without Stage Zero

To ensure that findings are not a function of the Stage Zero specification, I test whether results are robust to using the realized values of G_j^{immig} instead of their predicted value \tilde{G}_j^{immig} . In

Figure 23, I show the 20 industries with the highest value of G_j^{immig} , the *observed* residual growth in immigrant-owned businesses, instead of the value predicted by GDP growth in the countries of origin. There mechanically are fewer high values than in Figure 23 since the linear model in the Stage Zero (see Equation 7) will reduce variance.

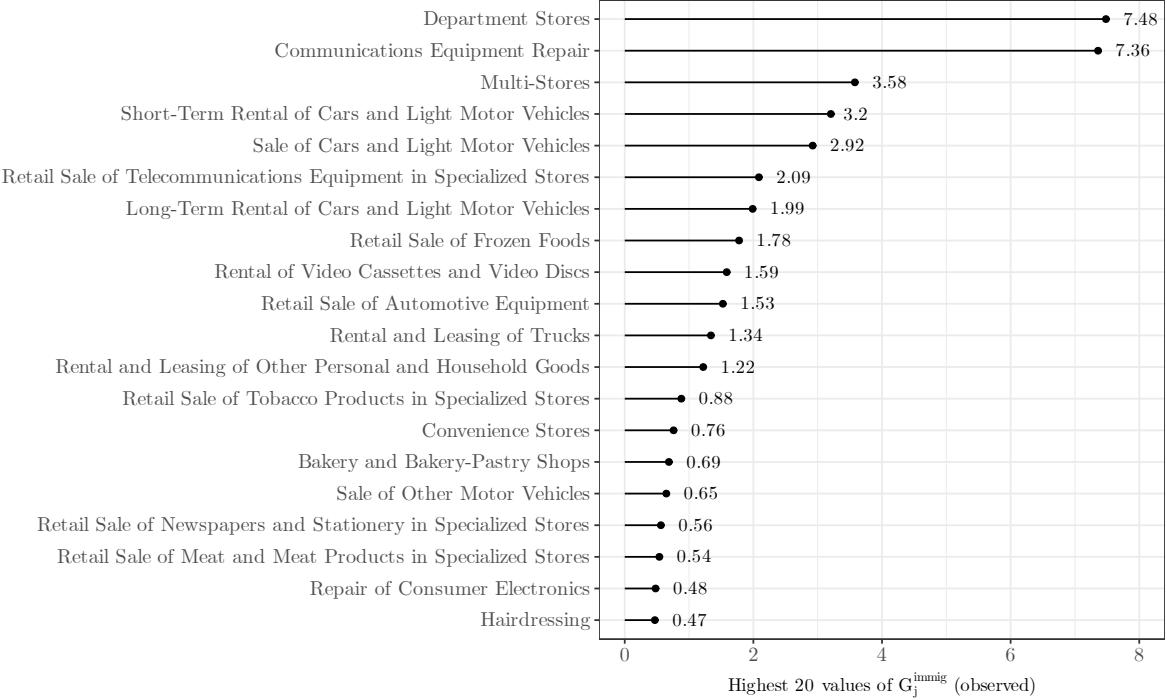


Figure 23: 20 industries with highest values of \tilde{G}_j^{immig} (Observed)

Estimates from the first stage using the observed value instead of the predicted are displayed in Table 8.

Table 8 shows a weaker but still precisely first-stage estimate. Importantly, the placebo checks for other paths through which the instrument could cause changes in the outcome reveal a positive correlation between the instrument and the change in the logged number of native businesses. While the effect is small, this hints at a potential violation of the exclusion restriction. In Borusyak et al.’s (2022) terms, this may due to a violation of quasi-random shock assignment: the national shocks may be correlated with other variables that differentially impact municipalities with different weights.

To account for this in the second stage, whose results are displayed in Table 9, I estimate a fourth model that controls for the change in the logged number of native businesses. This does not affect the main results. In general, the results are similar in magnitude and significance

Table 8: First stage and Placebo Checks with Observed Residual

	First Stage		Placebo
	Δ Exposure (log)	Δ Nat. Bus. (log)	Δ Foreign Pop. (log)
Instrument	0.19*** (0.04)	-0.03*** (0.01)	0.01 (0.06)
Dist. to highway (log)	-0.02*** (0.01)	-0.01*** (0.00)	-0.00 (0.01)
Native businesses (log, 2009)	-0.10*** (0.02)	-0.15*** (0.01)	-0.01 (0.03)
Foreign pop. in mun. (log, 2009)	0.01** (0.00)	-0.00 (0.00)	-0.68*** (0.01)
Foreign pop. around (log, 2009)	-0.00 (0.01)	0.01*** (0.00)	0.03* (0.02)
Population (log, 2009)	0.04** (0.02)	0.09*** (0.01)	0.91*** (0.04)
Population around (log, 2009)	0.02 (0.02)	0.00 (0.01)	0.07 (0.05)
Prop. unemployed (2009)	-1.11*** (0.36)	-0.23*** (0.08)	5.07*** (0.67)
Prop. college graduate (2009)	-0.10 (0.11)	0.42*** (0.07)	-0.51** (0.21)
Prop. HS graduate (2009)	0.31 (0.28)	0.80*** (0.09)	-0.23 (0.53)
Urban/Rural controls	Yes	Yes	Yes
Department FE	Yes	Yes	Yes
Num. obs.	33721	33721	33721
R ²	0.06	0.47	0.33
Adj. R ²	0.06	0.47	0.32

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

to the paper's main specification including a Stage Zero.

Table 9: Reduced Form and Second Stage with Observed Residual

	Δ RN Vote Share			
	Reduced Form	OLS	2SLS	2SLS
Instrument	0.01*** (0.00)			
Δ Exposure (log)		0.00*** (0.00)	0.06*** (0.02)	0.06*** (0.02)
Foreign pop. (log, 2009)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)
Foreign pop. around (log, 2009)	-0.00** (0.00)	-0.00** (0.00)	-0.00 (0.00)	-0.00 (0.00)
Population (log, 2009)	0.00** (0.00)	0.00** (0.00)	-0.00 (0.00)	0.00 (0.00)
Population around (log, 2009)	0.00** (0.00)	0.00*** (0.00)	0.00 (0.00)	0.00 (0.00)
Prop. unemployed (2009)	0.19** (0.09)	0.18* (0.09)	0.26*** (0.09)	0.26*** (0.09)
Prop. college graduate (2009)	-0.35*** (0.05)	-0.35*** (0.05)	-0.34*** (0.05)	-0.34*** (0.06)
Prop. HS graduate (2009)	-0.16*** (0.04)	-0.17*** (0.04)	-0.18*** (0.05)	-0.18*** (0.05)
Native businesses (log, 2009)	-0.01*** (0.00)	-0.01*** (0.00)	-0.01* (0.00)	-0.01* (0.00)
Δ Native bus. (log)				-0.00 (0.00)
Dist. to highway (log)	-0.00** (0.00)	-0.00** (0.00)	-0.00 (0.00)	-0.00 (0.00)
Urban/Rural controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Outcome Mean	0.21	0.21	0.21	0.21
Num. obs.	33720	33720	33720	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

L Jackknife

Table 10 displays the results of the first and second stage estimated for a version of the instrument using jackknife sampling (Wu, 1986) such that for each municipality, the exogenous shocks of the shift-share instrument are calculated by excluding the municipality in question from the sample. This is particularly important to make sure that the value of the instrument in large cities, who are given more weights in the IV estimates, is not driven by observed business-creation in these cities themselves. The results are similar in substance and significance.

Table 10: 2SLS with (with jackknife)

	Δ Exposure (log) First Stage	Δ RN Vote Share Second Stage
Instrument	0.467*** (0.124)	
Δ Exposure (log)		0.053** (0.025)
Foreign pop. (log, 2008)	0.011*** (0.004)	-0.002*** (0.001)
Foreign pop. around (log, 2008)	-0.008 (0.010)	-0.002 (0.001)
Population (log, 2008)	0.041** (0.016)	-0.000 (0.002)
Population around (log, 2008)	0.023 (0.016)	0.003** (0.002)
Prop. unemployed (2008)	-1.336*** (0.354)	0.190 (0.123)
Prop. college graduate (2008)	-0.129 (0.095)	-0.329*** (0.053)
Prop. HS graduate (2008)	0.323 (0.263)	-0.181*** (0.050)
Native businesses (log, 2008)	-0.094*** (0.017)	-0.007** (0.003)
Dist. to highway (log)	-0.008*** (0.002)	0.000 (0.000)
Urban/Rural controls	Yes	Yes
Department FE	Yes	Yes
Num. obs.	34776	34775

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

M Instrumental Variable: Further Tests of Assumptions

M.1 Balance Checks

To further evaluate the plausibility of the identifying assumptions of the instrumental variable approach, I test whether, conditional on the variables included in the first stage, the instrument predicts change in other variables between 2009 and 2019. Table 11 displays the estimates from these models.

Table 11: First stage and Placebo Checks

	Δ College	Δ High School	Δ Unemployment	Δ Population
Instrument	0.01 (0.01)	0.00 (0.00)	-0.01* (0.00)	0.08*** (0.02)
Dist. to highway (log)	-0.00*** (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.01*** (0.00)
Native businesses (log, 2009)	0.01*** (0.00)	-0.00*** (0.00)	-0.00 (0.00)	-0.02*** (0.00)
Foreign pop. in mun. (log, 2009)	-0.00 (0.00)	-0.00*** (0.00)	0.00*** (0.00)	-0.00 (0.00)
Foreign pop. around (log, 2009)	0.00* (0.00)	0.00*** (0.00)	0.00 (0.00)	0.01*** (0.00)
Population (log, 2009)	-0.00 (0.00)	0.00*** (0.00)	0.00** (0.00)	0.00 (0.00)
Population around (log, 2009)	-0.00 (0.00)	-0.00*** (0.00)	-0.00 (0.00)	-0.01* (0.00)
Prop. unemployed (2009)	-0.30*** (0.03)	-0.06*** (0.01)	-0.35*** (0.03)	-0.19** (0.09)
Prop. college graduate (2009)	-0.00 (0.01)	-0.06*** (0.01)	-0.04*** (0.00)	0.07 (0.08)
Prop. HS graduate (2009)	0.18*** (0.02)	-0.45*** (0.02)	-0.06*** (0.01)	0.46*** (0.06)
Urban/Rural controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Num. obs.	33721	33721	33721	33721
R ²	0.25	0.48	0.19	0.30
Adj. R ²	0.24	0.48	0.19	0.29

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

The change in unemployment and in the logged population has a significantly relationship with the instrument at the conventional significance level. If either variable affects the outcome, this could suggest a violation of the exclusion restriction. Table 12 shows the 2SLS

estimates adjusted for the change in population and for the change in foreign-born population, which was shown to be non-significant but moderately large in Table 1. The results are robust: the coefficients on all three variables are weak and the coefficient of interest on exposure is unchanged in all four specifications.

Table 12: Second Stage with Additional Adjustment

	2SLS with Adjustment for:			
	Δ Pop. (log)	Δ Foreign Pop. (log)	Δ Unemp.	All
Δ Exposure (log)	0.08*** (0.03)	0.07*** (0.03)	0.07*** (0.03)	0.08*** (0.03)
Foreign pop. (log, 2009)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)
Δ Foreign. pop (log)		-0.00*** (0.00)		-0.00*** (0.00)
Population (log, 2009)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Population around (log, 2009)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Δ Population (log)	-0.03 (0.02)			-0.03 (0.02)
Foreign pop. around (log, 2009)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Prop. unemployed (2009)	0.27*** (0.10)	0.29*** (0.10)	0.29*** (0.11)	0.31*** (0.11)
Δ Prop. unemployed			0.05 (0.08)	0.07 (0.07)
Prop. college graduate (2009)	-0.33*** (0.05)	-0.34*** (0.05)	-0.34*** (0.05)	-0.33*** (0.05)
Prop. HS graduate (2009)	-0.17*** (0.05)	-0.18*** (0.05)	-0.18*** (0.06)	-0.17*** (0.05)
Native businesses (log, 2009)	-0.01 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Dist. to highway (log)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Urban/Rural controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Num. obs.	33720	33720	33720	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

M.2 Placebo Treatment

As a further test of the identifying assumptions, I run the first-stage specification with a placebo period for the treatment. A significant relationship would indicate a potential violation of the exogeneity of the instrument, which could be correlated pre-existing local conditions that affect support for the far right. I estimate the effect of the instrument on the 2001 to 2009-change in exposure to immigrant businesses. Table 13 shows a clearly null estimate.

Table 13: First stage with Placebo Period as Treatment (2001 to 2009)

	Placebo First Stage Δ Exposure (log, 2001 - 2009)
Instrument	-0.03 (0.19)
Dist. to highway (log)	0.01 (0.01)
Native businesses (log, 2009)	-0.02 (0.02)
Foreign pop. in mun. (log, 2009)	-0.00 (0.00)
Foreign pop. around (log, 2009)	-0.04*** (0.01)
Population (log, 2009)	0.00 (0.02)
Population around (log, 2009)	0.06*** (0.02)
Prop. unemployed (2009)	0.06 (0.47)
Prop. college graduate (2009)	-0.28** (0.11)
Prop. HS graduate (2009)	0.02 (0.33)
Urban/Rural controls	Yes
Department FE	Yes
Num. obs.	33721
R ²	0.06
Adj. R ²	0.06

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

This table reports a first-stage estimate in which the true treatment (change in exposure between 2009 and 2019) is replaced by a placebo treatment (change in exposure between 2001 and 2009). The coefficient is very close to zero and non significant.

M.3 Monotonicity

To assess monotonicity, I estimate the first stage separately for each municipality category defined by Insee. The results are displayed in Figure 24. The estimate is positive everywhere but for category 2 (intermediate urban centers), for which it is very imprecisely estimated. In Figure 25, I show the second-stage estimates with one municipality type excluded at a time. The results are robust to the exclusion of any of the municipality types, suggesting that they are not driven by a small subgroup of municipalities.

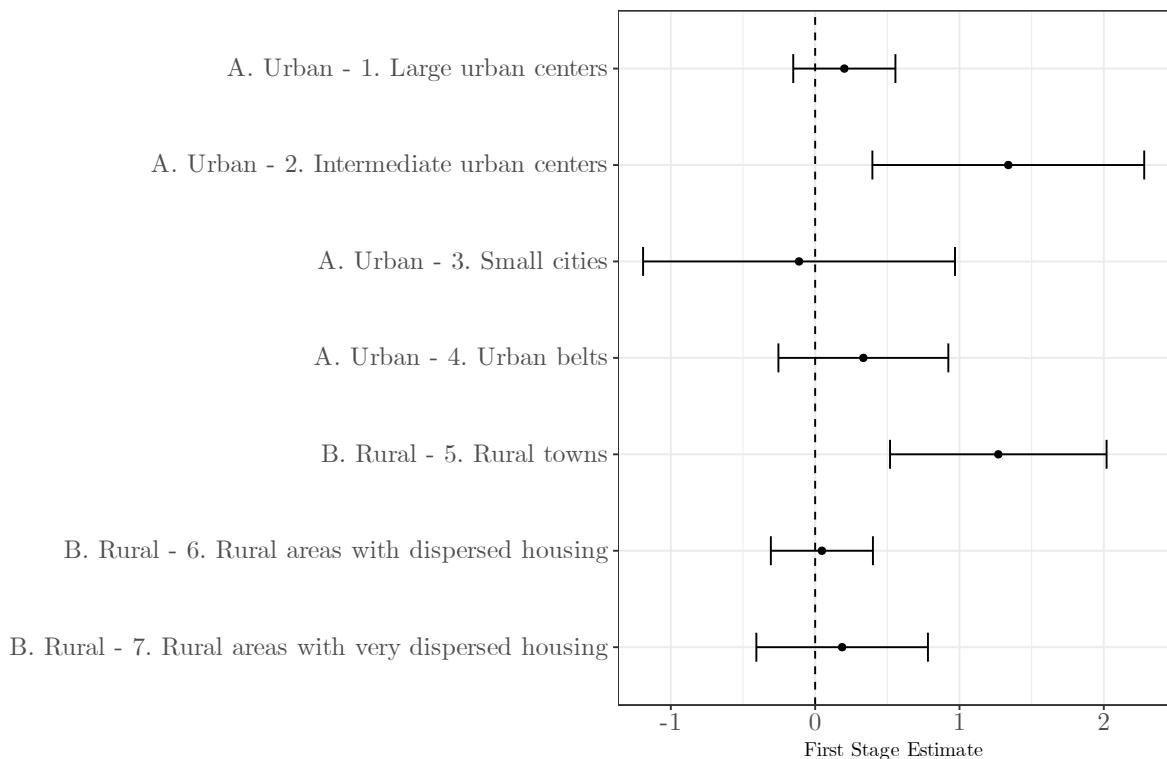


Figure 24: First-Stage Estimate by Municipality Type

N Other Voting Outcomes

To examine whether the effect is due to voter mobilization or switches, I also estimate the effect of an increase in exposure on voter turnout, and replicate the main specification using the RN vote as a proportion of all registered voters instead of a proportion of voters who turned out to vote. Table 14 displays the corresponding estimates.

The estimate for the proportion of registered voters is significant and positive, as in the

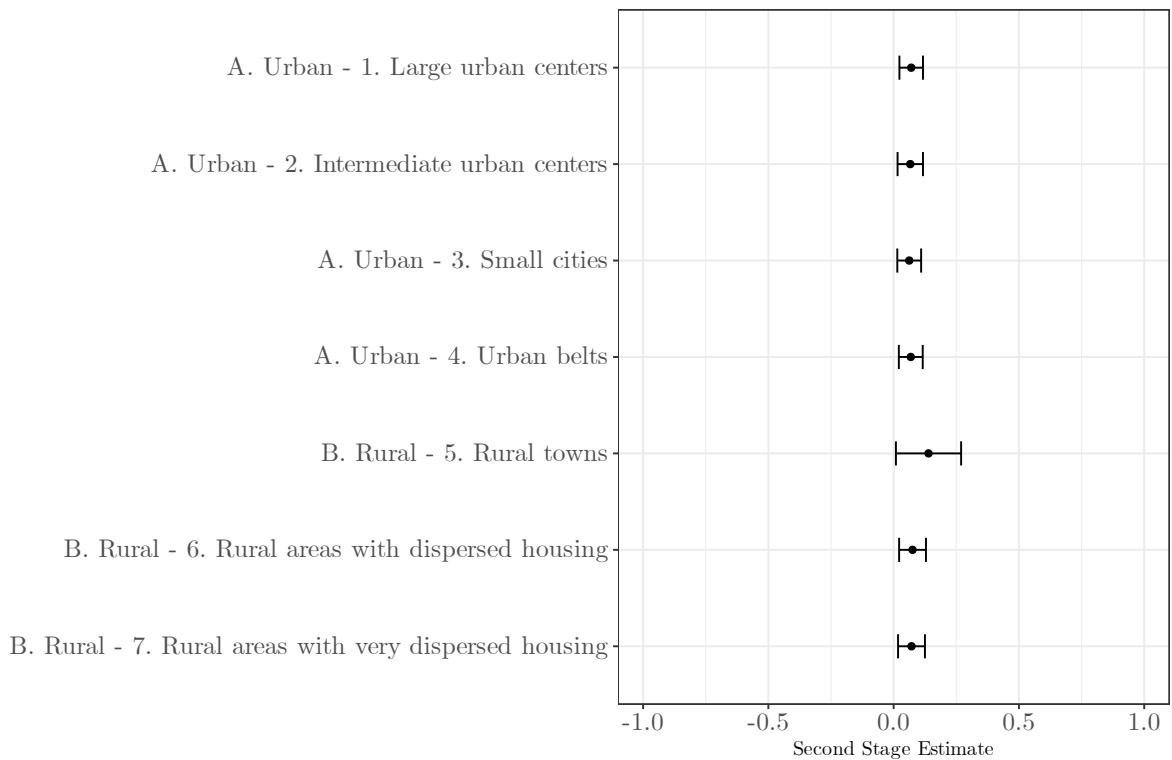


Figure 25: Leave-One-Out Robustness Check, Excluding Municipality Types

same specification, but mechanically smaller (since the denominator is larger). The results suggest that the effect may largely be driven by the mobilization of new voters, as the magnitude of effect on turnout is commensurate with the magnitude of the effect on the change in registered voters' support for the far right.

Table 14: Second-Stage Stage Estimates, Other Voting Outcomes

	2SLS	
	Δ RN Prop. of Reg. Voters	Δ Turnout
Δ Exposure (log)	0.03** (0.01)	0.04* (0.02)
Foreign pop. (log, 2009)	-0.00*** (0.00)	-0.00** (0.00)
Foreign pop. around (log, 2009)	-0.00 (0.00)	-0.00 (0.00)
Population (log, 2009)	-0.00*** (0.00)	-0.00 (0.00)
Population around (log, 2009)	0.00 (0.00)	0.00 (0.00)
Prop. unemployed (2009)	0.06 (0.06)	-0.00 (0.04)
Prop. college graduate (2009)	-0.14*** (0.03)	-0.01 (0.03)
Prop. HS graduate (2009)	-0.07** (0.03)	0.00 (0.03)
Native businesses (log, 2009)	-0.00 (0.00)	0.00 (0.00)
Dist. to highway (log)	-0.00 (0.00)	0.00 (0.00)
Urban/Rural controls	Yes	Yes
Department FE	Yes	Yes
F (first stage)	45.5	45.5
Outcome Mean	0.12	0.11
Num. obs.	33720	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

This table reports second-stage estimates in which the main outcome (change in support for the RN between 2009 and 2019) is replaced by 1) change in support for the RN as a proportion of registered voters and 2) turnout.

O Robustness Check: Other Elections

Table 15 displays the results of the main IV specification using the 2007 and 2017 presidential elections instead of the 2009 and 2019 presidential elections. The magnitude and significance of the results are unchanged.

Table 15: 2SLS with (Presidential Elections)

	Δ Exposure (log)	Δ RN Vote Share
	First Stage	Second Stage
Instrument	0.461*** (0.140)	
Δ Exposure (log)		0.075** (0.031)
Foreign pop. (log, 2008)	0.010** (0.004)	-0.003*** (0.001)
Foreign pop. around (log, 2008)	-0.016 (0.011)	-0.001 (0.001)
Population (log, 2008)	0.033* (0.018)	0.002 (0.003)
Population around (log, 2008)	0.029* (0.016)	0.001 (0.002)
Prop. unemployed (2008)	-1.547*** (0.417)	0.102 (0.102)
Prop. college graduate (2008)	-0.286*** (0.087)	-0.210*** (0.037)
Prop. HS graduate (2008)	0.355 (0.271)	-0.038 (0.043)
Native businesses (log, 2008)	-0.084*** (0.018)	-0.008** (0.003)
Dist. to highway (log)	-0.006*** (0.002)	0.000 (0.000)
Urban/Rural controls	Yes	Yes
Department FE	Yes	Yes
Num. obs.	34777	34409

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

P Robustness Check: Other Distance Measure for Exposure

In this section, I replicate the second-stage estimation but change the unit of distance d used to construct the measure of exposure in Section 4.2. Instead of using 100 meters, I use 1000 meters, allocating significantly more weight to businesses located away from the municipality of interest. Table 16 shows that the 2SLS is still positive and significant, with an even larger magnitude.

Table 16: Second Stage with Larger Distance for Weights

	Δ RN Vote Share 2SLS (distance in km)
Δ Exposure (log)	0.184*** (0.070)
Foreign pop. (log, 2009)	-0.003*** (0.001)
Foreign pop. around (log, 2009)	-0.003 (0.002)
Population (log, 2009)	-0.007** (0.003)
Population around (log, 2009)	0.002 (0.003)
Prop. unemployed (2009)	0.332** (0.146)
Prop. college graduate (2009)	-0.369*** (0.070)
Prop. HS graduate (2009)	-0.233*** (0.067)
Native businesses (log, 2009)	0.014 (0.011)
Dist. to highway (log)	0.002 (0.002)
Urban/Rural controls	Yes
Department FE	Yes
F (first stage)	26.1
Outcome Mean	0.210
Num. obs.	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

The significantly larger result may be due to the fact that the 10-km radius limit cre-

ates more unnatural discontinuities in the 1000-meter version than the 100-meter version, for which businesses away from the municipalities are given almost no weights long before they reach the radius.

Q Results for Other Parties

I re-estimate the second stage with change in the support for other major parties as the outcome variables. The theory presented in Section 2 indicates that immigrant businesses will primarily affect voting outcomes by triggering native backlash, which could translate into voting behavior changes when vote choice is a function of attitudes toward immigration. Because none of the other parties has historically centered the issue of immigration like the far right has, no strong effect is expected.

Table 17: Second Stage for Other Parties

	2SLS			
	Far Left	Green Party	Socialists	Republicans
Δ Exposure (log)	-0.02 (0.02)	-0.01 (0.01)	-0.01 (0.02)	0.03 (0.02)
Dist. to highway (log)	0.00 (0.00)	-0.00*** (0.00)	0.00 (0.00)	-0.00 (0.00)
Native businesses (2009)	-0.00 (0.00)	0.00 (0.00)	0.01*** (0.00)	-0.02*** (0.00)
Population (log, 2009)	0.01* (0.00)	-0.00 (0.00)	-0.00* (0.00)	0.01*** (0.00)
Population around (log, 2009)	-0.00 (0.00)	0.00 (0.00)	-0.00* (0.00)	0.00 (0.00)
Prop. unemployed (2009)	-0.01 (0.05)	-0.19*** (0.04)	-0.19*** (0.04)	0.51*** (0.06)
Prop. college graduate (2009)	0.06*** (0.02)	-0.06*** (0.01)	0.16*** (0.01)	-0.10** (0.04)
Prop. HS graduate (2009)	0.05** (0.02)	-0.03*** (0.01)	0.13*** (0.02)	-0.02 (0.04)
Urban/Rural controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
F (first stage)	47.9	47.9	47.9	47.9
Num. obs.	33720	33720	33720	33720

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

This table reports second-stage estimates in which the main outcome (change in support for the RN between 2009 and 2019) is replaced by corresponding outcomes for other parties. The coefficient are all non-significant but in the expected directions.

Table 17 displays the results. As expected, change in exposure to immigrant businesses does not cause significant change in the electoral performance of the other political parties. However, the direction of the results provides a sanity check based on the general position of

these parties on the issue of the immigration: support for left-wing parties who are generally more favorable to pro-immigration policies decreases, while support for the mainstream conservative party increases. This is consistent with an interpretation of the main results driven by change in attitudes toward immigration.

R Alternative Identification Approaches

R.1 Two-Way Fixed Effects Estimation

Table 18 displays the TWFE estimates.

Table 18: TWFE Estimation

	RN Vote Share Model 1
All exposure (storefront, log)	0.002** (0.001)
All exposure (hidden, log)	-0.005 (0.003)
Foreign pop. (log)	-0.001*** (0.000)
Foreign pop. around (log)	-0.009 (0.008)
Population (log)	0.027** (0.012)
Population around (log)	0.039 (0.059)
Prop. unemployed	0.155 (0.133)
Prop. college graduate	-0.241*** (0.027)
Prop. HS graduate	0.430*** (0.091)
Native businesses (storefront log)	0.022** (0.010)
Native businesses (hidden, log)	-0.104** (0.043)
Year FE	Yes
Municipality FE	Yes
Num. obs.	434048
Adj. R ² (full model)	0.847
Adj. R ² (proj model)	0.059

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

Consistent with the results presented in Section 5, column A shows that a higher exposure to storefront immigrant businesses is associated with an increase in support for the far right. The magnitude of the effect suggests that a doubling of the number of immigrant businesses locally is associated to a quarter-percentage-point increase in support for the far right.

The estimate's magnitude is significantly smaller than the 2SLS estimate from Table 2, but commensurate with naive OLS³².

The coefficient on exposure to non-visible businesses is not significant. This indicates that their role as symbols may be paramount in explaining their influence on natives: even though hidden businesses contribute to the local economy, they do not appear to affect attitudes, either because natives are not aware of their existence, or because they do not mind businesses that are not symbols of group identity.

R.2 Approach inspired by Imai et al. (2023)

To estimate the causal effect of exposure, a continuous treatment variable in a panel setting, I adapt the PanelMatch framework from Imai et al. (2023) into a stepwise treatment design. First, I discretize the continuous treatment into K ordered bins (e.g., thirds or deciles), such that each observation (a municipality i at an election time t) is assigned a discrete "treatment level" ranging from 1 to K . Then, I estimate the average treatment effect of a one-step increase in the treatment, i.e., the causal effect of moving from bin k to bin $k + 1$.

The target estimand is thus the average treatment effect on the treated of a marginal increase in exposure. Formally,

$$ATT_{k \rightarrow k+1} = \mathbb{E} [Y_{t+1}(k+1) - Y_{t+1}(k) \mid D_{it} = k+1, \text{treatment history}]$$

Where D_{it} is the binned treatment status of municipality i in election t . These ATT estimates are computed for each adjacent bin pair using Imai et al.'s (2023) `PanelMatch` package, which matches treated and control units based on pre-treatment covariate histories (treatment status history).

This approach relies on a conditional parallel trends assumption: for each bin step $k \rightarrow k + 1$, I assume that in the absence of treatment escalation, the outcome trajectories of treated and matched control units would evolve similarly. By estimating ATT separately for each bin transition, I avoid imposing strong parametric assumptions about the functional form of the

³²For an interpretation of the difference in magnitude between OLS and 2SLS estimates, see the discussion of bias and principal stratification in Section 5.

treatment effect across the range of the continuous exposure.

Estimates are likely to depend on the choice of the number of bins K . For low K s (for example only two halves), the bins are very large, making the estimates substantively hard to interpret. At large K s, the number of observations in each individual bin decreases, increasing the imprecision. I therefore run the specification for all $K \in \{1, \dots, 10\}$.

To summarize the overall effect of marginal changes in treatment intensity, I compute an inverse-variance weighted average of the stepwise ATT estimates. This provides a single estimate of the average effect of increasing the treatment by one bin, while accounting for the varying precision of each step. This estimator captures the average local effect of increasing treatment intensity, while allowing for arbitrary nonlinearity in the effects across the range of bins.

Results are displayed in Figure 26. The horizontal axis shows the number of bins K . The effect is consistently positive across choices of K .

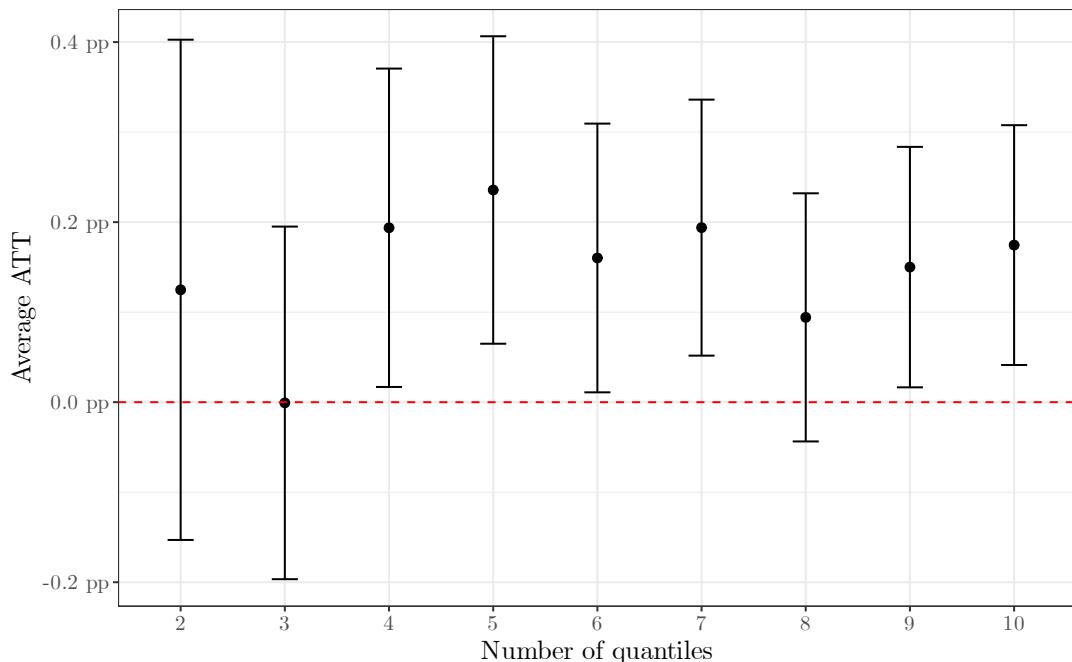


Figure 26: Average Step-Wise ATT Estimates for Different Quantile Numbers

S Survey Experiment Design

The eligible population for the study is the set of registered CloudResearch's Prime Panels platform survey takers living in France and speaking French. CloudResearch has been widely used by researchers and, unlike many competing online platforms, includes a large number of potential French respondents. This is a convenience sample that may not be nationally representative. It is in particular likely to be younger and more digitally proficient on average than the French population. While this may affect the generalizability of the findings, some studies have shown that convenience samples can provide findings consistent with nationally representative samples (Berinsky et al., 2012; Mullinix et al., 2015; Coppock et al., 2018). I will estimate Target Population quantities using weighting-based estimators (Egami and Hartman, 2022). The expected sample size is 1,500 respondents.

Assignment to treatment is blocked on two variables: whether the respondent voted for Marine Le Pen (the far-right candidate) in the second round of the last presidential election, and whether the municipality that the respondent resides in is rural or urban, where rural is defined using the following Insee categories: "rural villages", "rural areas with dispersed settlements", "rural areas with highly dispersed settlements". Blocking is implemented using the "Evenly present elements" functionality of Qualtrics within block, ensuring that within each of the four blocks, Qualtrics allocates the same number of respondents to the four groups.

More information is available in the pre-registration at <https://osf.io/3ydxk/>.

S.1 Newspaper Articles (Translations)

In Perpignan, the bakery-pastry shop is leaving the Halles Vauban for good: find out what concept will replace it

Published on 11/05/2024 at 5:48 PM

By Diane Sabouraud

The Halles Vauban pastry shop ceased all operations on October 31st. Within its walls, a brasserie project is being spearheaded by two young Catalan entrepreneurs.

The Halles pastry shop has closed its doors for good. It's the most visible brand at the Halles Vauban, as it was the first to introduce the twenty or so corners located in the former eponymous passageway. It's therefore difficult to walk past without noticing that the stalls have been emptied of their delicacies, the breads from their baskets, and the coffees from their cups, which still sit proudly on the back counter. However, the establishment ceased all operations on the evening of Thursday, October 31, 2024.

"They've been trying to sell for months, as business hasn't been going as planned," they say at the Halles Vauban.

But such a location obviously doesn't stay on the market for long. According to information from L'Indépendant, a buyer is already in the running. While they wish to remain discreet while the administrative process is finalized, two young entrepreneurs have launched procedures to take over the business. But they will completely change the nature of the place. Out with the pastry shop, in with a brasserie. The project requires major renovations to redesign the premises. Ultimately, 60 to 70 seats are expected inside and around 40 on the terrace.

...

A new bakery coming soon?

We'll have to wait a while before discovering this new culinary offering in town. While the transaction is finalized and the renovations are completed, the opening is not expected to take place before the end of the year.

Figure 27: Control Group Article (English Translation)

Perpignan City Council: The establishment of "halal restaurants" on Rue de la Cloche-d'Or raises eyebrows among Louis Aliot and the opposition

Published on 06/22/2022 at 6:52 PM, updated on 06/23/2022 at 10:34 AM

By Diane Sabouraud

During the city council meeting this Wednesday, June 22, Louis Aliot returned to the issue of the purchase of the eleven empty businesses on Rue de la Cloche d'Or. He asserted, among other things, that the buyers were companies specializing in halal catering. And he protested.

...

In the Perpignan assembly, the RN mayor, Louis Aliot, raised the issue before beginning the agenda. For the record, lindependant.fr revealed the day before that the eleven businesses purchased by entrepreneur Abi-Nader Chukri in October 2020 to create a gourmet street had been resold. The buyer is believed to be a group of investors, represented by a spokesperson. "This is a rather serious matter," Louis Aliot announced solemnly. "Our services have received a declaration of intent to sell (DIA) concerning these eleven businesses. We met with the selling owner this morning, who confirmed our concerns. He plans to sell to companies that share the common goal of providing halal restaurants."

...

"This also raises a question of overall identity."

The RN mayor insists that this plan for halal restaurants is in "total opposition to our plan for the city center, which is to make it a high-quality commercial hub. And this also raises a question of overall identity."

Two proposals were submitted to the city council: buying back the businesses or preempting them. On behalf of the opposition LR party, Pierre Parrat advocated for pre-emption, assuring that "given the state of the shops, it won't cost you a lot. And whatever the cost, you must act. We share your concerns and, in this regard, we will be fully in line with the mayor's decision."

Figure 28: Treatment Group Article (English Translation)

S.2 Example of Businesses shown in Second Treatment

Grill Garden "Le Bar A Bieres Et Burgers"

Traiteur, Restaurant Barbecue, Magasin De Bière,

Pâtisserie, Bar À Cocktails, Restaurant Français, Restaurant

De Hamburgers, Pâtisserie, Bar À Vin

📍 4 Rue Des Grands Prés, Pontfaverger-Moronvilliers



Connaissez-vous cet établissement : Grill Garden "Le Bar A Bieres Et Burgers" ?

oui

Non

Je ne suis pas sûr

Cet établissement n'existe plus

Figure 29: Qualtrics screenshot—example of business shown in the control condition

Royalkebab2

Kebab, Restaurant De Restauration Rapide

 6 Rue Paul Eluard, Veynes



Connaissez-vous cet établissement : Royalkebab2 ?

oui

Non

Je ne suis pas sûr

 Cet établissement n'existe plus

Figure 30: Qualtrics screenshot—example of business shown in the treatment condition

T Imputation of the Presence of Immigration

Across the main specification, I control for the share of immigrants in the population to ensure that the effect estimated is specific to businesses. In the tables reported so far, I do so by controlling for the share of foreign-born because I do not have data on the share of second-generation immigrants for the regulatory reasons outlined in Section 4.1. I argue that the share of foreign-born residents is most likely strongly correlated to the latent, true share of residents that are immigrants in the sense that I defined immigrant businesses³³.

However, even under this assumption, one may be worried that what the variation in the number of immigrant businesses truly captures is the residual variation in immigrant residents not captured by the foreign-born population. In such a scenario, the estimated effect would be confounded by this latent residual variation. This concern is not salient for the 2SLS estimation but relevant in these two-way fixed effects models. To address it, I use a dataset reported annually by the French government providing the full list of deceased residents in the previous calendar year, including their names and municipality at the time of death. I first classify these names with the algorithm used to classify business owners. Then, I estimate the share of deceased residents that are immigrants using the same rules as above for each municipality. I do so for 2009 and 2019, using the four years before and after each point in time to increase the sample size, under the assumption that the population composition is sticky. I thus estimate the proportion of immigrants among deceased residents in two periods: [2005-2013] and [2015-2023], which I assign to the years 2009 and 2019. I then linearly extrapolate to the other years in the analysis.

Table 19 replicates the results from Tables 3 with an additional control for this variable: the imputed share of immigrants among deceased residents. The results and conclusions are robust to this change, with no notable difference in magnitude or significance.

³³I.e., the share of residents, including second-generation immigrants and beyond, likely perceived as immigrants by natives because of their origins.

Table 19: Linear Models with Imputed Proportion of Immigrants

	RN Vote Share		
	A. Aggregate	B. By origin	C. By type of space
All exposure (storefront, log)	0.003** (0.001)		0.001 (0.001)
All exposure (hidden, log)	-0.001 (0.003)		-0.004 (0.003)
Exposure - Muslim (storefront, log)		0.002** (0.001)	
Exposure - other (storefront, log)		-0.001 (0.001)	
Exposure - Muslim (hidden, log)		-0.002 (0.002)	
Exposure - other (hidden, log)		-0.003** (0.001)	
Exposure × Prop. Soc. Spaces			0.002 (0.003)
Foreign pop. (log)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Foreign pop. around (log)	-0.013* (0.007)	-0.011 (0.007)	-0.011 (0.007)
Imputed foreign pop. (prop.)	-0.165*** (0.050)	-0.229*** (0.058)	-0.230*** (0.059)
Population (log)	0.008 (0.015)	0.014 (0.017)	0.014 (0.017)
Population around (log)	0.039 (0.051)	0.034 (0.053)	0.035 (0.053)
Prop. unemployed	0.109 (0.098)	0.120 (0.093)	0.117 (0.093)
Prop. college graduate	-0.202*** (0.029)	-0.224*** (0.026)	-0.224*** (0.026)
Prop. HS graduate	0.272*** (0.050)	0.349*** (0.064)	0.347*** (0.065)
Native businesses (storefront log)	0.012 (0.010)	0.023*** (0.008)	0.023*** (0.008)
Native businesses (hidden, log)	-0.039 (0.027)	-0.081** (0.032)	-0.081** (0.032)
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Num. obs.	413200	413200	413200
Adj. R ² (full model)	0.848	0.847	0.847
Adj. R ² (proj model)	0.061	0.052	0.051

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

U Robustness Check: Proportion as the Treatment

Across the models presented, the coefficient on the log number of native storefront businesses is also positive and significant, suggesting that the relationship observed could simply be capturing a broader relationship between areas prone to support the far right and development of storefront businesses, perhaps because in many industries this type of activity requires lower skill levels than other forms of employment. To rule out this possibility, I estimate the same model replacing exposure by the weighted proportion of all storefront businesses that are immigrant owned, as opposed to their absolute number. The results are displayed in Appendix 20. In aggregate, an increase in the proportion of businesses in and around a municipality that are immigrant owned leads to a slight increase in support for the far right, but once broken down by immigrant origin, the picture is clear and consistent with Hypothesis 1: an increase in businesses owned by immigrants from Muslim majority countries leads to a strong and significant increase in support for the far right, while an increase in the proportion of businesses owned by other immigrants leads to a decrease in support for the far right.

Table 20: Linear Models with Proportion instead of count

	RN Vote Share	
	A. Aggregate	B. By origin
Prop. immigrant businesses (storefront)	-0.241*** (0.066)	
Prop. immigrant businesses (hidden)	-0.134 (0.095)	
Prop. Muslim businesses (storefront)		1.198*** (0.326)
Prop. other businesses (storefront)		-0.896*** (0.216)
Prop. Muslim businesses (hidden)		-0.022 (0.124)
Prop. other businesses (hidden)		-0.534*** (0.207)
Foreign pop. (log)	-0.001*** (0.000)	-0.001*** (0.000)
Foreign pop. around (log)	-0.014* (0.008)	-0.013* (0.008)
Imputed foreign pop. (prop.)	-0.164*** (0.029)	-0.131*** (0.023)
Population (log)	0.020 (0.015)	0.024* (0.014)
Population around (log)	0.029 (0.049)	0.020 (0.045)
Prop. unemployed	0.158 (0.098)	0.171* (0.095)
Prop. college graduate	-0.240*** (0.029)	-0.214*** (0.026)
Prop. HS graduate	0.408*** (0.069)	0.376*** (0.059)
Number of businesses (storefront, log)	0.013 (0.009)	0.001 (0.010)
Number of businesses (hidden, log)	-0.070** (0.030)	-0.041 (0.028)
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Num. obs.	413200	413200
Adj. R ² (full model)	0.845	0.846
Adj. R ² (proj model)	0.068	0.075

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

This table displays the results of Equation 5, broken down by origin in column 2, but where Exposure is measured as the weighted proportion of all businesses in the area as opposed to the log count.

V Observational Evidence of Economic Effects

In the survey experiment, treated respondents report agreeing more with the claim that immigrant businesses threaten native businesses and the claim that immigrant-businesses pose a threat to local real-estate prices. As an observational test of these mechanisms, I adjust the model shown in Equation 5. First, I interact exposure with the proportion of new immigrant businesses that have replaced a native business between the last and the current period. I code a new business as a displacement if a native business at the same address closed in the 6 months before or after it opened³⁴. I also run the model with the average logged municipal property price (per squared meter) as an outcome variable. Results are displayed in Table 21.

While the interaction term is not significant, results in the first column show a strong positive relationship between the proportion of displacements and support for the RN. An increase by 50 percentage points in the proportion of new immigrant businesses that displaced native businesses is associated with a two-percentage-point increase in support for the far right.

The second column of Table 21 shows no significant relationship between exposure and average property value. However, the negative coefficient is consistent with the survey results and with theoretical work by Faury (2024) suggesting that natives worry that the presence of immigrant businesses deemed undesirable will lower the perceived quality of their neighborhood and negatively affect property values.

³⁴There is likely a small amount of error in the coding of business openings and closings: some businesses register right away, and some businesses close but do not register their closure immediately. I therefore count as instances of displacement cases where the new business opens *right before* the previous business closed in the data.

Table 21: Linear Models: Further Economic Mechanisms

	OLS	
	Outcome: RN Vote Share	Outcome: Avg. m2 price (logged)
Exposure (storefront, log)	0.002** (0.001)	-0.002 (0.003)
Prop. displaced (t-1 to t)	0.008 (0.012)	
Exposure (hidden, log)	-0.005 (0.003)	-0.015 (0.009)
Foreign pop. (log)	-0.001*** (0.000)	0.000 (0.001)
Foreign pop. around (log)	-0.009 (0.008)	0.056*** (0.018)
Population (log)	0.027** (0.012)	-0.009 (0.123)
Population around (log)	0.039 (0.059)	0.292 (0.243)
Prop. unemployed	0.156 (0.133)	-0.043 (0.240)
Prop. college graduate	-0.241*** (0.027)	0.373*** (0.073)
Prop. HS graduate	0.431*** (0.091)	-0.156 (0.100)
Native businesses (storefront log)	0.023** (0.010)	0.052** (0.023)
Native businesses (hidden, log)	-0.105** (0.043)	0.209*** (0.041)
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Num. obs.	431093	284803
Adj. R ² (full model)	0.847	0.875
Adj. R ² (proj model)	0.059	0.005

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).

W Effect on Local Taxes

Table 22 shows that an increase in local exposure to immigrant-owned businesses leads to an increase in the local business tax.

Table 22: Second-Stage Stage Estimate of the Effect on Tax Rates

	2SLS
	Δ Local Corporate Tax Rate
Δ Exposure (log)	3.13*
	(1.61)
Foreign pop. (log, 2009)	0.01
	(0.04)
Foreign pop. around (log, 2009)	0.45**
	(0.18)
Population (log, 2009)	−0.34**
	(0.15)
Population around (log, 2009)	−0.75***
	(0.24)
Prop. unemployed (2009)	−3.07
	(4.35)
Prop. college graduate (2009)	−1.06
	(1.05)
Prop. HS graduate (2009)	4.12*
	(2.25)
Native businesses (log, 2009)	0.42**
	(0.19)
Dist. to highway (log)	−0.02
	(0.08)
Urban/Rural controls	Yes
Department FE	Yes
F (first stage)	45.5
Outcome Mean	12.08
Num. obs.	33719

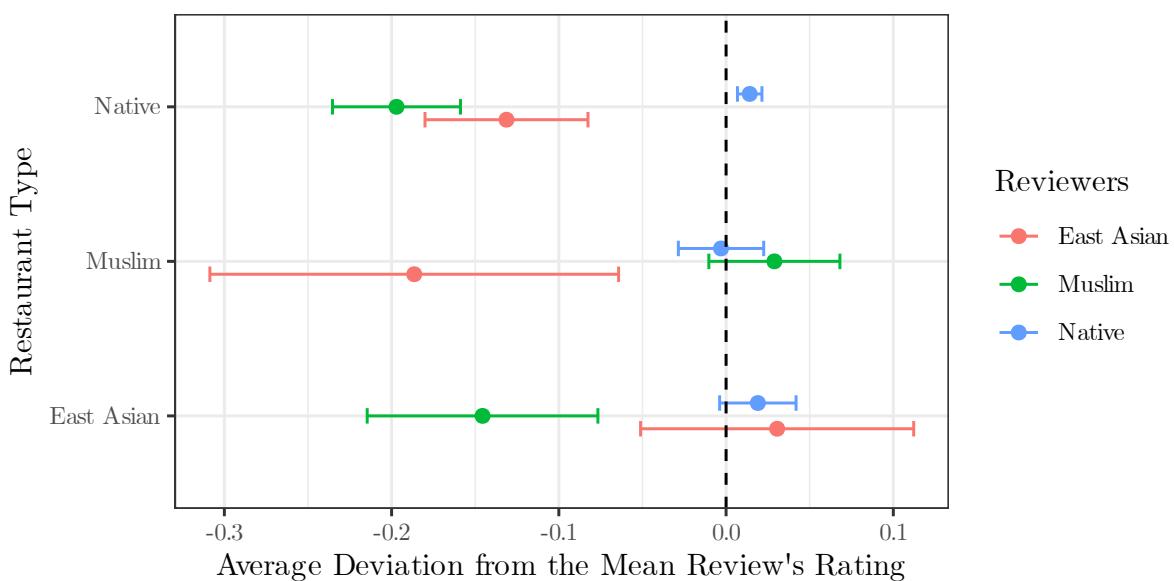
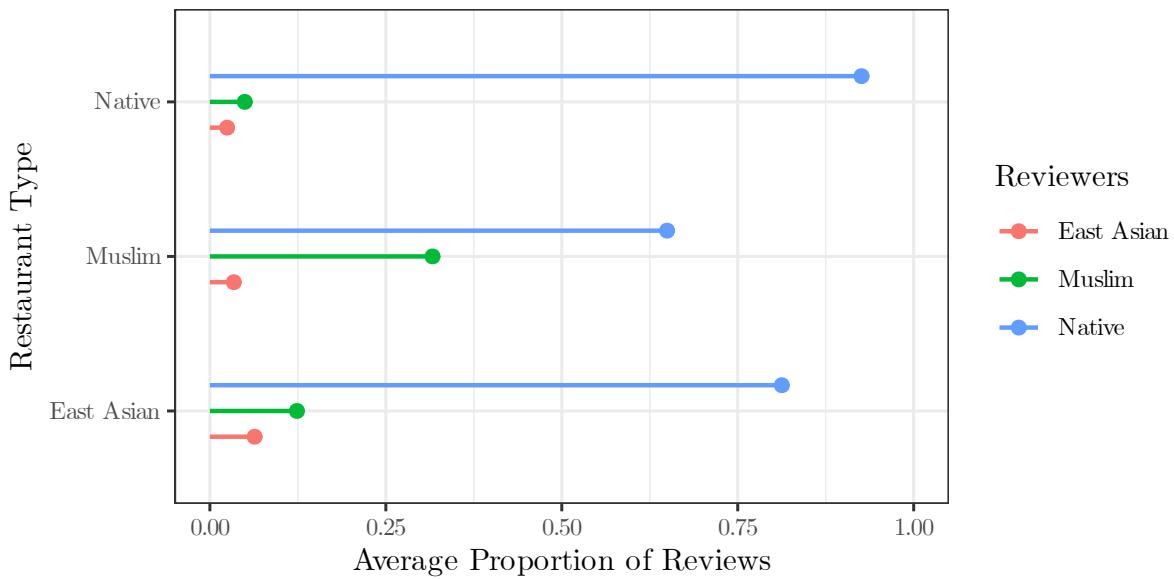
*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km)

X Analysis of Google Maps data

To better understand the native-immigrant interactions embedded in customer-business relationships, I analyze restaurant ratings using the Google Maps API. I first scraped data on all restaurants located in Metropolitan France between December 2024 and January 2025. I then collect up to 300 reviews for a random sample of these businesses. I select the first 300 reviews provided by Google Maps: while no public algorithm specifies which reviews are selected, the order appears to be a combination of reviews deemed informative (in either positive or negative directions) by Google Maps, as well as more recent reviews. I classify the restaurants by the immigrant group they are most likely associated with using the “categories” provided on Google Maps, for example “Lebanese Takeout”, “Japanese Restaurant”, or “French Bistrot”. I exclude businesses whose category is ambiguous and keep three groups: native restaurants, East Asian restaurants, and restaurants from Muslim majority countries. As a result of this process, the final sample includes 4,800 restaurants. I then classify reviewers using the Nam-sor algorithm described in Appendix E. I first exclude reviewers whose names are apparent pseudonyms unlikely to be real first and last names.

Figure 31 displays the proportion of reviewers from each group in each type of restaurant. It clearly shows that even though each group has a preference for associated restaurants, natives remain the primary customers across restaurant types. This is likely a mechanical result of the numbers of natives in the population.

Figure 32 displays the average difference between the reviews of each reviewer group and the average review of each restaurant type. It shows that natives do not tend to rate restaurants from other groups worse.



Y Analysis of Spatial Segregation

I construct indexes of spatial segregation using the `seg` package in R (Hong et al., 2014). I create 100 meter \times 100 meter grids in the zones defined by a 10-km buffer around the downtown area of a municipality. I then estimate the spatial dissimilarity measure and the spatial relative diversity measures created by Reardon and O’Sullivan (2004). Each measure has a maximum of one under complete segregation and takes a value of zero if the distribution of immigrant businesses is equal to that of the general distribution of businesses. I interact the segregation indexes with the main treatment (exposure) and replicate the OLS estimation from Equation 5.

Table 23: Linear Models: Segregation

	OLS	
	RN Vote Share	RN Vote Share
Exposure (storefront, log)	-0.003*	-0.000 (0.001)
Dissimilarity Index	-0.206 (0.149)	
Relative Diversity Index		-0.018 (0.040)
Exposure × Diss. Index	0.048*** (0.016)	
Exposure × Relative Div. Index		0.036*** (0.008)
Exposure (hidden, log)	-0.002 (0.003)	-0.004 (0.003)
Foreign pop. (log)	-0.001*** (0.000)	-0.001*** (0.000)
Foreign pop. around (log)	-0.015** (0.007)	-0.012* (0.007)
Imputed foreign pop. (prop.)	-0.172*** (0.048)	-0.221*** (0.058)
Population (log)	0.007 (0.013)	0.007 (0.018)
Population around (log)	0.039 (0.045)	0.047 (0.055)
Prop. unemployed	0.113 (0.095)	0.119 (0.094)
Prop. college graduate	-0.203*** (0.028)	-0.219*** (0.024)
Prop. HS graduate	0.269*** (0.047)	0.327*** (0.064)
Native businesses (storefront log)	0.014 (0.009)	0.022*** (0.008)
Native businesses (hidden, log)	-0.043 (0.030)	-0.066** (0.033)
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Num. obs.	413200	413200
Adj. R ² (full model)	0.849	0.848
Adj. R ² (proj model)	0.062	0.056

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Conley Standard errors (50 km).