

Does local immigration contribute to the rise of the far-right? Evidence from Spanish municipalities

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Abstract

In 2019, Spain lost its status as one of the few European countries without an anti-immigration party, with the sudden entry of VOX into Parliament. Using municipality data and a past settlements IV, I evaluate the effect of 2004-2019 immigration flows, on VOX and aggregate right-wing vote shares. *First*, immigration inflows at the municipality level have a positive impact on the VOX vote share. Non-economic concerns, about the cultural mix in compositional amenities or of cultural threat, may play a role in fuelling natives' anti-immigrant attitudes - as culturally-close Latin American inflows are unlikely to generate voting responses. Economic threat perceptions might be relevant too, as the effect of immigrant inflows on the VOX vote intensifies when combined with poor or deteriorating local economic conditions. *Second*, I find substantial voting responses for immigration inflows *adjacent to* municipalities where natives reside. Controlling for these reduces effects of inflows at the municipality level - questioning their relevance. Larger voting responses to inflows outside of natives' municipalities of residence could partly be due to the more sporadic inter-group contact between natives and immigrants that may exist.

JEL classification: D72, F22, J15, J61.

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1. Introduction

One of the most significant changes in European democracies over the last decades has been the growing support for far-right and right-wing populist political parties. While these parties are not single-issue, they consistently share a hard-line anti-immigrant rhetoric. France's Front National, Austria's Freiheitliche Partei Österreichs, Germany's Alternative für Deutschland, or Britain's UKIP, are good examples. In 2019, VOX disrupted in Spanish politics, with 15% of vote shares, becoming the first anti-immigration party to enter Parliament.

These developments have motivated scrutiny on whether immigration can fuel support for far-right parties. Several studies for Europe have consistently suggest that this is the case - e.g., in France (Edo et al., 2019), Austria (Halla et al., 2017), Italy (Barone et al., 2016), Germany (Otto & Steinhardt, 2014) or Greece (Chletsos et al., 2018). These papers show that natives' anti-immigrant attitudes are driven by concerns about labour market competition, the welfare state and the cultural mix of local compositional amenities. For Spain, in contrast, previous studies, which have looked at support for the major mainstream parties, suggest that immigration could play a rather timid role in fostering far-right support (Mendez & Cutillas, 2014; Gálvez-Ititesta & Groizard, 2021).

VOX and Spain are a special case worth studying. Firstly, the current immigration rate in Spain is the result of a very high influx of immigration, over about two decades since the end of the 90s. Notably, from 2002 to 2014, the remarkable inflow received in Spain is one of the largest waves of immigration in European history relative to its population (Bruegel, 2015). Secondly, in spite of such immigrant wave, no anti-immigration party in Spain had managed to irrupt to the scene, until the sudden incursion of VOX in 2019. Yet, the extent to which VOX capitalized on anti-immigration attitudes remains unclear, as it entered Parliament at an extraordinary time in Spanish politics - with corruption scandals of mainstream right-wing party PP, and the push for independence in Catalonia.

Using municipality data, I evaluate if the VOX vote share in 2019 can be associated to a treatment consisting of 2004-2019 foreign-born population flows. To derive causal estimates, I exogenize immigration flows with a past settlements instrument. Further, for robustness, I estimate a first-differenced model, which exploits changes in immigration flows and aggregate right-wing vote between election years from 2004 to 2019. Finally, while the core my paper addresses voting responses to immigration flows at the municipality level (*in-municipality immigration*), I also subsidiarily evaluate responses to immigration flows in the adjacent areas of municipalities (*neighbouring immigration*).

My findings are grouped in three areas. First, in-municipality immigration inflows have a positive and statistically significant average effect on the VOX vote share. This suggests that VOX has indeed capitalized on latent anti-immigration attitudes. Taking into account naturalization of foreign-born individuals and their possible impacts on municipalities' voting outcomes, a 1 pp increase in the population share of immigrants might drive an increase in the VOX vote share from 0.281 to 1.032 pp. The back-of-the-envelope calculation would imply that the 2019-2004 immigration growth treatment accounts from 7.5% to 27.4% of the VOX vote. Altogether, the range of estimates that I find are broadly in line with studies in other European countries - suggesting a similar story for Spain.

Second, I note a pattern of mechanisms also consistent with existing European studies. On the one hand, non-economic concerns about immigration, such as worries about the cultural mix in compositional amenities or of cultural threat, may be a pertinent channel. American

immigration, primarily from culturally-close Latin America, is unlikely to generate anti-immigrant vote responses. Instead, African, Asian and European immigrants might generate positive voting impacts - their growth is likely particularly driven by relatively more culturally-distant Morocco, China and Romania. On the other hand, I also find that economic concerns about immigration might partly explain VOX support. The effect of immigrant inflows on the VOX vote share intensifies in municipalities with poor or deteriorating local economic conditions, in terms of unemployment.

Third, immigration inflows adjacent to municipalities where natives reside, possibly within travelling distance, i.e. neighbouring immigration, generate substantial voting responses. The latter effects could be greater, relative to responses found for immigration measured at the very municipality level, i.e. in-municipality immigration, discussed above. In fact, point estimates of responses to the latter reduce, once I control for neighbouring flows. This raises caution about my in-municipality immigration estimates, and questions the extent to which immigration flows at the very municipality level matter. The greater effects for neighbouring immigration on voting could partly be due to the more sporadic inter-group contact between natives and immigrants that may exist outside of natives' municipality of residence - although there could exist other channels.

I believe that I make two contributions. First, I contribute to evidence of the Spanish experience. To my knowledge, I provide the first analysis with municipality data, and specific to VOX. This complements research by Gálvez-Iniesta & Groizard (2021) and Mendez & Cutillas (2014). Using province-level data, these studies only find positive impacts on right-wing support for irregular immigrants, which represent a very limited portion of immigrants, or a quantitatively small effect for African immigrants. The fact that I identify more intense voting responses might suggest that the channels by which immigration raises anti-immigration attitudes intensify at units smaller than the large 52 provinces in Spain. This is consistent with David et al., (2018), who argue that natives' economic and non-economic concerns might be weakly captured when immigration is aggregated at large units.

Second, I contribute to incipient international literature studying the effects of immigration located outside of natives' place of residence. My analysis for neighbouring immigration may resonate well with Evans & Ivaldi (2021). Using individual-level data, they find that French natives that reside in high-immigration communes do not respond with more far-right vote, while those that live adjacent to high-immigration communes do. My paper might provide confirmatory evidence, with an alternative measure. First, I evaluate effects of immigration change, instead of effects of immigration levels, as it is the former that is more likely a concern for natives (Kaufmann, 2017). Second, using municipalities as units, I estimate effects of adjacent overall exposure to immigration, rather than the effect of exposure to high-immigration areas - hence providing a broader contextual measure.

My paper is organized as follows. [Section 2](#) provides background on the Spanish context, and mechanisms by which immigrants can fuel anti-immigrant attitudes of natives. [Section 3](#) presents the data. [Section 4](#) discusses the identification strategy, with [Section 4](#) then exploring the working of the past settlements' instrumental variable. [Section 6](#) contains the results for the effect of in-municipality immigration, and its heterogeneity - my core analysis. [Section 7](#) discusses the effects of neighbouring immigration, as subsidiary analysis. Finally, [Section 8](#) concludes by revisiting the meaning of my results and discussing future avenues of research.

2. Background

2.1 The Spanish experience

Spain has immigration figures in line with other European countries. As of 2021, the overall share of foreign-born population stands at around 15%. This is not substantially different from the figures in France (13%), Italy (10%), Germany (18%) or Austria (19%) - according to Eurostat. However, what makes the case of Spanish immigration somewhat unique, is the fact that the current immigration rate is the result of an extraordinarily high inflow that has taken place over roughly just two decades. It was not after joining the European Union in 1986, that Spain started being a receiver country (Alberto Valore & Coca, 2014). Notably, from 2002 to 2014, Spain received an accumulated immigrant inflow of 7.3 million, in one of the largest waves of migration in European history, relative to its population (Bruegel, 2015). Key contributors were African immigrants, primarily from Morocco. Southern American countries, too. And Romania, as it joined the EU - eventually becoming the largest community from Europe.

Despite such extraordinary inflow, Spain did not have an anti-immigration party, until the recent incursion of VOX in 2019. Alonso & Rovira Kaltwasser (2015) argue that, while the rapid inflow might have certainly generated political demand for an anti-immigration party, three supply-side factors might have prevented its entry. First, policy positions of mainstream right-wing party PP might have successfully catered to the preferences of far right-wing voters, by including in its agenda issues that populist parties try to capitalize on. Second, the electoral system imposes high barriers to entry to political newcomers. This is due to disproportional allocation of seats, combined with rules that penalize fragmented support across the territory. Third, party competition has been increasingly shaped by conflict between national and regional parties, leaving little space for newcomers.

VOX's anti-immigration discourse echoes parties of similar populist and radical right flare across Europe (Rama et al., 2021). Its agenda advocates for the deportation of all undocumented immigrants, the deportation of those who commit any crime, and an end to social policies that serve to attract immigrants (VOX, 2018). This hard-line stance on immigration breaks away from pre-existing discourses from the rest of the Spanish parties (Morales et al., 2015). Yet, VOX is far from being a single-issue anti-immigrant party. It also strongly supports Spanish nationalism, authoritarian "law and order" and traditional conservatism values (Turnbull-Dugarte, 2019).

VOX incurred in general elections for the first time in 2015, obtaining negligible support. When VOX entered Parliament in 2019, with roughly 15% of votes, two key developments dominated the political battlefield. First, in the aftermath of the illegal 2017 independentist referendum, regional authorities in Catalonia pushed for independence. VOX's emphasis on national unity, strongly positioned against Catalan independence, might have allowed itself to differentiate from mainstream right-wing PP (Turnbull-Dugarte, 2019). Second, PP was plagued with corruption scandals, resulting in a less crowded right-wing space (Guriev & Papaioannou, 2022). These developments might have possibly alleviated supply-side constraints, discussed earlier, for an anti-immigration party to abruptly enter. Yet, the question of whether VOX support capitalized on latent anti-immigration attitudes, is still to be empirically assessed.

2.2 A review to channels

In this section, I review economic and non-economic channels, by which immigration can lead to natives voting to anti-immigration parties. I will also take the opportunity to elaborate three hypotheses, that I leverage in my empirical exercise.

Regarding economic channels, first, some have found that natives worry about welfare state implications of the arrival of immigrants. For example, natives worry they may worry that immigrants compete with them for public services, especially when access to them is poor (Cremaschi et al., 2022), or that immigrants represent a tax burden on public finances (Facchini & Mayda, 2009). Second, others have found evidence that anti-immigrant vote is fuelled by natives worrying about labour market competition (Barone et al., 2016; Bolet 2020; Chletsos et al., 2018; Edo et al., 2019; Halla et al., 2017). While adverse effects of immigration on natives' employment outcomes are often small or insignificant (Aubry et al., 2022 for a general review; González & Ortega, 2017, for Spain), labour market competition concerns might be particularly salient for low-skill natives, particularly as immigrants at all skill levels are compelled to accept lower-skilled jobs (Borjas, 2003; Dustmann et al., 2013).

Perhaps more directly linked with the labour market competition channel, several have shown that the positive effects of immigration on radical right voting might compound when combined with poor or deteriorating local economic conditions. Bolet (2020) and Halla et al. (2017) find that local economic deprivation, as measured through higher background unemployment, has intensified far-right support in France and Austria. They find that *actual* or *perceived* labour market competition may be amplified when natives experience poor economic conditions. In a similar vein, the deterioration of economic conditions may generally fuel economic insecurity of natives, leading to higher far-right vote (Halikiopoulou & Vlandas, 2016; Witte, 1999). These insights led to an *economic deprivation hypothesis*:

a. Economic deprivation hypothesis - Municipalities that are or become more economically deprived, as measured by unemployment, are more likely to respond to immigrant inflows with higher far-right vote shares.

Moving to non-economic channels, research shows that anti-immigrant attitudes intensify with larger cultural distances between natives and immigrants (Bargain et al., 2022; Peresman et al., 2021; Brunner & Kuhn, 2018; Edo et al., 2019). Some have framed this finding as natives placing value on the cultural mix of local population, with more cultural diversity reducing the utility they derive from local compositional amenities. Specifically, natives might worry that immigration dampens the value they derive from their neighbourhoods, workplaces, shopping areas or schools (Halla et al. 2017). Card et al. (2012), using European survey data, finds that compositional amenities concerns, related to more cultural diversity, are 2 to 5 times more important relative to economic reasons.

More intense reactions to culturally-distant immigrants have also been framed as natives considering immigrants as a threat to their cultural dominance (a by-product of Blumer 1958's group threat theory). The argument is that natives may fear that the cultural change that may come with immigration challenges the dominance of the native group, in terms of language, ethnicity, values or religion. Analysis of European survey data shows individuals with stronger cultural and national identity views are less enthusiastic about immigrants about high levels of immigration (Sides & Citrin, 2007). In sum, these ideas led to the following hypothesis:

b. Compositional amenities and cultural group threat hypotheses - Municipalities that register immigrant inflows from more culturally distant origins are more likely to register increased far-right vote shares.

On the opposite direction to the two non-economic channels above, inter-group contact theory highlights that contact between natives and immigrants can alleviate prejudices, stereotypes and lack of trust (Allport, 1954). Inter-group contact has been suggested by studies noting lower far-right vote by natives, with the quasi-random arrival of refugees in municipalities of Austria, Denmark and France (Dustmann et al., 2019; Schneider-Strawczynski, 2021; Steinmayr, 2016). More intense reductions in far-right vote are noted for shorter cultural distance between natives and refugees, and when their arrival is less disruptive in terms of inflows magnitude (Schneider-Strawczynski, 2021).

Further, Evans & Ivaldi (2021) has shown that inter-group contact effects require immigration presence *inside* areas where natives reside. They show that French individuals living inside communities with high immigrant presence do not to respond with higher FN vote. In contrast, individuals that live adjacent to communities with high immigrant presence (at distances from 5 to 20km) do seem more likely to vote for FN. This might be explained by natives living in areas with immigrant presence experiencing regular contact with immigrants (in neighbourhoods, schools, residential areas), while only having sporadic interactions with immigrant presence in areas adjacent to where they live (e.g. commuting or retail activities). On the basis of these ideas, I develop the following hypothesis:

c. Inter-group contact hypothesis - Immigration inflows outside of natives' municipalities of residence are likely to generate stronger far-right vote response, compared to immigration inflows in the municipality where natives reside.

3. Data

3.1 Data sources

I define immigrants as individuals born in a foreign country. This includes immigrants of foreign nationality, and immigrants that naturalize (i.e. obtain Spanish nationality). Because immigrants that naturalize may still be perceived as immigrants by natives, this allows me to measure the perceived immigrant share in a municipality. Spanish Law requires every person who lives in Spain to register in the census of the municipality where he or she regularly resides (Padrón Municipal). Registering gives access to health benefits and proof for residence for later regularization - providing strong incentives for all immigrants to enrol in municipal population registers (Bertoli & Fernández-Huertas Moraga, 2013). Based on municipal population registers, the Spanish Statistics Institute (INE) provides the number of individuals born in a foreign country, per four regions of origin (Africa, America, Asia and Europe). I have compiled yearly historical series of foreign-born population, from 2004 to 2019, and combined the latter with population data, to calculate population shares of foreign-born individuals of each region, for each municipality. Separately, for the IV of my instrument, I compiled data on municipalities' number of foreign-born individuals 2003.

Regarding political voting, as migration policies are defined at the national level, I focus on general elections. I obtained voting data for political parties from the Spanish Ministry of Home Affairs (Ministerio del Interior). For each municipality, my data records the number of valid votes, per party, and the total number of votes (valid and invalid). I have calculated vote shares by dividing the number of votes cast for each party by the total number of votes (valid and invalid votes). To evaluate immigration effects on VOX support, VOX vote shares are calculated as per the 2019 November election, when its support was highest (15.08% of votes). Separately, for my robustness analysis, I have also aggregated vote shares for all right-wing parties, for all election years, going from 2004 to 2019. Right-wing parties include mainstream right-wing party PP, centre-right challenger *Ciudadanos*, since 2008 and far-right VOX, since 2015.

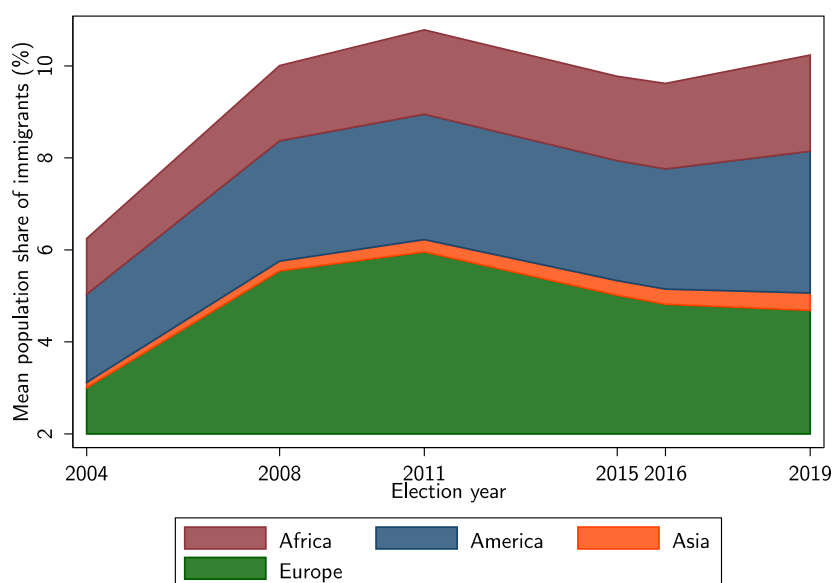
In terms of economic and demographic controls, first, unemployment is proxied on the basis of data from the State Public Employment Service (SEPE). This data reports number of individuals who register as being unemployed, for the purposes of receiving unemployment benefits. To construct my unemployment proxy, I have divided the latter by the number of individuals at working age, as reported by the Spanish Statistics Institute (INE).¹ For the initial year in my analysis (2004), I have used the earliest available year in the series (2006). Second, data of age structure of population is sourced from the Spanish Statistics Institute (INE). I calculate the shares of population less than 25, prime-age (25-64) and older than 65 - over total population. Third, to derive urbanization dummies, data on total population is sourced from the Spanish Statistics Institute (INE).

¹ Monthly data on individuals registered as unemployed has been retrieved, for each municipality, and it has been averaged on a yearly basis. Relative to the actual unemployment rate, notice that my unemployment proxy has a different denominator: working age population, rather than active population. The nominator varies, too: individuals that actively register as being unemployed (at employment offices), rather than individuals who report being unemployed for a given period of time (in employment surveys).

3.2 Descriptive statistics

Based on data availability, my sample covers 2,747 municipalities, out of 8,131 in Spain. Municipalities in the sample have mean population of around 15,000 in 2019. Figure 1 below shows the trend for the mean population share of immigrants, from 2004 to 2019, by election year. In my sample, the mean population share of immigrants grew by 3.99pp. This meant substantial growth, as the mean value increased from 6.71 to 10.60%. By region of origin, the largest increases came from Europe (1.68pp), America (1.16pp) and Africa (0.88pp). Further, Figure 1 shows instability in immigration flows, in the aftermath of the Great Recession. It shows that mean population share of immigrants expanded notably from 2004 to 2011; it then decreased from 2011 to 2016; to later recoup.

Figure 1. Population share of immigrants (2019-2004) by election year and immigrant group

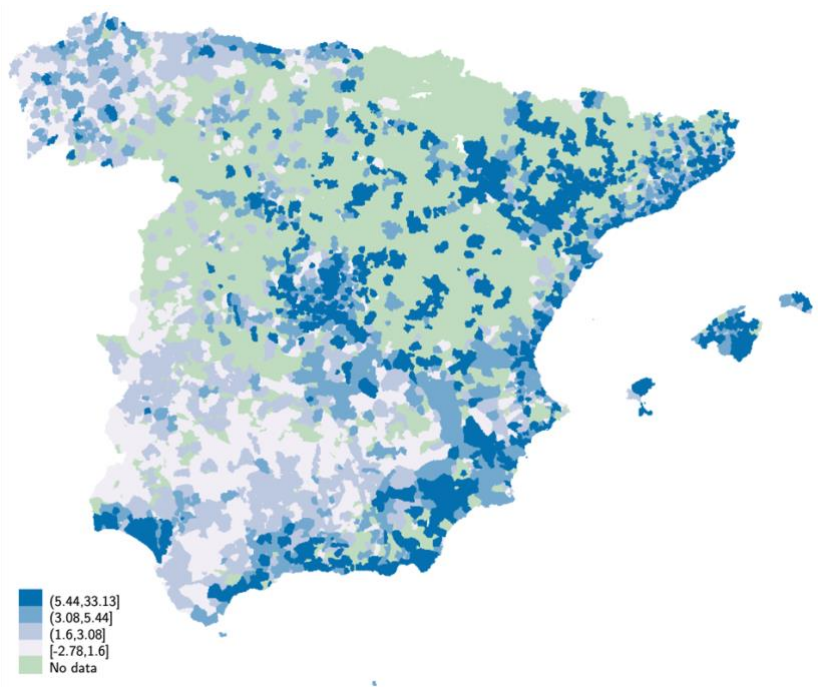


Note. The mean population share of immigrants (defined as foreign-born population) is calculated, in each election year, across the municipalities included in the sample.

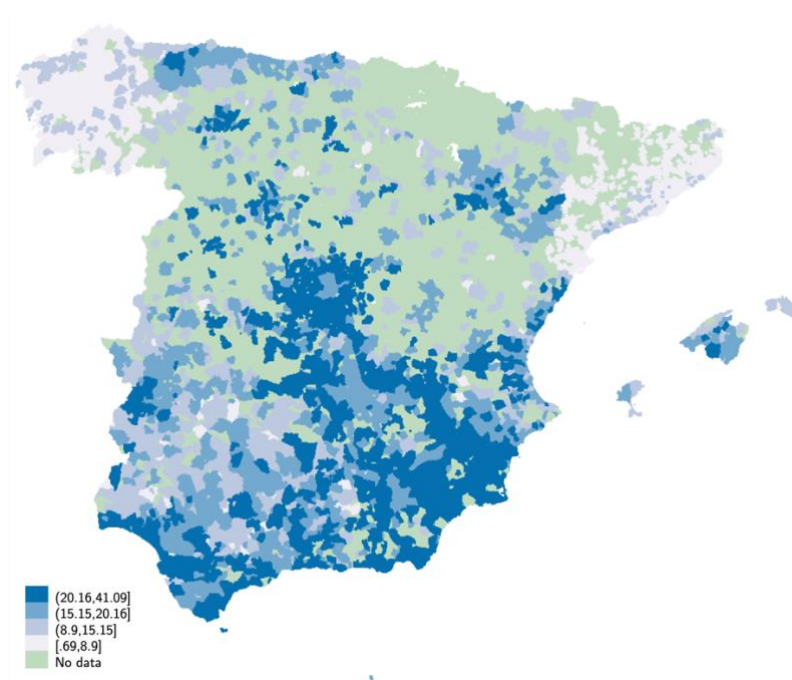
The dataset for primary analysis (*VOX model*) cross-sectionally relates this 2019-2004 immigration change, to the VOX vote share in 2019. To better understand cross-sectional variation, Figure 2 shows municipality-level heat maps for immigration change and VOX vote, by quartile (with means of 3.99 pp and 15.01%, respectively). Panel A shows that immigrant inflows have been high in Madrid, north-eastern regions and municipalities along the Mediterranean Sea. More interior municipalities in Southern Spain, and in north-western areas (i.e. Andalucía and Galicia), have recorded more modest change in the population share of immigrants. Panel B contains the corresponding heat map for VOX vote shares in 2019. It shows that municipalities belonging to the top quartile of the VOX vote share mostly are in centre-to-south regions. Notably, municipalities in Catalonia and Galicia feature notably low VOX vote shares - mostly belonging to the first quartile of VOX vote shares.

Figure 2. Heat maps for variation in the key variables of interest

Panel A. Δ Population share of total immigrants (2019-2004, in pp)



Panel B. VOX vote share (2019 level, as percentage)



Note. The change in the population share of immigrants is expressed in pp terms (Panel A), while the VOX vote share is expressed as percentage (Panel B). Municipalities are divided into quartiles. Municipalities from the Canary Islands have been excluded in the maps for visual clarity. Maps show territorial boundaries of municipalities as per shapefiles of the Spanish Statistics Office (INE) in 2022.

Table 1 provides full descriptive statistics for all the variables in the cross-sectional dataset. This includes immigration, outcome and control variables, over the 2004 to 2019 period.

Table 1. Descriptive statistics (*VOX model*)

| Variable | Unit | Obs. | Mean | Std. Dev. | Min. | Max. |
|---------------------------------------|------|-------|-------|-----------|--------|--------|
| <i>Panel A: Immigration variables</i> | | | | | | |
| (Initial) Share of total imm. | % | 2,747 | 6.24 | 6.00 | 0.00 | 60.30 |
| Δ Share of total imm. | pp. | 2,747 | 3.99 | 3.55 | -2.78 | 33.14 |
| (Initial) Share of Africa imm. | % | 2,747 | 1.21 | 1.83 | 0.00 | 19.51 |
| Δ Share of Africa imm. | pp. | 2,747 | 0.88 | 1.54 | -1.60 | 15.52 |
| (Initial) Share of America imm. | % | 2,747 | 1.92 | 2.07 | 0.00 | 24.42 |
| Δ Share of America imm. | pp. | 2,747 | 1.16 | 1.31 | -1.84 | 11.25 |
| (Initial) Share of Europe imm. | % | 2,747 | 3.00 | 4.07 | 0.00 | 56.66 |
| Δ Share of Europe imm. | pp. | 2,747 | 1.68 | 2.43 | -2.93 | 33.48 |
| (Initial) Share of Asia imm. | % | 2,747 | 0.12 | 0.26 | 0.00 | 3.55 |
| Δ Share of Asia imm. | pp. | 2,747 | 0.26 | 0.46 | -0.24 | 7.44 |
| <i>Panel B: Outcome variable</i> | | | | | | |
| VOX vote share | % | 2,747 | 15.04 | 7.45 | 0.70 | 41.09 |
| <i>Panel C: Control Variables</i> | | | | | | |
| (Initial) Unemployment | % | 2,747 | 6.88 | 3.07 | 1.00 | 25.11 |
| Δ Unemployment | pp. | 2,747 | 3.38 | 2.74 | -7.37 | 14.80 |
| (Initial) Pop. Over 65 share | % | 2,747 | 27.54 | 7.18 | 2.87 | 44.91 |
| Δ Pop. Over 65 share | pp. | 2,747 | 0.40 | 12.00 | -59.53 | 56.91 |
| (Initial) Pop. Under 25 share | % | 2,747 | 18.99 | 5.91 | 7.66 | 38.99 |
| Δ Pop. Under 25 share | pp. | 2,747 | -1.06 | 8.81 | -35.44 | 34.06 |
| (Initial) Population | th. | 2,747 | 13.93 | 76.37 | 235 | 3,100 |
| Δ Population | th. | 2,747 | 1.34 | 5.25 | -23.30 | 166.29 |
| (Initial) Right-wing vote share | % | 2,747 | 37.54 | 15.68 | 3.36 | 88.26 |

Note. Variables labelled as (Initial) refer to 2004 levels. Δ Refers to 2019-2004 change.

Separately, the dataset for my robustness analysis is a panel, which captures changes in immigration and in aggregate right-wing vote support, between election years (*FD model*). Considering changes between election years (2004, 2008, 2011, 2015, 2016 and 2019), the mean population share of immigrants grew by 0.80 pp, with mean aggregate right-wing vote share too increasing by 1.09. Full descriptive statistics for this dataset are available in [Appendix 1.1](#).

4. Identification strategy

4.1 In-municipality immigration

Naïve model. The standard model in literature on impacts of immigration on political voting consists of first-differenced panel data (Edo et al., 2019; Gálvez-Iniesta & Groizard, 2021; Halla et al., 2017; Mendez & Cutillas, 2014) (*FD model*). This exploits within-municipality changes in immigration, between election years, and relates them to changes in voting outcomes. The advantage of this framework, indeed, is that, by exploiting differences, it eliminates any bias introduced by municipality-specific fixed effects, that might be correlated with immigrant presence and political voting. Formally, letting i be a municipality, and t an election year:

$$\Delta \text{Votes}_{it} = \alpha_1 \Delta m_{it} + \Delta C_{it}' \psi + \omega_t + \rho + \Delta \varepsilon_{it} \quad (1)$$

where the difference operator indicates changes between election years. ΔVotes_{it} is the change in the vote share for a far-right party. ΔC_{it} is a battery of changes in municipality controls (typically, unemployment, demographic structure and education). Δm_{it} is the change in the population share of immigrants. Year FE ω_t are added to control for aggregate shocks that vary from election to election, and affect voting patterns in all municipalities. Regional FE ρ take away regional trends in voting outcomes, due to unobserved factors. $\Delta \varepsilon_{it}$ is the error term.

The strict application of this panel data model to the VOX case would involve a panel considering elections from 2015 (the first year it incurred in elections, with negligible support) to 2019. However, constraining to this period would rule out immigration effects on native attitudes, which might be latent due to broader immigration inflows, and that could not materialize into VOX vote due to its inexistence - possibly given political supply constraints (see [Section 2.1](#)).² I hence turn to a cross-sectional analysis, where I will observe VOX vote shares in 2019, and let the latter be impacted by a 2019-2004 immigration shock treatment (*VOX model*). Fundamentally, the hypothesis that I test is whether VOX support benefited from latent anti-immigration attitudes created by 2004-2019 immigration flows. To follow the notation used so far, let me now define a two-period model where $t \in \{2004, 2019\}$:

$$\text{Votes}_{it}^{\text{VOX}} = \beta_0 + \beta_1 \Delta m_{it} + \beta_2 \text{Votes}_{it-1}^{\text{RWing}} + C_{it-1}' \varphi + \Delta C_{it}' \chi \delta + u_{it} \quad (2)$$

or, resorting to a more explicit notation for time:

$$\text{Votes}_{i19}^{\text{VOX}} = \beta_0 + \beta_1 \Delta m_{i19-04} + \beta_2 \text{Votes}_{i04}^{\text{RWing}} + C_{i04}' \varphi + \Delta C_{i19-04}' \chi + \delta + u_{it} \quad (3)$$

$\text{Votes}_{i19}^{\text{VOX}}$ is the VOX vote share in 2019. Δm_{i19-04} is the change in the population share of immigrants living in municipality from 2004 to 2019. The coefficient of interest β_1 is the pp increase in VOX vote share, associated to an increase in the population share of immigrants of 1 pp.

² Moreover, doing so would involve a short time span, over which there is limited change in immigration.

$Votes_{i04}^{RightWing}$ is municipality's i vote share to right-wing parties in 2004. This is measured as votes to mainstream right-wing party PP (at that time, the dominant right-wing party). I hence exploit variation in VOX vote among municipalities that had similar propensity to vote right-wing parties, prior to the immigration treatment. A positive effect of this variable could be interpreted as part of far-right VOX vote coming from previous vote to other right-wing parties, and/or greater overall propensity to support far-right in municipalities that are more right-wing, due to unobserved factors.

C_{i04}^1 ensures that municipalities are comparable in terms of other initial observed characteristics, prior to the immigration shock. First, I account for natives' initial economic conditions, proxying the latter through overall registered unemployment. Second, I control for initial demographic characteristics, with shares of population by age group - less than 25, prime-age 25-64, and older than 65. Third, I control for initial urbanization, through dummy variables for municipalities with more than 10,000 and 50,000 inhabitants. Separately, I also add a vector ΔC_{i19-04}^1 , which tracks changes in the three set of observed characteristics discussed, as these could be determinants of both immigration flows and political voting - mirroring Equation 1.³

Finally, I add province FE δ , to eliminate unobserved fixed regional heterogeneity (48 provinces in my sample). These are likely important in my setting, given propensity to vote for VOX varies substantially across provinces of Spain, for fixed reasons. This includes the fact that provinces in regions with greater cultural identity and autonomy in regional institutions (e.g. Catalonia, Galicia, in my sample) are expected to be less attracted by VOX's agenda on national unity and centralized government.

As already mentioned, the advantage of the *FD model* (Equation 1), compared to the *VOX model* (Equation 3), is that the former eliminates municipality fixed effects. Hence, for robustness, I also estimate Equation 1, using changes in the aggregate vote share of right-wing parties and in immigration, between all election years from 2004 to 2019. Equation 1 controls for changes in the same control variables discussed just above. Further, Equation 1's regional FE ρ consists of province and urban dummies (the latter being measured in the beginning of the period). These take away trends in political voting that may be specific to provinces and urban municipalities.

IV Approach. OLS might not provide the causal impact of immigration on votes, through native attitudes, because immigrant inflows are not randomly allocated across municipalities (see Jaeger et al. 2018 for a general discussion). First, inflows might go to economically successful municipalities and/or those that suffer less negative economic shocks (Peeters & Chasco, 2016; Viñuela et al., 2019). If better perceptions of the economy are less conducive to radical right voting (Sipma & Berning, 2021), one would expect downward bias. Also, immigrant inflows might actively avoid settling in municipalities with strong anti-immigration attitudes or where this will grow less (Bracco et al., 2017) - too, creating downward bias.

³ I notice that, in particular, changes in unemployment might be a channel by which immigration impacts on political voting - i.e., if immigrants have an adverse impact on labour market outcomes of natives. For this reason, it is worth evaluating how my immigration impact estimates evolve as I introduce the components of the model. Nevertheless, yet, related literature controls for changes in local unemployment, and presumes that the latter is exogenous to immigration (e.g. Edo et al. 2019).

My identification relies on the shift-share IV approach of Altonji & Card (1991) and Card (2001), later employed in literature studying far-right support - e.g. Barone et al. (2016); Edo et al. (2019); Gálvez-iniesta & Groizard (2021); Halla et al. (2017); Mayda et al. (2022); Mendez & Cutillas (2014); Moriconi et al. (2018); Otto & Steinhardt (2014). Here, immigration across regions is instrumented according to the distribution of immigrants in the past. Indeed, new immigrants' settlement choices might be influenced by the presence of earlier immigrants of similar cultural and linguistic background, due to network effects (Gross & Schmitt, 2003). For example, immigrants who settled in the past may provide new immigrants, of the same cultural and linguistic background, with information on labour and housing markets. This induces immigrants to settle in areas with already high immigrant concentrations of the same background (Dustmann et al. 2005). In my analysis, I use the 2003 distribution of immigrants from 4 regional groups (Africa, America, Asia and Europe), to predict immigrant flows. I specifically follow the application of Edo et al. (2019). *VOX model's* instrument for Δm_{i2019} , with $t \in 2004, 2019$, is defined as follows:

$$\widehat{\Delta m}_{i19-04} = \left[\frac{\widehat{imm}_{i19}}{\widehat{pop}_{i19}} \right] - \left[\frac{\widehat{imm}_{i04}}{\widehat{pop}_{i04}} \right] \quad (4)$$

where \widehat{imm}_{it} and \widehat{pop}_{it} are the predicted number of immigrants and total population, respectively, in municipality i and election year t . The predicted number of immigrants is obtained as follows:

$$\widehat{imm}_{it} = \sum_n^n imm_t^n \times \varphi_{it0}^n \quad (5)$$

where imm_t^n is Spanish-wide total stock of immigrants of region origin n in election year t , and φ_{it0}^n is the proportion of immigrants of region n living in municipality i in 2003 (or $t0$). Separately, total predicted population \widehat{pop}_{it} includes immigrants and Spanish-born individuals, and is also obtained using the same shift-share principle. From Equation 4, I end up with predicted change in the population share of immigrants $\widehat{\Delta m}_{i19-04}$, which is used as an instrument for actual change Δm_{i19-04} , in a 2SLS setting. A slight modification to Equation 4, restricting the numerators to immigrants of a specific region of origin, yields predicted change in the population share of immigrants from that region of origin. This can then be used in separate 2SLS regressions, as an instrument for immigration change from that region. Finally, a similar process applies to the robustness first-differenced *FD model*. Here, predicted and actual change in immigration is calculated for periods in-between all election years, going from 2004 to 2019 ($\widehat{\Delta m}_{it}$ and Δm_{it} , respectively).

The exclusion restriction slightly varies in *VOX* and *FD models*. In the *VOX model*, the exclusion restriction is that the 2003 spatial distribution cannot correlate with current municipality characteristics that drive *VOX* voting in 2019 (ER.1). In the robustness *FD model*, the 2003 instrument cannot correlate with shocks that drive *changes* in political voting in elections periods from 2004 to 2019 (ER.2). Note that this exclusion restriction applies after controlling for the set of factors that I consider in my regressions.

First, municipality economic characteristics in 2003 may not persist over time, given economic shocks that will follow, including the Great Recession. This might limit the correlation of my instrument with current municipality characteristics (ER.1). Second, early in the 2000s, the employment distribution of the immigrant stock consisted of services (hospitality, domestic

work, retail) and agriculture - to a lesser extent, construction (Alberto Valore & Coca, 2014; C. Carrasco, 1999). This employment distribution does not seem to concentrate on characteristics of strong resilience or vulnerability (e.g. industry and construction, respectively, see Ubago et al. 2019). This should limit the correlation of the 2003 distribution with the economic shocks themselves (ER2). Third, year 2003 is an early time in a great immigration wave. Prior arrivals, in terms of inflow sizes and speed, had been less disruptive. This may have meant anti-immigration attitudes being relatively less salient. The 2003 spatial distribution, therefore, may not reflect avoidance behaviour of immigrants in municipalities based on expected anti-immigration attitudes (ER.1) or their change (ER2).⁴

The exclusion restriction is more likely to be met when there are large time gaps, in between the base year of the instrument, and the time of the voting outcomes (Jaeger et al., 2018). This makes it more likely for immigrants to settle based on historical presence of network effects, rather than on municipality characteristics, that correlate to both immigrant inflows and political voting. In the *VOX model*, I have a certain gap, from the instrument base year, 2003, to political voting in 2019. In the robustness *FD model*, meanwhile, there is a shorter time gap in between 2003 and the 2004-2019 period, where I observe voting. Overall, the lack of a *large* time gap might deserve certain caution, in terms of exogeneity.⁵

Interpretation. My baseline estimates for the immigration variable must be interpreted as lower bounds for the effects of immigration on natives' attitudes. This is because my definition of immigrants in the treatment is that of foreign-born individuals. This includes immigrants of foreign nationality, and immigrants that naturalize (i.e. obtain Spanish nationality). Naturalized immigrants can vote. While Spanish literature suggests they typically have low turnout, they are less significantly likely to vote for anti-immigration and right-wing parties (Maria et al., 2016; Muñoz, 2011). Put in other words, my immigrant treatment exerts (i) a *direct effect* on vote shares, through naturalized immigrants voting; and (ii) an *indirect effect* on vote shares, by shaping the attitudes of natives. Hence, a positive impact of immigration on far-right vote shares should be interpreted as *indirect effects* dominating *direct effects*. Informatively, for immigrants to naturalize, regulations require proof of 10 years of minimum time of residence, generally. This requirement is only of 2 years for Latin American countries - hence I would expect this immigrant group to naturalize to a greater extent.

⁴ E.g., Bracco et al. (2017) highlights that the avoidance behaviour of immigrants, towards far-right municipalities, operates in Italian municipalities - for those that elect a Lega Nord mayor. Electing a far-right mayor would arguably generate some signaling - e.g. municipal regulations, public services, housing or schooling services - which would discourage the arrival of immigrants. My argument is that the limited salience of anti-immigration rhetoric, and the lack of anti-immigration parties in 2003, makes this channel less likely.

⁵ For example, Edo et al. (2019) exploit a 20-year gap.

4.2 Neighbouring immigration

As discussed in [Section 2.2](#), Evans & Ivaldi (2021)'s work might possibly suggest that voting in my municipalities could be influenced by immigration presence in adjacent locations, within travelling distance, instead of immigration *inside* the municipality. The same range of economic and non-economic perceived threats might drive opposition to immigration in adjacent areas, while inter-group contact should be weaker, compared to in-municipality flows. Using French survey individual data, they find that individuals living in areas of high immigrant presence do not vote more for FN. Meanwhile, individuals do vote more for FN, when exposed to high immigrant presence adjacent to where they live (in 5 to 20 km distances). This paper uses individual survey data, and measures distances from individuals to high immigration sub-communes, adjacent to them.

In my analysis, my units are municipalities, rather than individuals. The nearest adjacent presence of immigration, for a municipality, is that of its directly neighbour municipality (i.e. municipalities that are geographically contiguous). Further away, one could expand to direct neighbours, of its direct neighbours. In my sample, the average distance from municipalities' centeroid to that of its direct neighbours is of 10km. Therefore, I create concentric circles with 10km increments. I rely on three non-mutually-exclusive concentric circles: 0-10km; 0-20km; and 0-30km. In each concentric circle, I calculate the immigration shocks in the municipalities that fall within it. Since Spanish geography is heterogenous and my sample is geographically imbalanced, in each concentric circle, I weight immigration shocks with weights proportional to the squared inverse distance between municipalities. I end up with three distance-weighted measures for neighbouring immigration: $\overline{\Delta m_{i19-04}}^{10km}$, $\overline{\Delta m_{i19-04}}^{20km}$, $\overline{\Delta m_{i19-04}}^{30km}$ (with variables for immigration shocks between election years). Further details and descriptive statistics for the resulting variables are available in [Appendix 3.1](#).

My empirical strategy consists of estimating *VOX* and *FD models*, adding one measure of neighbouring immigration, along the in-municipality immigration shock. In terms of comparing effects of in-municipality and neighbouring immigration, note that the in-municipality measure generates both *direct* and *indirect effects* on voting outcomes, being a lower bound for impact on anti-immigrant attitudes of natives. Meanwhile, immigration in neighbouring municipalities only generates *indirect effects*, being a "true" impact on anti-immigrant attitudes of natives.

Finally, notice that my empirical strategy deviates from Evans & Ivaldi (2021), in two ways. First, I measure the effect of immigration *shocks*, rather than the effect of immigration levels. I believe this is more appropriate, as Kaufmann (2017) shows that anti-immigration response are driven by immigration change, rather than by established levels of immigration. Second, Evans & Ivaldi (2021) capture the effect of distances to high immigration areas. My measurement instead captures average responses to all neighbouring immigration. The latter can be thought a broader contextual measure, providing average effects to adjacent immigration. As such, my measurement approach resembles literature that has measured responses to distance-weighted local shocks (e.g. migration responses to labour demand, in Borusyak et al., 2022; or to import competition, in Greenland et al., 2019).

5. Exploring the IV

A first way to evaluate the working of the IV is to assess the geographic distribution of immigrants, over the years. As already stated, the principle of the IV is that the distribution of immigrants across municipalities tends to mimic that of past settlements. The instrument hence requires a positive spatial correlation in immigrant settlements, over the years. [Appendix 1.2](#) provides the geographic distribution of immigrants, normalized by municipalities' population - in 2003 (the base year of the IV), and for the later election years considered in my primary analysis (2004 and 2019, in the *VOX model*). Visual inspection indeed suggests that the share of immigrants in later years is higher in municipalities with a high immigrant share already back in 2003.

A second way to inspect the inner working of the instrument is to directly look at predicted and actual change in the population share of immigrants. First, Figure 3 plots predicted and actual changes in the population share of immigrants, for the entire 2019-2004 change - that is, $\widehat{\Delta m_{i2019}}$ and Δm_{i2019} (*VOX model*). To construct these charts, I divide the horizontal axis into 50 equal-sized bins, and plot the mean predicted change in population share of immigrants, against the mean actual change in population share of immigrants. Second, Figure 4 presents the same analysis, pooling all predicted and actual changes in-between election years, in the period 2004 to 2019 - that is, $\widehat{\Delta m_{it}}$ and Δm_{it} (*FD model*). Here, year fixed effects are controlled for. Panels A suggests that predicted and actual values for aggregate immigration flows are positively correlated. Panels B show this is also the case for immigrant flows per individual region of origin. Yet, note that these are raw, unconditional, associations - subject to holding after incorporating all factors incorporated in my models.⁶

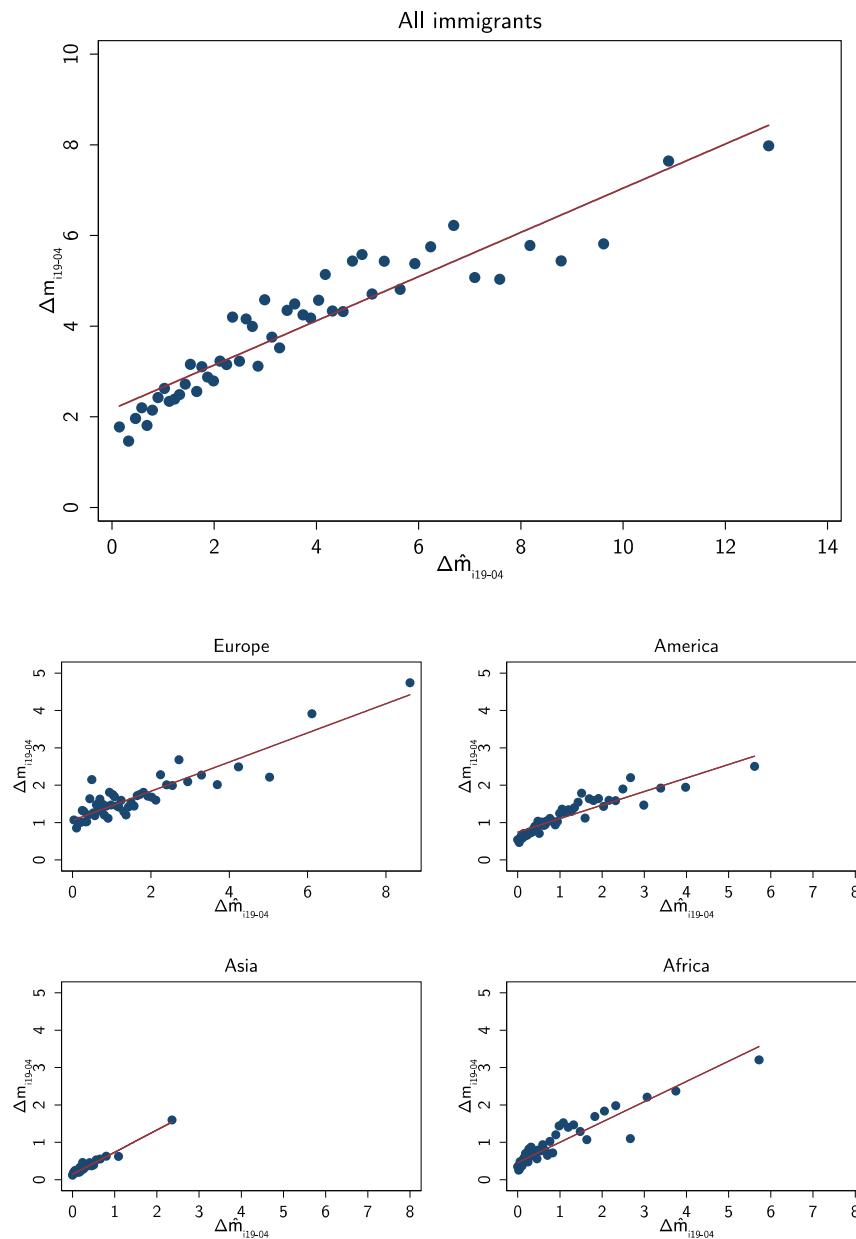
At this point, it is worth highlighting two related distinctions between the instruments for Δm_{i19-04} and Δm_{it} - which will be useful to later interpret First Stage regressions. First, (i) the association between actual and predicted changes, in-between election years, has varying strength, depending on the period. We are left with an average relationship, across periods. Second, (ii) predicted immigration change $\widehat{\Delta m_{it}}$ can have positive or negative values - as actual immigration change Δm_{it} does. This is because in-between election year periods have increases and decreases in the total stock of immigrants.⁷ Meanwhile, predicted immigration change $\widehat{\Delta m_{i19-04}}$ can only have positive values. This is due to the Spain-wide stock of immigrants only growing, from 2004 to 2019. While actual immigration flows Δm_{i19-04} are mostly positive, some municipalities have negative immigration change.⁸ Given my sample, these two factors empirically contribute to a less strong association between predicted and actual flows for the 2019-2004 change. This will later be seen more formally, in First Stage regressions.

⁶ I.e. the relationship between predicted and actual immigration change is subject to controlling for all factors accounted for in the two models discussed. These could partly explain the correlations depicted here, other than through network effects.

⁷ Notice that, in Equation 4, the total stock of Spain-born individuals (which enters into the prediction for population, in the denominator) does not have a relevant empirical role, as it is broadly maintained stagnant over the years.

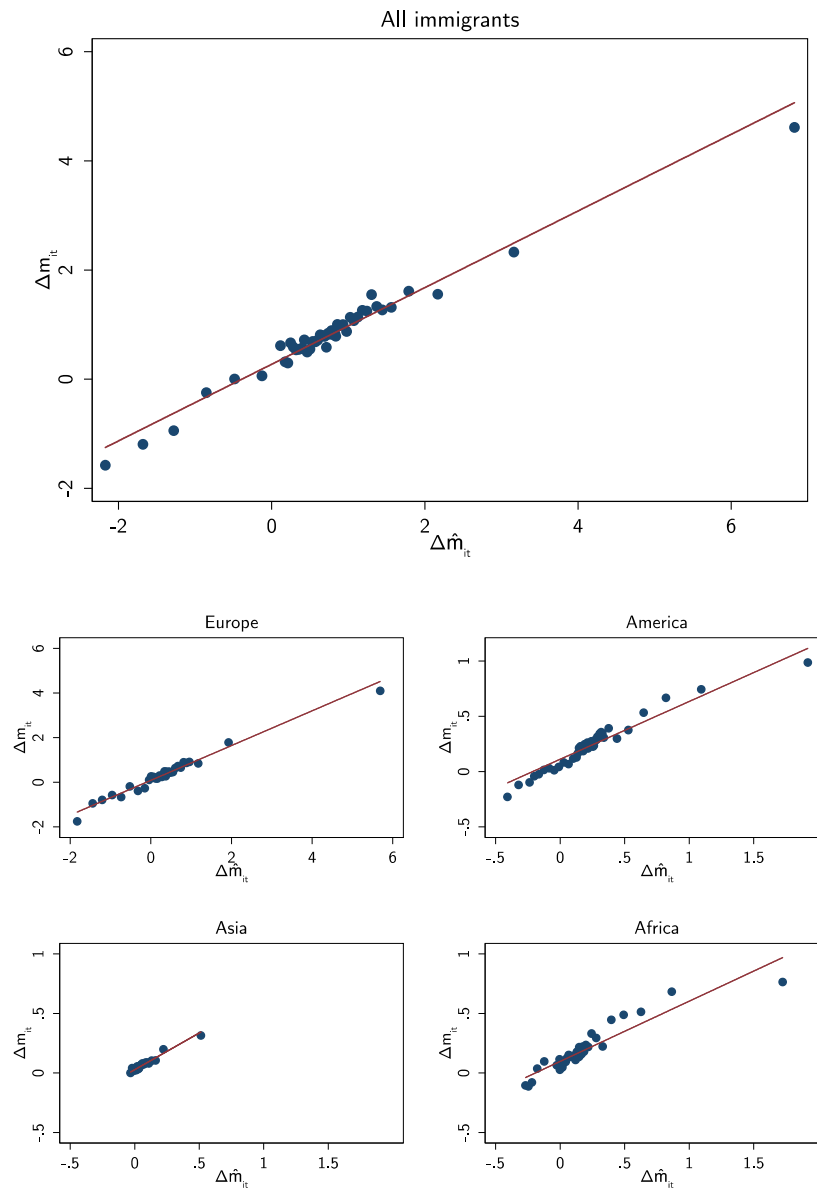
⁸ Considering the 2019-2004 change, negative aggregate immigration change is only present in the bottom 1% of the sample. Yet, this is relatively more salient when looking at specific regions of origin. The bottom 10% of American and African inflows consist of negative values. In Europe and Asia, the bottom 5% of inflows are negative change.

Figure 3. Actual and predicted change in the population share of immigrants in 2019-2004 (in pp) (*VOX model*)



Note. The horizontal axis is the predicted change in population share of immigrants, for the entire 2019-2004 change. The vertical axis represents the actual change in population share of immigrants, for the same period. To construct these plots, I divide the horizontal axis into 50 equal-sized bins and plot the mean predicted change in population share of immigrants against the mean actual change in population share of immigrants (using Stata's *binscatter* command). The red line represents a linear fit line.

Figure 4. Actual and predicted change in the population share of immigrants, in periods in-between election years (in pp.) (*FD model*)



Note. The horizontal axis is the predicted change in population share of immigrants, for periods in-between election years, from 2004 to 2019. The vertical axis represents the actual change in population share of immigrants, for the same periods. To construct these plots, I divide the horizontal axis into 50 equal-sized bins and plot the mean predicted change in population share of immigrants against the mean actual change in population share of immigrants. Year fixed effects are controlled for (using Stata's *binscatter* command "control" option). The red line represents a linear fit line.

6. In-municipality immigration

6.1 Average effects

a. Baseline VOX Model. Table 2 presents output for the *VOX model*. Each column cumulatively adds elements of the full model. Column 1 provides the unconditional immigration effect; Column 2 introduces province FE; Column 3 adds initial right-wing support; Column 4 controls for the rest of initial observed characteristics; and Column 5 additionally controls for their change. The latter constitutes the full model specification.

OLS results (Panel A) show that the immigration treatment is positively associated with VOX vote across all specifications. The estimate increases after adding province FE (Column 2). This means unobserved heterogeneity at the province level, which might influence both immigrant flows and VOX vote. The immigration estimate reduces after controlling for initial right-wing vote share (Column 3). As the latter has the positive expected sign, the reduction in the immigration effect implies that immigration flows and initial right-wing vote are positively correlated. Finally, the OLS estimate is stable after controlling for initial municipality characteristics (Column 4), while slightly reduces after accounting for their change (Column 5), possibly denoting correlation of immigration flows with changes in observed characteristics. Control variables show that municipalities with higher unemployment and relatively less old population, in initial levels and in change, are associated with higher VOX vote. The final model's OLS estimate for immigration stands at 0.221, statistically significant at the 1% level.

IV First Stage estimates (Panel C) show that the instrument has the expected positive sign, and is statistically significant at the 1% level, across all specifications. After accounting for province-level heterogeneity and municipality characteristics, the coefficient stands at 0.342 (Column 5). The F statistic is above the critical value of 10, meaning that my instrument does not suffer from weak instrumentalization. The First Stage is to be interpreted as 1 pp increase in the predicted population share of immigrants, leading to an increase in the actual population share of immigrants of 0.342 pp.⁹

IV Second Stage results (Panel B) show that the immigration treatment is positive and statistically significant at the 1% level across all specifications. The immigration effect increases notably once I introduce province FE (Columns 2). It reduces when accounting for right-wing vote support prior to the immigration treatment (Column 3), and when adding the remaining initial municipality characteristics (Column 4). Such variation in immigration estimates suggests correlation of instrumented immigration flows with province-level heterogeneity and initial municipality characteristics. Indeed, this suggests I should control for all these factors, as their omission might bias IV estimates. Finally, the immigration estimate seems broadly stable when I add changes in municipality characteristics (Column 5). In the full model, a 1 pp increase in the population share of immigrants is associated with a 0.281 pp increase in the VOX vote share.

⁹ Alternatively, out of 100 immigrants that settle in my sample, 34.2 of them choose their municipality locations due to network effects.

Table 2. Baseline estimates in the *VOX model*

| | (1) | (2) | (3) | (4) | (5) |
|-------------------------------|-------------------|--------------------|-------------------|--------------------|--------------------|
| Panel A: OLS | | | | | |
| Δm | .193*** (.047) | .303*** (.030) | .257*** (.027) | .256*** (.025) | .221*** (.028) |
| (Initial) RWing vote share | | | .228*** (.011) | .244*** (.011) | .246*** (.011) |
| (Initial) Unemployment | | | | .034 (.035) | .084** (.036) |
| (Initial) Pop. Under 25 share | | | | -.044 (.042) | -.047 (.057) |
| (Initial) Pop. Over 65 share | | | | -.322*** (.026) | -.358*** (.037) |
| Δ Unemployment | | | | | .197*** (.036) |
| Δ Pop. Under 25 share | | | | | .081 (.050) |
| Δ Pop. Over 65 share | | | | | -.212*** (.041) |
| Province FE | No | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | No | No | No | Yes | Yes |
| Δ Urban | No | No | No | No | Yes |
| Obs. | 2,747 | 2,747 | 2,747 | 2,747 | 2,747 |
| Adj. R2 | .008 | .692 | .740 | .781 | .792 |
| Panel B: IV Second Stage | | | | | |
| Δm | .297*** (.103) | 1.063*** (.137) | .777*** (.118) | .289*** (.111) | .281*** (.107) |
| (Initial) RWing vote share | | | .210*** (.013) | .243*** (.011) | .244*** (.011) |
| (Initial) Unemployment | | | | .039 (.037) | .091** (.038) |
| (Initial) Pop. Under 25 share | | | | -.042 (.042) | -.040 (.057) |
| (Initial) Pop. Over 65 share | | | | -.322*** (.026) | -.355*** (.037) |
| Δ Unemployment | | | | | .196*** (.036) |
| Δ Pop. Under 25 share | | | | | .083* (.050) |
| Δ Pop. Over 65 share | | | | | -.202*** (.044) |
| Panel C: IV First Stage | | | | | |
| $\Delta \hat{m}$ | .487*** (.027) | .371*** (.039) | .360*** (.040) | .336*** (.042) | .342*** (.041) |
| K.-P. rk Wald F statistic | 313.46 | 89.30 | 81.83 | 64.18 | 68.70 |

Notes. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% level

b. Baseline FD Model. Table 3 presents output of the *FD model*, where I consider aggregate right-wing vote share and immigration change, between election years. Column 1 provides the unconditional immigration effect. Column 2 adds year FE, to account for election year-specific shocks across municipalities. Column 3 introduces the battery of changes in controls. Finally, for further reassurance, Column 4 includes province FE, to account for trends in political voting specific to provinces, and Column 5 adds urban FE, with the same rationale.

Once year FE are added, OLS estimates (Panel A) tend to slightly decrease as I add changes to controls (Column 3), province FE (Column 4) and urban FE (Column 5). In the final model, the OLS estimate stands at 0.258 (Column 5). As for control variables, results are somewhat contrasting with *VOX model* output. In Columns 4-5, unemployment change is not associated with right-wing vote, and right-wing support is higher in municipalities with increasingly young and old populations - on average.

Turning to the IV First Stage (Panel C), the final model's F statistic is above 10, meaning the model does not suffer from weak instrumentalization - and with a first-stage instrument coefficient higher than the one I obtain in the *VOX model*. IV Second Stage results (Panel C) show that, once year FE are added, the immigration estimate too decreases slightly after accounting for observed municipality characteristics (Column 3). This means that the omission of changes in control variables might bias IV estimates. Yet, the IV estimates remain relatively stable when adding province and urban FE (Columns 4-5). In the final IV specification (Column 5), a 1 pp increase in the population share of immigrants results in an increase in the aggregate right-wing vote share of 0.360 pp.

Finally, I notice that, in both *VOX* and *FD models*, I obtain higher IV-point estimates, compared to OLS results. This would in principle suggest that the OLS average effect of immigration is *downwards* biased. This is consistent with results seen elsewhere in related literature, which uses past settlements instrumentalization, to investigate impacts on political voting outcomes (Barone et al., 2016; Edo et al., 2019; Gálvez-Iniesta & Groizard, 2021; Halla et al., 2017). This empirical observation is broadly interpreted as immigration flows settling in locations where electoral support for right-wing parties will tend to reduce, on average - and for reasons other than the immigration shock.

Table 3. Baseline estimates in the *FD model*

| | (1) | (2) | (3) | (4) | (5) |
|------------------------------|-------------------|-------------------|--------------------|-------------------|-------------------|
| Panel A: OLS | | | | | |
| Δm | .642*** (.021) | .359*** (.017) | .313*** (.018) | .264*** (.021) | .258*** (.021) |
| Δ Unemployment | | | .088*** (.021) | .032 (.021) | .031 (.021) |
| Δ Pop. Under 25 share | | | -.313*** (.038) | .090** (.042) | .092** (.042) |
| Δ Pop. Over 65 share | | | -.063 (.041) | .203*** (.043) | .188*** (.043) |
| Year FE | No | Yes | Yes | Yes | Yes |
| Province FE | No | No | No | Yes | Yes |
| (Initial) Urban FE | No | No | No | No | Yes |
| Δ Urban dummies | No | No | Yes | Yes | Yes |
| Obs. | 13,735 | 13,735 | 13,735 | 13,735 | 13,735 |
| Adj. R2 | .067 | .572 | .576 | .614 | .614 |
| Panel B: IV Second Stage | | | | | |
| Δm | .830*** (.027) | .436*** (.024) | .370*** (.028) | .370*** (.040) | .360*** (.040) |
| Δ Unemployment | | | .089*** (.021) | .034 (.021) | .033 (.021) |
| Δ Pop. Under 25 share | | | -.277*** (.038) | .110*** (.042) | .111*** (.042) |
| Δ Pop. Over 65 share | | | -.005 (.047) | .268*** (.048) | .254*** (.050) |
| Panel C: IV First Stage | | | | | |
| $\Delta \hat{m}$ | .970*** (.017) | .954*** (.017) | .859*** (.018) | .761*** (.023) | .750*** (.024) |
| K.-P. rk Wald F statistic | 3,352 | 2,993 | 2,329 | 1,047 | 971.11 |

Notes. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis, clustered at the municipality level. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.

c. An upper bound exercise. IV baseline estimates suggest that a 1 pp increase in the population share of immigrants increases the VOX vote share by 0.281 pp (with the robustness *FD model* pointing to an increase in the aggregate right-wing vote share of 0.360 pp). Given naturalization of foreign-born individuals, baseline estimates should be interpreted as lower bounds for effects on anti-immigrant attitudes of natives. The fact that I obtain positive impacts of the immigration shock on vote shares suggests that the *direct effect* that naturalized immigrants have, through their participation in elections, is offset by the *indirect effect* that they have on vote shares, by shaping anti-immigrant attitudes of natives.

Based on aggregate data from the Spanish Statistics Institute (INE), in 2019, the average rate of naturalization of foreign-born individuals was 49% for Americans, 23% for Africans, 18% for Europeans and 17% for Asians. In each election year in my analysis, I use these ratios to compute the absolute number of foreign-born individuals that are naturalized, in each municipality. I assume that (i) the latter participate in general elections, with the average turnout in their municipality, and that (ii) they all vote to other-than-VOX parties (*VOX model*) or other-than-right wing parties (*FD model*). First, using these assumptions, I can compute “native votes” in the municipality, as the difference between total and assumed naturalized immigrants’ votes. Second, I obtain new measures of vote shares for VOX and right-wing parties, using “native votes” as a denominator, rather than total votes. Finally, using this new “native vote shares” variable, I re-run regressions, to re-estimate the impact of the immigration shock.¹⁰

Table 4. Baseline and upper bound IV estimates for the average effect

| | VOX model (1) | FD model (2) |
|---------------------------|--------------------|-------------------|
| Baseline estimate | .281*** (.107) | .360*** (.040) |
| K.-P. rk Wald F statistic | 68.70 | 971.11 |
| N. | 2,747 | 13,735 |
| Upper bound estimate | 1.032*** (.153) | .920*** (.087) |
| K.-P. rk Wald F statistic | 68.70 | 971.11 |
| N. | 2,747 | 13,735 |

Note. Column 1 estimates are based on the model specification of Column 5 in Table 2. Column 2 estimates are based on the model specification of Column 5 in Table 3. Baseline estimates result from regressions with the actual VOX or aggregate right-wing vote share as an outcome. Upper bound estimates result from regressions where the outcome is “native vote shares” for VOX or for right-wing parties (see [Section 6.1](#)).

¹⁰ Given I assume fixed naturalization rates across municipalities, variation in the new “native vote share” variable, relative to the actual vote share, comes from the size of immigration flows in each municipality, from its composition (in terms of the 4 immigrant origins in scope) and from the average turnout rate in the municipality.

Table 4 summarizes IV Second Stage results. Given the adjustment made to vote shares, I obtain an upper bound effect on the VOX vote share of 1.032 (0.920 in the robustness *FD model*).¹¹ Considering baseline and upper bound effects on the VOX vote share, the back-of-the-envelope calculation would imply that the 2019-2004 immigration shock treatment would account for 7.5 to 27.4% of VOX vote. Overall, I notice that the range of these baseline and upper bound estimates seems broadly aligned with results found in other European countries. Indicatively, A 1 pp increase in the population share immigrants increases the far-right vote share, in pp terms, from 0.22 to 0.35, in Italy (Barone et al., 2016), Germany (Otto & Steinhardt, 2014) and Austria (Halla et al., 2017); from 0.4 to 2.4 in France (Edo et al., 2019); from 1.3 to 2.8 in Denmark (Harmon, 2018).

Finally, indicating where the true effect may lie is a difficult task. If we were to assume that the naturalization assumptions used are broadly correct, there is evidence to believe that actual turnout of naturalized foreign-born individuals is generally well below average turnout in the municipality. While data is scarce, Muñoz (2011) found turnout of naturalized immigrants at 20%, compared to an average turnout of around 63% - using Spanish data of 2007 *local* (rather than general) elections. Indeed, if we were to assume turnout well below municipalities' average, upper bound estimates would decrease notably.

¹¹ The change between baseline and upper bound in the *VOX model* is slightly larger than that seen in the *FD model*. This might be partly because, in the *VOX model* setting, naturalized immigrants that vote are calculated over the total stock of foreign-born population in 2019 (i.e. including foreign-born population flows from 2004 to 2019 and the pre-existing stock). Meanwhile, the first-differenced setting only takes into account naturalized immigrants among the foreign-born population flows, over the years.

6.2 Heterogeneity by immigrant region

As argued in [Section 2.2](#), several authors have tested for effects of immigration inflows on political voting, depending on origin. The hypothesis is that more culturally-distant immigrant origins might fuel concerns about the cultural mix in compositional amenities, or of broader cultural threat. Table 5 reports full model specifications, where I replace aggregate immigration flows by region-specific immigrants, of the 4 origins in my sample. *VOX model* output is reported in Columns 1-4, while results for the *FD model* are available in Columns 5-8.

Looking at Panel A, OLS estimates in the *VOX model* suggest positive and statistically significant effects for immigrants of Africa and Europe on the VOX vote share - with the former having a higher point estimate. OLS results in the *FD model* also suggest statistically significant, positive associations of the aggregate right vote share with Africa and European immigrants. Yet, here, the America grouping is also associated with increased right-wing vote - although the effect is statistically significant at the 5% level and with a lower point estimate.

Moving to Panel C, IV First Stage results for the *VOX model* suggest that instrumentalization for American immigration is weak - given its low F statistic. That is, after accounting for the components of the full specification, results suggest weak network effects among American immigrants, over the entire 2019-2004 change. The rest of immigration instruments do not suffer from weak instrumentalization. In the *FD model*, meanwhile, all instruments for all immigrant origins have F statistics over 10.¹²

In Panel B, IV Second Stage estimates in the *VOX model* suggest a statistically significant, large, positive impact of African immigration on the VOX vote share. The rest of immigrant groups do not appear to generate statistically significant IV effects. The first-differenced *FD model* provides four insights. First, it confirms a statistically significant, positive impact of African immigration. Second, the Asian grouping has a positive, statistically significant effect - with a large point estimate, albeit relatively imprecise. Third, results also suggest positive impacts of European immigrants on right-wing vote, with a lower point estimate. Finally, the positive OLS effect of American immigrants on right-wing vote becomes insignificant, in the IV second stage.

To sum up, *VOX model* estimates suggest that only African immigrants increase the VOX vote share. Further, in the *FD model*, I learn that positive effects of immigration on right-voting might exist for Asian and European immigration, too. According to Spanish aggregate data, the key drivers for change from these regions are Morocco (70% of flows), China (42% of flows) and Romania (40% of European flows; 60% of EU28 flows).¹³ Meanwhile, estimates suggest that American immigrants are unlikely to have a significant effect on voting. These flows are primarily from Latin American countries.

¹² As discussed in [Section 5](#), differences First Stage coefficients appear to be (i) due to actual and predicted changes in-between election years being, on average, more closely linked, and (ii) due to predicted changes in-between election years having both negative and positive values.

¹³ For overall Europe, after Romania, the next contributors are Italy (10%), UK (8%), Russia (7%), Ukraine (7%) and Bulgaria (5%). The remaining contributions are rather fragmented (shares of 1-2%). For overall Asia, the next contributors after China are Pakistan (21%), India (11%) and Bangladesh (4%) - with remaining contributors being fragmented (shares of 1-2%).

Table 5. Baseline estimates by region of origin

| | VOX model | | | | FD model | | | |
|---------------------------|--------------------------|-------------------|-------------------|-------------------|--------------------------|-------------------|-------------------|--------------------|
| | Africa (1) | America (2) | Europe (3) | Asia (4) | Africa (5) | America (6) | Europe (7) | Asia (8) |
| | Panel A: OLS | | | | Panel A: OLS | | | |
| Δm | .341*** (.057) | .093 (.061) | .252*** (.040) | .003 (.130) | .249*** (.064) | .165** (.068) | .278*** (.025) | .006 (.204) |
| Year FE | N.A. | N.A. | N.A. | N.A. | Yes | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 2,747 | 2,747 | 2,747 | 2,747 | 13,735 | 13,735 | 13,735 | 13,735 |
| Adj. R2 | .788 | .785 | .790 | .784 | .609 | .609 | .614 | .609 |
| | Panel B: IV Second Stage | | | | Panel B: IV Second Stage | | | |
| Δm | .839*** (.205) | -2.451 (2.447) | .298 (.182) | -.255 (.411) | .984*** (.295) | -.141 (.217) | .432*** (.048) | 2.033*** (.686) |
| | Panel C: IV First Stage | | | | Panel C: IV First Stage | | | |
| $\Delta \hat{m}$ | .428*** (.063) | .053 (.042) | .321*** (.061) | .493*** (.095) | .450*** (.060) | .530*** (.034) | .815*** (.032) | .547*** (.084) |
| K.-P. rk Wald F statistic | 46.23 | 1.58 | 27.81 | 27.20 | 56.19 | 241.39 | 645.60 | 42.70 |

Notes. Columns 1-5 (*VOX model*) provide immigration effect estimates, with the model specification of Column 5 in Table 2. Columns 6-10 (*FD model*) provide immigration effect estimates, with the model specification of Column 5 in Table 3. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis, in Columns 1-5. Robust standard errors are clustered at the municipality level, in Columns 6-10. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.

Given the above results, one might hypothesize that the relatively shorter cultural distance with Latin American immigrants (i.e. in terms of language, religion, race, common historical roots), vis-à-vis the relatively greater cultural distance with the other immigrant groups, might suggest non-economic mechanisms. Yet, notice that, since immigrants naturalize to different extents, depending on their country of origin, they likely generate *direct effects* on voting outcomes of varying intensity. Latin Americans likely have the highest naturalization in my sample, due to more favourable minimum time of residence requirements. If Latin Americans naturalize and participate in elections substantially, their *direct effect* on vote shares could be hiding their *indirect effect* on vote shares. Further exploring this is important, as observing right-voting responses from Latin Americans flows would not be, a priori, fully consistent with non-economic mechanisms driving anti-immigrant attitudes of natives.

[Appendix 2](#) uses the upper bound exercise (with naturalization rates specific to each immigrant group), to estimate upper bound effects of immigrants by region of origin. I carry out this exercise only for the *FD* model, given strong First Stages in all immigrant groups. Even with upper bound estimates, I find that the American group does not exert a statistically significant impact on aggregate right-wing vote. Given the results of the *FD model*, I conclude that the America grouping is unlikely to generate voting responses. Overall, the combination of these results seems hence a priori compatible with non-economic mechanisms, underlying natives' concerns for the cultural mix in compositional amenities, or of broader cultural threat.

6.3 Heterogeneity by local economic conditions

As discussed in [Section 2.2](#), the economic deprivation hypothesis argues that anti-immigrant attitudes may intensify when immigrant presence is combined with poorer local economic conditions, in terms of unemployment. The latter might amplify actual or perceived labour market competition, and/or fuel economic insecurity of natives. Table 6 provides models where I interact the immigration shock with unemployment change (Columns 1-3). Further, I provide a separate interaction of the immigration shock, with a dummy variable taking value 1 for municipalities with high “long-run” unemployment levels. The latter is proxied by average registered unemployment throughout the years, and uses the median value of 10.16% across municipalities as a cut-off. I focus on OLS, given the added complexity that would involve instrumenting the interaction term in the IV model.¹⁴

Looking at the analysis for unemployment change (Columns 1-3), controlling for unemployment change barely affects the immigration treatment estimate (Column 2) - which would in principle suggest little correlation of immigration flows with the evolution of unemployment. Second, the interaction model (Column 3) shows that immigration flows, combined with a deterioration of labour market conditions, does increase VOX and aggregate-right wing vote. The effect is significant at the 5% level in the *VOX model*, while significant at the 1% level in the *FD model*. Unemployment change, on its own, does increase the VOX vote share, but this is not robust to the first-differenced framework.¹⁵

Turning to the analysis for high “long-run” unemployment levels (Column 5), I also find a positive and statistically significant interaction effect - significant at the 5% in the *VOX model*, while significant at the 1% level in the *FD model*. Further, *VOX model* output suggests that high “long-run” unemployment does *not* have an own-effect on the VOX vote share. That is, it only increases the VOX vote share when combined with the immigration shock. Further, first-differenced analysis suggests that high “long-run” unemployment has a negative own-effect on right-wing voting, while having a positive effect on the right-wing vote share through the immigration shock.¹⁶

¹⁴ In additional IV tests, I use Stata’s standard *reg* command to store predicted First Stage immigration change, and create interaction variables with unemployment change. I obtain similar results as with OLS.

¹⁵ I notice that the own-effect of immigration on voting tends to reduce once I control for its interaction with unemployment change, relative to the models where I omit the interaction. This might possibly suggest that the interaction effect is somewhat incorporated in the immigration own-effect estimates in Column 2.

¹⁶ The negative own-effect of unemployment on aggregate right-wing support could perhaps be explained by higher unemployment increasing left-wing support - e.g. through more demand for redistribution.

Table 6. OLS Baseline estimates of immigration interacted with unemployment

| | (1) | (2) | (3) | (4) | (5) |
|--------------------------------|-------------------|-------------------|-------------------|-------------------|--------------------|
| Panel A: VOX model | | | | | |
| Δm | .222*** (.028) | .221*** (.028) | .138*** (.050) | .222*** (.027) | .178*** (.035) |
| $\Delta Unemp$ | | .197*** (.036) | .121** (.048) | | |
| $\Delta m \times \Delta Unemp$ | | | .021** (.010) | | |
| HighUnemp | | | | .657*** (.198) | .129 (.285) |
| $\Delta m \times HighUnemp$ | | | | | .123** (.050) |
| Province FE | Yes | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes | Yes |
| Obs. | 2,747 | 2,747 | 2,747 | 2,747 | 2,747 |
| Adj. R2 | .790 | .792 | .793 | .790 | .791 |
| Panel B: FD model | | | | | |
| Δm | .257*** (.021) | .258*** (.021) | .224*** (.021) | .256*** (.021) | .138*** (.024) |
| $\Delta Unemp$ | | .031 (.021) | .027 (.021) | | |
| $\Delta m \times \Delta Unemp$ | | | .041*** (.008) | | |
| HighUnemp | | | | .059 (.050) | -.176*** (.055) |
| $\Delta m \times HighUnemp$ | | | | | .297*** (.034) |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes | Yes |
| Obs. | 13,735 | 13,735 | 13,735 | 13,735 | 13,735 |
| Adj. R2 | .614 | .614 | .615 | .614 | .617 |

Note. *HighUnemp* is a dummy taking value 1 for municipalities with average registered unemployment over the years, above the median of the sample (10.16%) - 0, otherwise. Estimates in Panel A are based on the model specification in Column 5 of Table 2. Estimates in Panel B result from the model specification in Column 5 of Table 3. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis, in Columns 1-3. Robust standard errors are clustered at the municipality level, in Columns 4-6. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.

To sum up - On the one hand, the immigration shock, combined with a *deterioration* in economic conditions, as measured by unemployment change, may led more intense effects of immigration on VOX vote. This may be due to amplified *actual* or *perceived* labour market competition, and/or due to broader economic insecurity perceptions of natives.¹⁷ On the other hand, the immigration shock combined with a poorer *level* of economic conditions, as measured by high “long-run” unemployment, may also intensify immigration effects on VOX vote. One may argue that the measure for “long-run” unemployment may less likely capture actual adverse effects of the immigration shock on natives’ employment outcomes (compared to the interaction with unemployment change).

¹⁷ More conclusively finding if actual labour market competition is the specific channel would in principle require unavailable data on skills of immigrants and natives. Informatively, existing empirical analyses for Spain have failed find sizeable adverse effects for natives’ labour market outcomes. In particular, unskilled immigrant labour force has been absorbed with higher total employment (González & Ortega, 2007), and no negative effects are identified for wages or employment rates of natives (R. Carrasco et al., 2008).

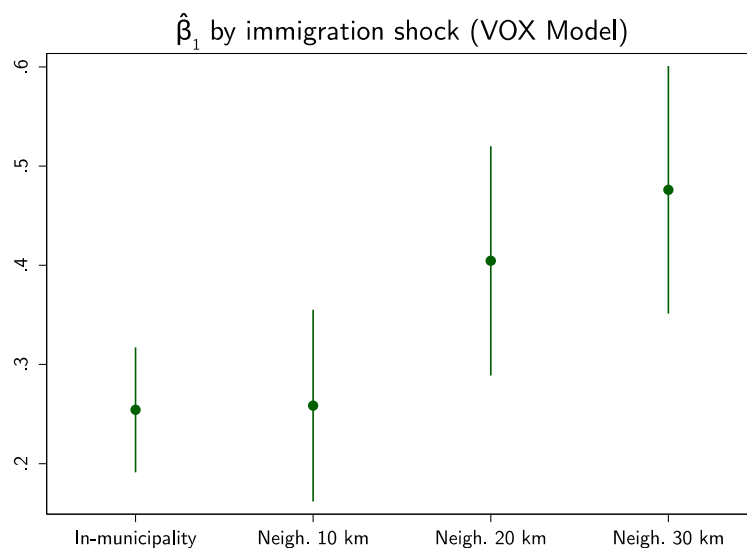
7. Neighbouring immigration

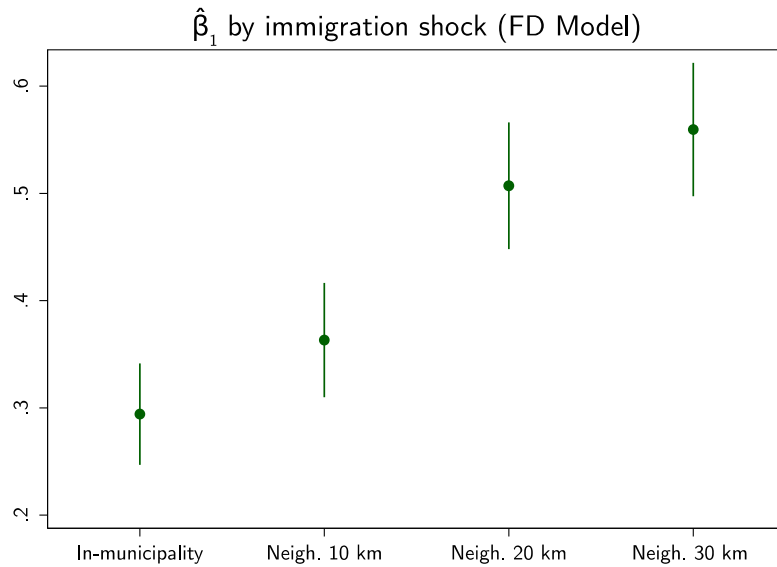
I now discuss voting responses to in-municipality immigration, and immigration in neighbouring municipalities. Inspired by Evans & Ivaldi (2021), the hypothesis is that in-municipality immigration may provide with relatively more regular and quality inter-group contact (e.g. in neighbourhoods, schools, residential areas), while neighbouring immigration inflows, possibly within travelling distance, may consist of more sporadic interactions (e.g. commuting or retail activities). While with less inter-group contact, the latter may still generate anti-immigrant attitudes, through the range of economic and non-economic mechanisms discussed in [Section 2.2](#).

Before discussing results, two notes shall be made. First, since I will use multiple immigration flows variables as regressors, providing IV estimates would require running multiple instruments for the multiple variables. Given added complexity, I focus on OLS results. Second, since some municipalities in my sample do not have neighbours nearby, I restrict the sample to municipalities that at least have one neighbour, within the smallest radius, of 10km (2,138 out of 2,742 in the sample).

Effects of in-municipality and neighbouring immigration. I start by providing estimates from separate regressions, where I estimate the effects of the immigration variables, one at a time. Figure 5 shows point estimates, with 95% confidence intervals. Neighbouring immigration shocks generate positive and statistically significant effects. Point estimates are larger than that of the in-municipality immigration variable, when I use the radiuses of 20 and 30 km, in both *VOX* and *FD models*. In the latter, this also holds for the 10 km radius. Estimates for the effect of neighbouring immigration appear to increase, when capturing shocks in further distant areas. This consistently happens, as I increase the concentric circles from 10 to 20km, and from 20km to 30km.

Figure 5. OLS Voting responses to in-municipality and neighbouring immigration shocks





The figure on the top (*VOX model*) provides OLS estimates, based on the model specification of Column 5 in Table 2. The figure on the bottom (*FD model*) provides OLS estimates based on the model specification of Column 5 in Table 3. Point estimates are provided along 95% confidence intervals.

As both in-municipality and neighbouring immigration may affect natives' voting, Table 7 provides models combining them (*VOX model* in Panel A; *FD model* in Panel B). Column 1 reports the in-municipality immigration effect alone. Columns 2 to 4 take into account distance-weighted immigration shocks in neighbouring municipalities - as measured in the 10, 20 and 30km concentric circles. In the largest radius of 30km, ceteris paribus, a 1 pp increase in neighbouring immigration, drives an increase in vote shares of 0.324 and 0.498 pp, in the *VOX* and *FD models*, respectively (Column 4).

Once I include neighbouring immigration presence in 20 and 30km concentric circles, neighbouring immigration are consistently above those of in-municipality immigration (Columns 3-4). The estimate for in-municipality flows reduces, as I control for immigration shocks further away. Notably, *FD model* output (Panel B) shows that the estimate for the in-municipality immigration reduces from 0.290 to 0.067, and becomes significant at the 10% level, when controlling for the neighbouring immigration in the 30km concentric circle (Column 4). This result suggests that in-municipality immigration shocks are positively correlated with immigration in neighbouring areas. This would imply that Column 1 estimates, which do not account for neighbouring immigration, might be picking up effects of the latter - hence being upwards biased (i.e. omitted variable bias).

Separately, I note that in-municipality immigration generates both *direct* and *indirect effects* on voting outcomes, being a lower bound for impact on anti-immigrant attitudes of natives. Meanwhile, immigration in neighbouring municipalities only generates *indirect effects*, being a "true" impact on anti-immigrant attitudes of natives. I check whether smaller impacts found for in-municipality immigration are driven by this in [Appendix 3.2](#) - with the upper bound estimates. Results show that, even when using in-municipality immigration upper bound estimates, neighbouring immigration for the 20 and 30km concentric circles have higher point estimates.

Table 7. Baseline OLS estimates of in-municipality and neighbouring immigration (VOX and FD models)

| | (1) | (2) | (3) | (4) |
|---|-------------------|-------------------|-------------------|-------------------|
| Panel A: VOX model | | | | |
| Δm | .254*** (.032) | .200*** (.032) | .180*** (.032) | .172*** (.032) |
| $\frac{\Delta m}{\Delta m}$ ^{10km} | | .151*** (.045) | | |
| $\frac{\Delta m}{\Delta m}$ ^{20km} | | | .263*** (.057) | |
| $\frac{\Delta m}{\Delta m}$ ^{30km} | | | | .324*** (.062) |
| Province FE | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes |
| Obs. | 2,138 | 2,138 | 2,138 | 2,138 |
| Adj. R2 | .827 | .828 | .829 | .830 |
| Panel B: FD model | | | | |
| Δm | .290*** (.024) | .159*** (.030) | .090*** (.031) | .067** (.032) |
| $\frac{\Delta m}{\Delta m}$ ^{10km} | | .260*** (.033) | | |
| $\frac{\Delta m}{\Delta m}$ ^{20km} | | | .430*** (.040) | |
| $\frac{\Delta m}{\Delta m}$ ^{30km} | | | | .498*** (.043) |
| Year and Province FE | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes |
| Obs. | 10,690 | 10,690 | 10,690 | 10,690 |
| Adj. R2 | .608 | .611 | .613 | .615 |

Note. Panel A provides estimates with the model specification of Column 5 in Table 2. Panel B shows estimates with the model specification of Column 5 in Table 3. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis in Panel A. Robust standard errors are clustered at the municipality level in Panel B. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.

Can lack of inter-group contact explain greater response to neighbouring immigrants? As discussed, greater neighbouring immigration effects might be expected given inter-group contact of natives with immigration in adjacent areas are likely relatively more sporadic. I now proceed with a test for whether larger effects noted for neighbouring immigration might be driven by lack of contact. If contact effects are part of the explanation, I hypothesize that contact built with in-municipality immigration might spill over to lower responses to neighbouring immigration. The underlying assumption is that the contact stock that is built with in-municipality interactions is *not* municipality-specific. I hence evaluate if in-municipality immigration shocks reduce responses to neighbouring immigration.

I use two approaches. First, I estimate continuous treatment model, where I add an interaction term, between in-municipality and neighbouring immigration. Second, I run a discrete treatment approach. Treated units are municipalities that receive relatively high in-municipality immigration shocks. I use a treatment dummy variable that takes value 1, when the municipality receives an in-municipality immigration shock that belongs to the top quartile of all flows, 0 otherwise. In the *VOX model*, this represents municipalities with average inflows of 8.71 pp; in the *FD model*, treated units have average inflows of 3.79 pp.¹⁸ Finally, because I find largest responses with the 30 km concentric circle measure, I focus on the latter.

Results are shown in Table 8. Panel A reports estimates for the continuous treatment approach (*VOX model* in Columns 1-3; *FD model* in Columns 4-6). Columns 3 and 6 contain the model where I control for in-municipality and neighbouring immigration, along their interaction. The sign of the interaction term is negative in both cases. Yet, it is only statistically significant, at the 1% level, in the first-differenced *FD model* (Column 6). The magnitude of the latter estimate seems relatively small. On average, a 1 pp increase in in-municipality immigration reduces responses to neighbouring immigration by 0.022 pp. A municipality with 0 in-municipality immigration shock would respond to a 1 pp increase in neighbouring immigration with a 0.582 pp increase in the aggregate right-wing vote share. The response to a 1 pp increase in neighbouring immigration would reduce to 0,560 pp if a municipality experiences an in-municipality immigration shock of 1 pp.¹⁹

¹⁸ Treatment in the *FD model* could be alternatively defined as per the top quartile of immigration flows within each period. Because this means using different thresholds, for the treatment dummy, depending on the period, interpretation of the treatment parameter would not be very obvious. For this reason, I set treatment based on flows from all periods - with a single threshold. Results are similar using both approaches.

¹⁹ Also, one can interpret the parameters of Column 6 as implying that the marginal effect of neighbouring immigration on voting is 0 for an in-municipality immigration shock of roughly 26 pp. Formally, the marginal effect for neighbouring immigration is provided by the following expression: $\frac{d\Delta Y}{d\Delta m}_{30km} = 0.582 - (0.022 \times \Delta m)$. By equating the latter to 0, one can find the in-municipality immigration shock that would bring the marginal effect of neighbouring immigration to 0.

Table 8. Baseline OLS interaction effects of in-municipality and neighbouring immigration (*VOX* and *FD* models)

| | VOX Model | | | FD Model | | |
|---|--------------------|-------------------|-------------------|-------------------|-------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: Continuous treatment | | | | | | |
| Δm | .254*** (.032) | .172*** (.032) | .181*** (.054) | .290*** (.024) | .067** (.032) | .146*** (.037) |
| $\frac{\Delta m}{\Delta m}^{30km}$ | | .321*** (.062) | .331*** (.077) | | .498*** (.043) | .582*** (.047) |
| $\Delta m \times \frac{\Delta m}{\Delta m}^{30km}$ | | | -.001 (.007) | | | -.022*** (.005) |
| Year FE | N.A. | N.A. | N.A. | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 2,138 | 2,138 | 2,138 | 10,690 | 10,690 | 10,690 |
| Adj. R2 | .827 | .829 | .830 | .608 | .615 | .616 |
| Panel B: Discrete treatment | | | | | | |
| $\Delta mTreat$ | 1.357*** (.209) | .849*** (.200) | .932*** (.552) | .878*** (.119) | .174 (.125) | .299** (.144) |
| $\frac{\Delta mTreat}{\Delta mTreat}^{30km}$ | | .418*** (.064) | .428*** (.076) | | .542*** (.033) | .630*** (.063) |
| $\Delta mTreat \times \frac{\Delta mTreat}{\Delta mTreat}^{30km}$ | | | -.017 (.105) | | | -.119* (.069) |
| Year FE | N.A. | N.A. | N.A. | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 2,138 | 2,138 | 2,138 | 10,690 | 10,690 | 10,690 |
| Adj. R2 | .821 | .828 | .828 | .603 | .614 | .615 |

Note. Columns 1-3 (*VOX model*) provide estimates with the model specification of Column 5 in Table 2. Columns 4-6 (*FD model*) show estimates with the model specification of Column 5 in Table 3. (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis in Columns 1-3. Robust standard errors are clustered at the municipality level in Columns 4-6. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.

Panel B provides results for the discrete treatment approach. The interactions of the treatment dummy with neighbouring immigration have negative signs, too. But, again, the interaction is only significant in the *FD Model* (Column 6). Moreover, the statistical significance level is of 10%. In this specification, on average, being treated with a high in-municipality immigration shock reduces voting responses to neighbouring immigration by 0.119 pp. An untreated municipality would respond to a 1 pp increase in neighbouring immigration with a 0.630 pp increase in the aggregate right-wing vote share. The response to a 1 pp increase in neighbouring immigration would be of 0.511 pp for a treated municipality.

A tentative interpretation. I find larger effects for neighbouring immigration, compared to in-municipality immigration shocks. Moreover, controlling for neighbouring immigration flows reduces effects observed for in-municipality immigration. This implies that the latter estimates might be upwards biased, hence questioning the magnitude of their effects. Overall, the larger effects observed for neighbouring immigration might be in principle compatible with contact being more sporadic for immigration placed adjacent to municipalities' boundaries. Yet, the specific mechanisms by which I observe large reactions for neighbouring immigration might seem at this stage relatively unclear.

On the one hand, regarding the contact hypothesis, I find only indicative evidence for interaction effects (in the *FD model*), with either quantitatively small effects, or with a 10% statistical significance level. One could possibly argue that inter-group contact, defined as natives' regularity and quality of interactions with immigrants, may not be well-measured at the municipality level.²⁰ Moreover, the lack of more convincing evidence could be because the contact stock that is built in-municipality does not spill over, because it is municipality-specific (e.g., depending on the immigration origin mix.).

On the other hand, alternatively, the lack of more convincing interaction evidence might also suggest that the greater effects observed for neighbouring immigration cannot be entirely explained only through relatively less contact. The neighbouring immigration variable could also be reflecting other-than-contact channels by which immigrant presence could trigger stronger natives' anti-immigrant attitudes. For example, it might be that my 20 or 30 km radiuses might reflect immigrants' presence in the employment pool where natives work. If I do capture effects at the employment pool, immigration-related threat concerns might intensify through the labour market competition channel (David et al., 2018).

²⁰ First, in-municipality immigration might not properly capture the quality and quantity of interactions of natives with immigrants. E.g., more refined measures, such as immigrant presence at the neighbourhood or tract where they live, might arguably be a better proxy (Kaufmann & Goodwin, 2018). Further, similarly, my distance measures to neighbouring immigration may not be good proxy for natives' interactions and awareness of immigration outside of where the municipality. This would deserve developing measures that capture the actual knowledge and time spent outside of natives' living areas.

8. Conclusions

Growing support to anti-immigration parties across European countries has motivated scrutiny of the effects of immigration on political voting, with many finding that immigration is paying a dividend to the far-right. The case of Spain is special. Even though Spain has received a large immigrant wave, since the end of the 1990s, Spain did not have an anti-immigrant party, until the rise of VOX in 2019. This exceptionalism, along the abrupt entry of VOX in an extraordinary time in Spanish politics, questioned whether VOX vote came from latent anti-immigrant attitudes, and deserved being studied.

Using Spanish municipality data and a past settlements instrument, I test if the VOX vote share in 2019 can be associated to foreign-born population flows in the 2004-2019 period. For robustness, I estimate a first-differenced model with changes in immigration and aggregate-right wing support, between election years from 2004 to 2019. While the core of my paper assesses the effects of immigration flows at the municipality level (*in-municipality immigration*), I also subsidiarily evaluate effects of immigration flows in areas adjacent to municipalities (*neighbouring immigration*).

First, I find that immigration flows at the municipality level have led to higher VOX support. Indeed, this suggests that VOX has capitalized on latent anti-immigration attitudes of natives. Estimates that include the possible role of naturalized foreign-born individuals on municipalities' voting outcomes suggest that a 1 pp increase in the population share of immigrants might drive an increase in the VOX vote share from 0.281 to 1.032 pp. The back-of-the-envelope calculation would imply that the 2019-2004 immigration growth treatment might account from 7.5% to 27.4% of VOX support. Overall, I have noted that the direction of this finding, and point estimates, seem broadly consistent with the patterns that others have noted in other European countries - suggesting a similar behaviour for the Spanish case.

Second, both non-economic and economic mechanisms might be at play. The heterogeneity of effects by region of origin, with culturally-close Latin American immigration not materializing into voting responses, could suggest natives care about the cultural mix in compositional amenities, and/or that they fear cultural change. Moreover, immigration effects on VOX support intensify with poor or deteriorating local economic conditions, in terms of unemployment - which suggest that natives also worry about the economic consequences of immigration (including labour market competition). The mixture of these mechanisms is also consistent with results elsewhere in Europe, which find that anti-immigration attitudes are a complex phenomenon, possibly driven by a combination of factors.

Third, immigration flows adjacent to municipalities, possibly within travelling distance, i.e. neighbouring immigration, suggest that the former generate substantial voting responses. In fact, natives' far-right voting appears to be to a greater extent influenced by immigration flows adjacent to the municipalities, rather than in their own municipality. Importantly, the magnitude of responses to the latter tends to reduce, suggesting that the effects of in-municipality immigration might be actually picking up responses to immigration adjacent to where natives reside. This therefore suggests caution, in terms of the point estimates found for in-municipality immigration. I have argued larger reactions to neighbouring immigration might be compatible with inter-group contact being of more sporadic nature, outside of the municipalities where natives reside - although there could exist other channels.

Regarding further research, it is my analysis for the effects of immigration adjacent to areas where natives live, that particularly opens two questions. First, it may be helpful to understand

all the mechanisms by which natives may react more strongly to immigration outside of the areas where natives reside - and to try to separate them from dynamics operating when immigration is present in areas where natives reside. While lack of inter-group contact may be part of the story, more accurate measures of inter-group contact are needed (other than mere geographical distance), to better describe the quantity and quality of interactions between natives and immigrants. Moreover, it would be helpful to understand if the larger reactions to immigrants outside of the areas where natives reside may also have to do with other-than-contact mechanisms, which might intensify when measuring more contextual immigrant presence - e.g. competition in the employment pool. Second, there is a more general question with the measurement of immigrant presence. When measured at municipality level, estimates may actually pick up effects from spatially correlated immigration presence in adjacent areas. This is something that further research, at units such as the municipality level, should consider.

9. References

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Appendices

Appendix 1. Further descriptive statistics

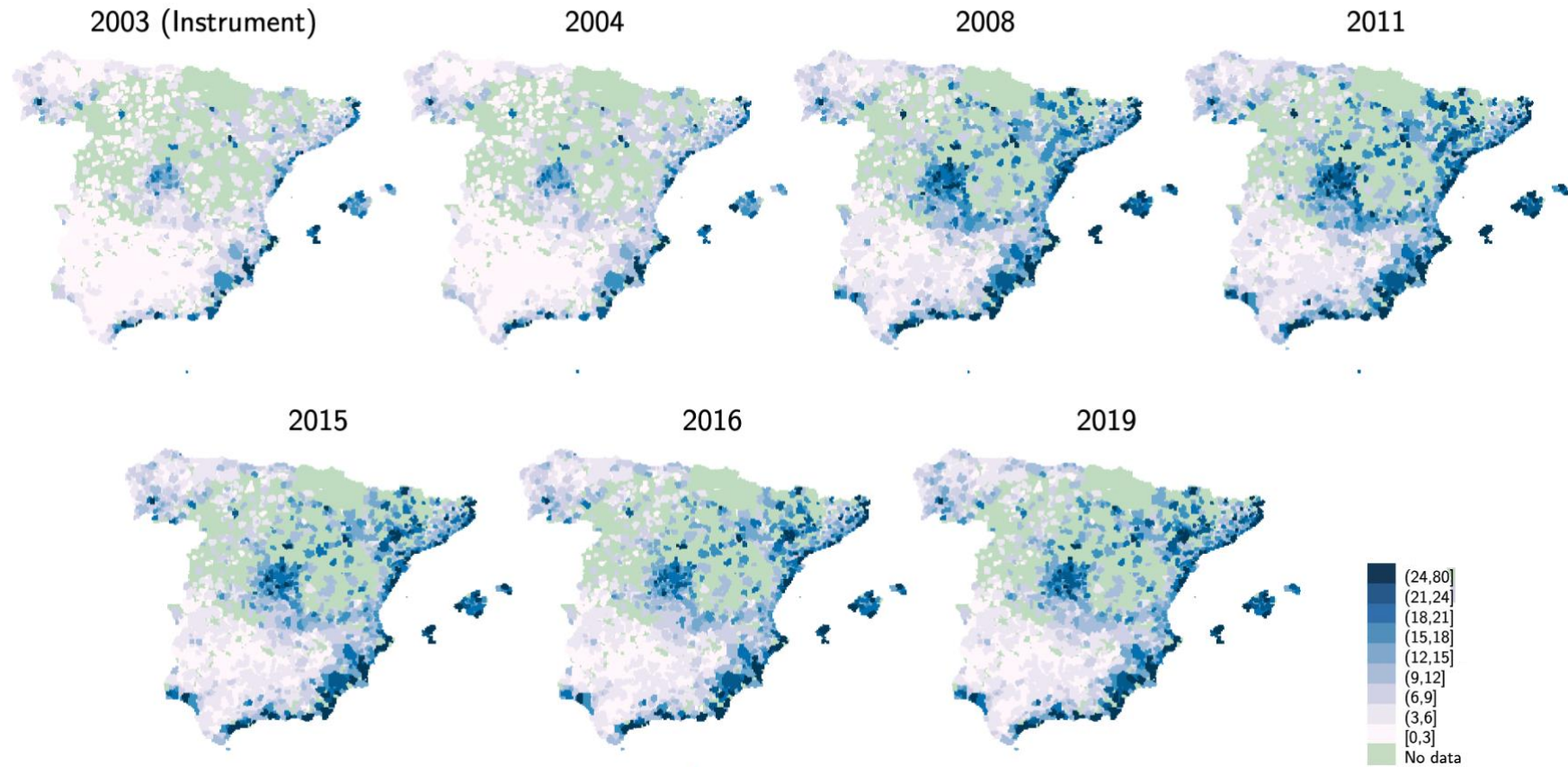
Appendix 1.1 Descriptive statistics for the FD model

Table 9. Descriptive statistics (*FD model*)

| Variable | Unit | Obs. | Mean | Std. Dev. | Min. | Max. |
|--------------------------------|------|--------|-------|-----------|---------|----------|
| Panel A: Immigration variables | | | | | | |
| Share of total imm. | % | 16,482 | 9.44 | 7.99 | 0.00 | 76.17 |
| Δ Share of total imm. | pp. | 13,735 | 0.80 | 2.48 | -12.60 | 28.43 |
| Share of Africa imm. | % | 16,482 | 1.74 | 2.47 | 0.00 | 31.62 |
| Δ Share of Africa imm. | pp. | 13,735 | 0.18 | 0.62 | -4.80 | 9.65 |
| Share of America imm. | % | 16,482 | 2.59 | 2.49 | 0.00 | 27.43 |
| Δ Share of America imm. | pp. | 13,735 | 0.23 | 0.68 | -3.90 | 8.46 |
| Share of Europe imm. | % | 16,482 | 4.84 | 5.69 | 0.00 | 71.62 |
| Δ Share of Europe imm. | pp. | 13,735 | 0.34 | 2.01 | -13.49 | 29.23 |
| Share of Asia imm. | % | 16,482 | 0.27 | 0.51 | 0.00 | 9.27 |
| Δ Share of Asia imm. | pp. | 13,735 | 0.05 | 0.17 | -2.46 | 3.31 |
| Panel B: Outcome variables | | | | | | |
| RWing vote share | % | 16,482 | 42.34 | 15.70 | 2.37 | 90.09 |
| Δ RWing vote share | pp. | 13,735 | 1.09 | 6.37 | -27.15 | 33.38 |
| Panel C: Control variables | | | | | | |
| Unemployment | % | 16,482 | 10.70 | 4.96 | 1.00 | 36.30 |
| Δ Unemployment | pp. | 13,735 | 0.68 | 3.46 | -11.78 | 16.55 |
| Pop. Over 65 share | % | 16,482 | 19.35 | 6.81 | 2.87 | 48.70 |
| Δ Pop. Over 65 share | pp. | 13,735 | 0.24 | 1.18 | -22.84 | 9.47 |
| Pop. Under 25 share | % | 16,482 | 24.47 | 4.93 | 7.06 | 38.99 |
| Δ Pop. Under 25 share | pp. | 13,735 | -0.63 | 1.06 | -6.87 | 14.82 |
| Population | th. | 16,482 | 14.94 | 78.60 | 0.24 | 3,266.12 |
| Δ Population | th. | 13,735 | 0.27 | 2.22 | -123.05 | 113.48 |

Note. The time dimension of the panel is election years. In the period 2004-2019, this means covering years 2004, 2008, 2011, 2015, 2016, and 2019. Variables in labels provide statistics for all data points in these years. Variables in Δ express statistics for changes in between election years.

Appendix 1.2 The spatial distribution of immigrants by election year



Note. These figures depict the share of immigrants (defined as foreign-born individuals, as percentage of population) across Spanish municipalities in the sample. 2003 is the year used for the instrument. 2004 and 2009 are the election years used to predict 2019-2004 immigration change, in the *VOX model*. The rest of election years, from 2004 to 2009, are used for to predict immigration change for periods between election years, in the robustness *FD model*.

Appendix 2. An upper bound exercise

Based on aggregate data from the Spanish Statistics Institute (INE), in 2019, the average rate of naturalization of foreign-born individuals was 49% for Americans, 23% for Africans, 18% for Europeans and 17% for Asians. In each election year, from 2004 to 2019, I use these ratios to compute the absolute number of foreign-born individuals that are naturalized. I assume that (i) the latter participate in general elections, with the average turnout in their municipality, and that (ii) they all vote to other-than-VOX or other-than-right wing parties. First, using these assumptions, I can compute “native votes” in the municipality, as the difference between total and assumed immigrants’ votes. Second, I obtain new measures of vote shares for VOX and right-wing parties, using “native votes” as a denominator, rather than total votes. Finally, using this new “native vote shares” variable, I re-run regressions.²¹ A similar approach is taken by Mendez & Cutillas (2014).

Upper bound average effect. Table 9 summarizes IV baseline and upper bound estimates, for the average effect of immigrants.

Table 10. Baseline and upper bound IV estimates for the average effect

| | VOX model (1) | FD model (2) |
|---------------------------|--------------------|-------------------|
| Baseline estimate | .281*** (.107) | .360*** (.040) |
| K.-P. rk Wald F statistic | 68.70 | 971.11 |
| N. | 2,747 | 13,735 |
| Upper bound estimate | 1.032*** (.153) | .920*** (.087) |
| K.-P. rk Wald F statistic | 68.70 | 971.11 |
| N. | 2,747 | 13,735 |

Note. Column 1 estimates are based on the model specification of Column 5 in Table 2. Column 2 estimates are based on the model specification of Column 5 in Table 3. Baseline estimates result from regressions with the actual VOX or aggregate right-wing vote share as an outcome. Upper bound estimates result from regressions where the outcome is “native vote shares” for VOX or for right-wing parties, computed as explained in [Appendix 2](#).

²¹ Given I assume fixed naturalization rates across municipalities, variation in the new “native vote share” variable, relative to the actual vote share, comes from the size of inflows in each municipality, and its composition (in terms of the 4 immigrant origins in scope) and from the average turnout rate in the municipality.

Upper bound effect by immigrant region. For upper bounds of immigrants by region of origin, I focus on the *FD model*. Here, the strong First Stages across all immigrant groups allows me to understand the direction of effects across all groups.

Table 11. Baseline and upper bound IV estimates by region of origin (*FD model*)

| | Africa (2) | America (3) | Europe (4) | Asia (5) |
|---------------------------|--------------------|-----------------|--------------------|---------------------|
| Baseline estimate | .984*** (.295) | -.141 (.217) | .432*** (.048) | 2.033*** (.686) |
| K.-P. rk Wald F statistic | 56.19 | 241.39 | 645.60 | 42.70 |
| N. | 13,735 | 13,735 | 13,735 | 13,735 |
| Upper bound estimate | 1.493*** (.381) | .393 (.293) | 1.100*** (.117) | 3.817*** (1.112) |
| K.-P. rk Wald F statistic | 56.19 | 241.39 | 645.60 | 42.70 |
| N. | 13,735 | 13,735 | 13,735 | 13,735 |

Note. Estimates are based on the model specification of Column 5 in Table 3. Baseline estimates result from regressions with the actual aggregate right-wing vote share as an outcome. Upper bound estimates result from regressions where the outcome is “native vote shares” for right-wing parties, computed as explained in [Appendix 2](#).

Appendix 3. Further neighbouring immigration analysis

Appendix 3.1 Measurement

In order to measure distances between municipalities, I calculate these taking the geographical centeroid of each municipality area as a reference. To do so, I use shapefiles of the Spanish Statistics Institute (INE) as per 2022.²² For this exercise, I restrict my sample to 2,138 municipalities, out of the 2,747 included elsewhere in my analyses. The remaining municipalities have been excluded because they had no municipality neighbours in the nearest radius, of 10 km. In terms of calculus, Equation 6 provides the expression for the weighted average of immigration shocks in the 10km radius, using the *VOX model* notation:

$$\overline{\Delta m_{i19-04}}^{10km} = \frac{\sum_{j=1}^n (v_j \times \Delta m_{j19-04})}{\sum_{j=1}^n v_j}, \text{ where } v_j = \frac{1}{d_j^2} \quad (6)$$

The subscript i denotes a given municipality, with j being its neighbours, within the 10km radius. Each immigration shock in a j neighbour (Δm_{j2019}), is weighted with v_j . The latter depends on the squared inverse distance d , to municipality i . $\overline{\Delta m_{i19-04}}^{10km}$ is the resulting weighted sum of immigration shocks in neighbouring municipalities within the 10km radius.

Table 12 provides descriptive statistics for the sample employed in this analysis (for *VOX* and *FD models*). First, Table 12 shows that, within the 10km radius, the average municipality has 5.16 neighbours. This increases to 17.02 and 32.41 neighbours, in the 20 and 30 km radiuses. Second, Table 5 shows the statistics for the resulting distance-weighted immigration shocks in each radius. Comparing mean and standard deviation values of in-municipality immigration, with neighbouring immigration shocks, values are similar. Variables are indeed substantially correlated (raw correlation between the in-municipality and the neighbouring variables stands at 0.61 to 0.66, statistically significant at the 1% level).

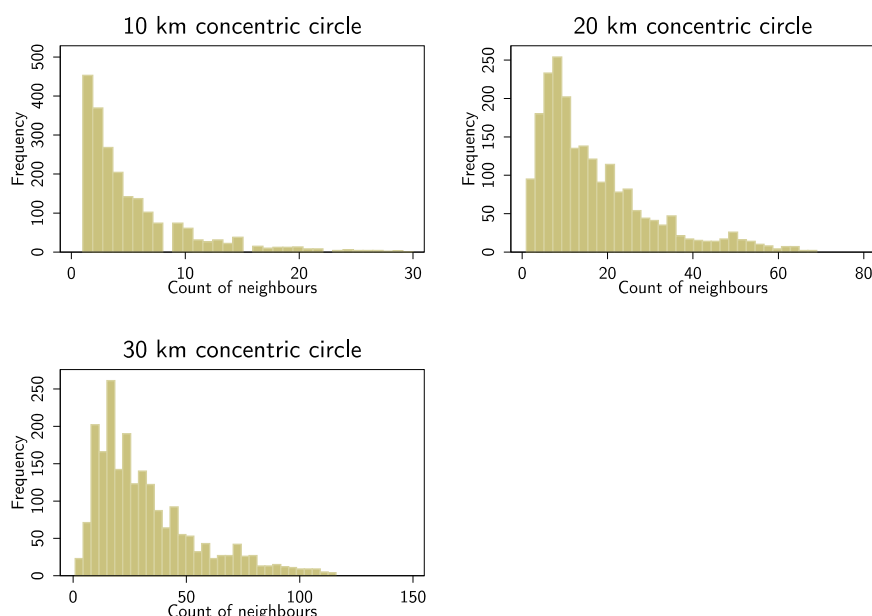
²² Note that municipalities' territorial boundaries may have changed over the 2004-2019 period. This could be a relevant measurement limitation if territorial boundaries change substantially.

Table 12. Descriptive statistics for the neighbouring immigration analysis

| Variable | Unit | Obs. | Mean | Std. Dev. | Min. | Max. |
|---|------|--------|-------|-----------|--------|-------|
| Panel A: VOX Model | | | | | | |
| Count of neighbours (10km) | N. | 2,138 | 5.16 | 4.96 | 1.00 | 30 |
| Count of neighbours (20km) | N. | 2,138 | 17.02 | 12.94 | 1.00 | 69 |
| Count of neighbours (30km) | N. | 2,138 | 32.41 | 22.79 | 1.00 | 116 |
| Δ Share of total imm. (10 km) | pp. | 2,138 | 4.05 | 3.08 | -2.30 | 33.14 |
| Δ Share of total imm. (20 km) | pp. | 2,138 | 4.02 | 2.56 | -1.30 | 24.93 |
| Δ Share of total imm. (30 km) | pp. | 2,138 | 4.01 | 2.41 | -1.06 | 22.68 |
| Δ Share of total imm. (in-municipality) | pp. | 2,138 | 4.01 | 3.55 | -2.78 | 33.14 |
| Panel B: FD Model | | | | | | |
| Count of neighbours (10km) | N. | 10,690 | 5.16 | 4.96 | 1.00 | 30 |
| Count of neighbours (20km) | N. | 10,690 | 17.02 | 12.94 | 1.00 | 69 |
| Count of neighbours (30km) | N. | 10,690 | 32.41 | 22.79 | 1.00 | 116 |
| Δ Share of total imm. (10 km) | pp. | 10,690 | .81 | 2.27 | -9.72 | 28.21 |
| Δ Share of total imm. (20 km) | pp. | 10,690 | .80 | 2.08 | -7.44 | 21.28 |
| Δ Share of total imm. (30 km) | pp. | 10,690 | .80 | 2.03 | -6.90 | 19.81 |
| Δ Share of total imm. (in-municipality) | pp. | 10,690 | .80 | 2.49 | -12.60 | 28.43 |

Note. Panel A deltas denote 2019-2004 change. Panel B deltas refer to change between election years. The count of neighbours represents the number of municipalities that are within a given concentric circle measure (in terms of geographical distance between municipalities' centeroids). The counts of neighbours do not change in Panel A and Panel B because territorial boundaries are measured as per one single year, 2022 (according to shapefiles of the Spanish Statistics Institute, INE)

Figure 6. Frequency histograms for the count of neighbours included in each concentric circle



Note. The count of neighbours represents the number of municipalities that are within a given concentric circle measure (in terms of geographical distance between municipalities' centeroids). Distances are based on territorial boundaries as per 2022 (according to shapefiles of the Spanish Statistics Institute, INE).

Appendix 3.2 Upper bound estimates

Table 13. Upper bound OLS estimates of in-municipality and neighbouring immigration

| | (1) | (2) | (3) | (4) |
|--|-------------------|-------------------|-------------------|-------------------|
| Panel A: VOX model upper bound estimates | | | | |
| Δm | .432*** (.053) | .362*** (.052) | .335*** (.051) | .327*** (.051) |
| $\frac{\text{---}10\text{km}}{\Delta m}$ | | .193*** (.059) | | |
| $\frac{\text{---}20\text{km}}{\Delta m}$ | | | .340*** (.074) | |
| $\frac{\text{---}30\text{km}}{\Delta m}$ | | | | .412*** (.084) |
| Province FE | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes |
| Obs. | 2,138 | 2,138 | 2,138 | 2,138 |
| Adj. R2 | .815 | .817 | .820 | .820 |
| Panel B: FD model upper bound estimates | | | | |
| Δm | .680*** (.054) | .514*** (.055) | .439*** (.058) | .411*** (.057) |
| $\frac{\text{---}10\text{km}}{\Delta m}$ | | .329*** (.058) | | |
| $\frac{\text{---}20\text{km}}{\Delta m}$ | | | .518*** (.067) | |
| $\frac{\text{---}30\text{km}}{\Delta m}$ | | | | .599*** (.073) |
| Year and province FE | Yes | Yes | Yes | Yes |
| (Initial) Urban FE | Yes | Yes | Yes | Yes |
| Rest of controls | Yes | Yes | Yes | Yes |
| Obs. | 10,690 | 10,690 | 10,690 | 10,690 |
| Adj. R2 | .598 | .602 | .604 | .605 |

Note. Panel A provides estimates with the model specification of Column 5 in Table 2. Panel B shows estimates with the model specification of Column 5 in Table 3. Upper bound estimates result from regressions where the outcome is “native vote shares” for VOX or for right-wing parties, computed as explained in [Appendix 2](#). (Initial) Urban FE denotes dummy variables for municipalities with population over 10,000 and 50,000, set as per initial population. Robust standard errors are provided in parenthesis in Panel A. Robust standard errors are clustered at the municipality level in Panel B. *** Denotes statistical significance at the 1%, ** at the 5%, and * at the 10% levels.