

# Not welcome anymore: the effect of electoral incentives on the reception of refugees

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## Abstract

Do electoral incentives affect immigration policies? Exploiting the Italian system for refugees' reception and data from Italian municipalities, we show that proximity to elections reduces the probability that a municipality applies to host a refugee center by 26%, despite the economic benefits arising from these centers. Low electoral competition and high shares of extreme-right voters drive the effect. Our results are rationalized by a theoretical model and can explain the unequal distribution of refugees across and within countries.

**Keywords:** migration, reception of refugees, electoral incentives, fiscal grants

**JEL classifications:** R23, J61, D72, C23

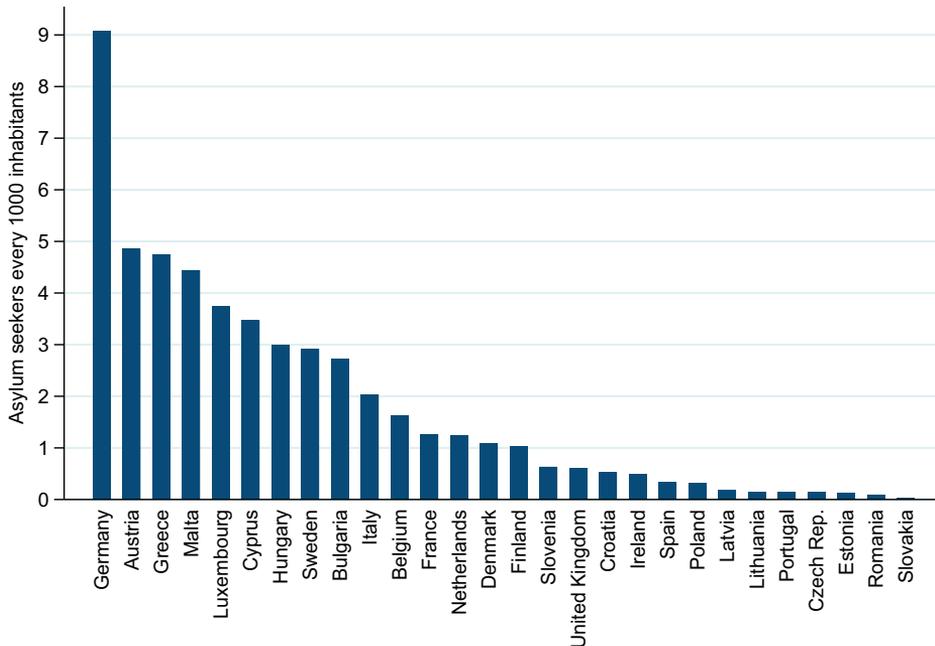
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## 1. Introduction

Recently, international migration has become a hotly debated issue. It has been one of the central topics in the electoral campaign of Donald Trump and the Brexit referendum. Moreover, following the increased flow of people seeking protection in western countries, the reception of refugees has become a critical challenge. Many national and local governments refuse to host refugees and asylum seekers, producing asymmetries in terms of 'responsibility' or 'burden-sharing' across and within countries (Thielemann et al., 2010; Fernández-Huertas Moraga and Rapoport, 2014, 2015; Dustmann et al., 2017). As shown in Figure 1, the unbalance in the reception of asylum seekers across countries was stark in 2016. Given the high numbers of people fleeing war and political persecution and uncertainty about how to respond among national and local governments, it is important to understand the political determinants of immigration policies (Fisher Williamson, 2018).

As described in Section 2, recent literature in economics and political science has demonstrated that immigration influences electoral results, with rising support for extreme-right parties and anti-immigration policies (Barone et al., 2016; Dinas et al., 2018; Hangartner et al., 2018; Dustmann et al., 2019; Tabellini, 2020). However, while the literature has produced results about the behavior of voters (i.e. the demand side), there has been limited attention to immigration policies and the behavior of politicians dealing with immigration issues (i.e. the supply side).

This article contributes to filling this gap. We study how electoral incentives affect governments' immigration policies, specifically the reception of refugees and asylum seekers. As immigration affects electoral outcomes (Barone et al., 2016; Dinas et al., 2018; Hangartner et al.,



**Figure 1.** Unbalanced reception EU countries in 2016.

Source: Eurostat.

2018; Dustmann et al., 2019; Tabellini, 2020; Vertier et al., 2023), and given that politicians can anticipate voters' reactions, we can expect governments to manipulate immigration policies to gain votes or avoid losing popularity. In addition, if voters do not observe politicians' preferences (Drazen and Eslava, 2010), we can expect politicians to manipulate immigration policies before elections to signal that their preferences are close to those of voters.

We investigate this question using data from Italian municipalities from 2005 to 2017 (see Section 3 for a description of the dataset and [Supplementary Appendix A](#) for descriptive statistics). We take advantage of a peculiar refugee allocation policy promoted by the Italian Home Office, called 'The Protection System for Asylum Seekers and Refugees' (SPRAR). SPRAR centers are the second level of reception (as opposed to the first level, which receives migrants who just entered Italy and allows them to apply for asylum), and their goal is to promote the integration of refugees and asylum seekers. SPRAR centers are allocated to municipalities through tenders issued by the Home Office. Municipalities that open a SPRAR center receive fiscal grants from the central government. To give an idea of the significant economic magnitude of SPRAR grants, we calculate that the average per capita SPRAR grant was equal to 26% of the total per capita grants and 8% of the total per capita municipal budget. Thus, opening a reception center may be an investment for a municipality, benefiting the local economy. There is, in fact, anecdotal evidence that describes how municipalities in the program benefit from hosting refugees and the fiscal grants received.<sup>1</sup> Besides, [Gamalerio et al. \(2021b\)](#) show how SPRAR centers positively affect

1 For example, Cityscope (5 November 2015): 'In Italy, a struggling town looks to refugees for revival'; BBC News (26 September 2016): 'Riace: The Italian village abandoned by locals, adopted by migrants'; Linkiesta (5 November 2016; in Italian): 'Il welfare buono dei migranti, che al Sud crea ricchezza e lavoro.'

local ‘compositional amenities’ and population growth, suggesting that the economic benefits of SPRARs may go beyond the fiscal grants received. [Supplementary Appendix B](#) describes the Italian institutional setting, including its refugee reception system.

As Section 4 describes, for the empirical analysis, we exploit two features of the SPRAR system. First, municipalities can choose whether to participate in the tender issued by the Home Office and bid to open a reception center on their territory (i.e. refugee policy is locally controlled). This setup enables us to analyze governments’ immigration policies avoiding the limitations of cross-Country studies, whose findings are biased by cross-Country institutional and cultural differences. In addition, the large number of Italian municipalities allows us to exploit the substantial variation in immigration policy decisions across different areas of Italy. Second, the timing of the tenders is determined by the Home Office and international events and is exogenous to local circumstances and the timing of municipal elections. Thus, although municipal governments can decide whether or not to open a reception center, the timing of decisions vis-a-vis the timing of elections is out of their control. Combining the exogenous timing of SPRAR’s tenders and the staggered timing of municipal elections<sup>2</sup> allows comparing mayors in the final year of their term (i.e. just before elections) when the Home Office launches a tender with mayors in other years of their term. Following the literature ([Labonne, 2016](#)), we interpret the parameter estimated through this comparison as the effect of electoral incentives on the probability of opening a reception center.

As described in Section 5, our analysis shows that the probability of bidding for opening a reception center is 26% lower for municipalities in the final year of the term (i.e. just before new elections) when the Home Office issues a tender, compared with municipalities in other years of the term. The findings are robust to different specifications and survive a series of robustness checks, which we report in [Supplementary Appendix D](#). We further implement two heterogeneity analyses. First, we show that the negative effect of electoral incentives on refugees’ reception is reduced in municipalities where political competition, measured by the mayors’ margin of victory, is high. As discussed in Section 2, the result challenges previous conclusions on the effect of political competition on politicians’ behavior ([List and Sturm, 2006](#)) and represents one of the main contributions of the paper. Next, we show that municipalities with higher shares of voters with extreme-right political preferences drive the main results. These are typically individuals who feel strongly against immigration and whose vote can be highly conditioned by the decision to open a refugee center in their municipality.

Our results highlight the effect of electoral incentives on a municipality’s decision to host refugees. In [Supplementary Appendix F](#), we provide a model that rationalizes these results. When elections are far in time, incumbents follow their preferences and bid to open the refugee center only if they believe this is the right thing to do. As elections approach, the decision becomes conditioned by the need to attract votes.<sup>3</sup> This can push a pro-immigration incumbent to refuse to host refugees in an attempt to attract the votes of individuals that feel strongly against immigration. The larger this group, the stronger the

2 Municipal elections are staggered for historical reasons, due to past government crises interrupting electoral mandates before the natural deadline. Interruptions are less frequent today (only 5% in the data studied). [Coviello and Gagliarducci \(2017\)](#) and [Repetto \(2017\)](#) discuss the exogeneity of municipal election dates in Italy.

3 We assume that decisions taken at the beginning of the term do not affect voters’ decision to re-elect the mayor. This myopic behavior by voters is consistent with empirical and anecdotal evidence, as discussed in [Supplementary Appendix F](#).

incentives to do so. At the same time, however, not opening the refugee center implies foregoing the economic benefits generated by it, possibly losing the vote of the other part of the population. The more the voting decision of this group can be swung by the mayor's choice (i.e. the more competitive elections are), the lower the incentives to please anti-immigration voters at the end of the term.

In [Supplementary Appendix C](#), we provide further evidence supporting our intuition. Using a survey of Italian mayors implemented by the association Italian National Election Studies (Itanes), we produce descriptive statistics about the opinions of politicians about immigration. Although the survey asks questions about migration in general and not specifically on refugees' reception, the answers of the mayors may still help understand their motivations. The descriptive statistics show that most mayors interviewed think immigrants are good for the economy. However, the statistics also show that most mayors think most voters would not favor receiving more immigrants. This evidence suggests that mayors might not open refugee centers just before elections because they fear being punished by voters and not because they think receiving refugees may be detrimental to the economy. The intuition is reinforced by the suggestive evidence produced using electoral data, which shows that opening a refugee center in the final year of the term negatively correlates with the incumbent's vote share at the next election.

Finally, Section 6 and [Supplementary Appendix E](#) discuss how the effect of electoral incentives on refugees' reception can persist beyond the end of the electoral term, eventually leading to an unbalanced reception of refugees across municipalities in the medium and long run. Moreover, we provide suggestive evidence on these potential medium and long-run consequences. We show that municipalities where electoral incentives affected the reception of refugees more strongly in the past host a smaller share of refugees and have a lower probability of opening a refugee center in the last year available in the data. We also provide evidence that political competition seems to attenuate this medium-run persistence of the negative effect. Conversely, higher shares of extreme-right voters seem to drive this medium-run persistence. This evidence suggests that electoral incentives can lead to an unbalanced reception of refugees in the medium and long run. Section 7 concludes by describing the potential policy implications of our results and proposes potential future lines of investigation.

## 2. Related literature

This article contributes to different strands of the literature. First, it contributes to the literature on the distortive effect of electoral incentives on incumbents' policy-making decisions (see, among others, [Canes-Wrone et al., 2001](#); [Maskin and Tirole, 2004](#); [Acemoglu et al., 2013](#); [Ash et al., 2017](#)). This literature has shown how these incentives generate electoral cycles in public expenditures ([Akhmedov and Zhuravskaya, 2004](#); [Alt and Dreyer Lassen, 2006](#); [Drazen and Eslava, 2010](#); [Repetto, 2017](#)), taxes ([Alesina and Paradisi, 2017](#)), fiscal grants ([Brollo and Nannicini, 2012](#); [Bracco et al., 2015](#)) and employment levels ([Labonne, 2016](#)). In these articles, politicians provide voters with economic benefits in terms of higher (lower) public expenditures (taxes) or employment opportunities to gain popular support. Our article shows that politicians might instead decide to forgo significant economic benefits if this helps them gain the electoral support of a strategically important part of the population.

In this respect, our article is close to the literature on single-minded voters and secondary policy decisions (List and Sturm, 2006; Bouton et al., 2021). These articles show how, when deciding on issues like the environment, gun control or abortion rights, politicians might ignore their personal preferences and pander to the interests of a group whose vote crucially depends on these issues. Contrary to our findings, List and Sturm (2006) show that higher electoral competition increases the likelihood of an equilibrium where a politician against ‘green’ policies decides to pass them. The main difference between their setting and ours is that, at least at the time when the paper was written, environmental policies did not have any impact on fiscal policy or, more generally, on economic well-being. Indeed, in their model, decisions on environmental aspects do not affect the vote of the individuals who place low salience on the issue. Because of the positive economic benefits generated by immigration, this is not the case in our setting, and the decision to open a refugee center also affects voters who do not care about immigration through an indirect economic channel. Most importantly, our results show that when this indirect effect is present, the conclusions on the effect of electoral competition are completely reversed.

The article also contributes to the literature on the impact of immigration on the support for extreme-right parties and anti-immigration policies (Barone et al., 2016; Dinas et al., 2018; Hangartner et al., 2018; Dustmann et al., 2019; Tabellini, 2020). While this literature provides evidence about voters’ behavior (i.e. the demand side), there is little evidence about politicians dealing with immigration issues (i.e. the supply side). As far as we know, the only exceptions are Folke (2014), Facchini and Steinhardt (2011), Casarico et al. (2018) and Gamalerio et al. (2021a).<sup>4</sup> These papers look at different aspects of the same question and are complementary to our analysis. Folke (2014) focuses on how party representation affects immigration and environmental policies in Swedish municipalities. Facchini and Steinhardt (2011) and Casarico et al. (2018) study the determinants of the voting behavior of U.S. Congressmen concerning the legalization of undocumented migrants. Finally, Gamalerio et al. (2021a) focus on the effect of electoral systems in shaping migration policies.

The central intuitions of the article apply to other policies that, similarly to immigration (Dustmann et al., 2012; Dustmann and Frattini, 2014), may produce broad benefits but present concentrated costs or meet local opposition for ideological, cultural or economic reasons (Ferwerda et al., 2017). Examples of these policies are housing and urban development policies (Ahlfeldt, 2011; Ortalo-Magne and Prat, 2014), environmental policies (Stokes, 2015), big infrastructure projects (Ahlfeldt and Maennig, 2015) and all those policies that meet the opposition of ‘Not In My Back Yard’ movements (Fischel, 2001).

Other papers study the problem of immigration in the Italian context. Barone et al. (2016) studied the impact of immigration on the vote shares of extreme-right parties. Bratti et al. (2020), Gamalerio et al. (2021b) and Campo et al. (2021) study the electoral impact of the reception of refugees. Bracco et al. (2018) and Romarri (2020) show that the election of extreme-right mayors influences the location of migrants and hate crimes against them. Finally, Genovese et al. (2017) use survey data to study how public opinion is affected by exposure to refugee centers. Our article contributes to these works by looking at the role played by local governments and how they respond to electoral incentives.

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4 In addition, Farris and Holman (2017), Thompson (2019) and Magazinnik (2018) provide evidence that political factors drive the enforcement of local immigration laws by part of U.S. sheriffs.

### 3. Data

We use data on Italian municipalities for the years 2005–2017. First, we use data on the SPRAR tenders issued from 2005 to 2017. These data come from three different sources: the Home Office webpage, the webpage of SPRAR and the ‘Briguglio archive’, an online archive with material about migration. We have used the ‘Briguglio archive’ for double-checking the information from the official sources. The dataset on SPRARs contains information on the municipalities that bid for opening a SPRAR, those that won the bid, and the amount of SPRAR grants received. We use this information to build the dependent variable used in the analysis below.

We then use data on municipalities’ characteristics. From the Italian Statistical Office (ISTAT), we collect data on the following characteristics measured during the 2001 Census: the share of university graduates, the share of children (less than 5 years old) and elderly (more than 65 years old), population density, the area of the municipality (in squared kilometers), altitude, latitude, longitude, unemployment rate and the number of non-profit organizations per capita. From the Italian Home Office, we get information on income after taxes per capita and the number of firms per capita measured in 2005. We also get information on the presence of first-level reception centers (see [Supplementary Appendix B.2](#)). From [Cartocci \(2007\)](#), we get the number of non-sport daily newspapers sold every 1,000 people, measured in 2001. From ISTAT, we collect information on the municipal population and the share of migrants legally resident, which we measure as time-variant variables at the beginning of each electoral term. Data on politicians come from the Home Office and contain personal characteristics such as gender, age, employment status, past political experience, education, political affiliation, term-limited status and whether the electoral mandate was interrupted earlier than the natural deadline. We use this information to build our control variables.

To conduct our heterogeneity analysis, we collect data on municipal and European election results from the Italian Home Office. We include in the final sample all observations with non-missing data on SPRAR tenders, the treatment variable (i.e. mayors in the final year of their term) and the two main heterogeneity dimensions (i.e. the level of electoral competition and the share of anti-immigrant voters).<sup>5</sup> The final dataset is an unbalanced panel dataset composed of 71,162 observations, containing information on 7290 municipalities for the period 2005–2017.<sup>6</sup> We report the descriptive statistics of this dataset in [Table A1 of Supplementary Appendix A](#).

### 4. Empirical strategy

We run the following model:

$$\text{Refugees\_Centre}_{it} = \beta_0 + \beta_1 \text{Final}_{it} + \beta_2 X_{it} + \lambda_t + \gamma_i + \eta_{it}, \quad (1)$$

where  $\text{Refugees\_Centre}_{it}$  is equal to 1 if municipality  $i$  bids for opening a SPRAR center during tender  $t$ . The treatment  $\text{Final}_{it}$  is 1 for mayors in the final year of the term when

5 To maximize the sample size, we keep the observations with missing values in the municipal characteristics, replacing the missing observations with the sample mean and including a dummy variable for these observations. Similarly, we replace the observations with missing values in the personal characteristics of the mayor with a 0 and include a dummy variable equal to 1 for these observations. This procedure allows us to increase the sample size and obtain more precise estimates. The results are robust to the exclusion of these observations.

6 This sample constitutes around 90% of the total number of Italian municipalities, which, on the occasion of the 2011 Census, were 8092. We used a bigger sample based on 8025 municipalities in a previous version of the article. The results obtained with this sample were quantitatively and qualitatively the same.

tender  $t$  is issued and 0 for mayors in other years of the term. To assign SPRAR tenders to the correct electoral term and thus build  $\text{Final}_{it}$  appropriately, we exploit the fact that we know the starting and ending dates of the period during which a municipal government can bid for opening a SPRAR center. These dates are reported in Columns 3 and 4 of [Table B1 in Supplementary Appendix B](#). Combining these dates with the date of the elections, we can correctly assign SPRAR tenders to electoral terms. In a few cases where the election date lies between the starting and ending dates of a SPRAR tender, we assign the tender to the electoral term that covers the biggest share of the bidding window.

Municipal fixed effects  $\gamma_i$  control for the dependent variable's unobserved time-invariant municipal determinants. Municipal and mayoral characteristics are collected in  $X_{it}$ .<sup>7</sup> Given the structure of the data, standard errors are likely to be serially correlated within municipalities. Therefore, we cluster standard errors at the municipality level.<sup>8</sup> The parameter of interest  $\beta_1$  estimates the effect of electoral incentives on the reception of refugees. Identifying  $\beta_1$  relies on two sources of variation. First, the timing of the SPRAR tenders is decided by the Home Office and is exogenous to municipal circumstances and elections. Second, we combine this exogenous timing with the staggered schedule of municipal elections, which are not held simultaneously. The combination of the exogenous timing of SPRAR tenders with the staggered schedule of municipal elections is represented in [Figure 2](#), which reports the share of municipalities in the final year of the term by tender.

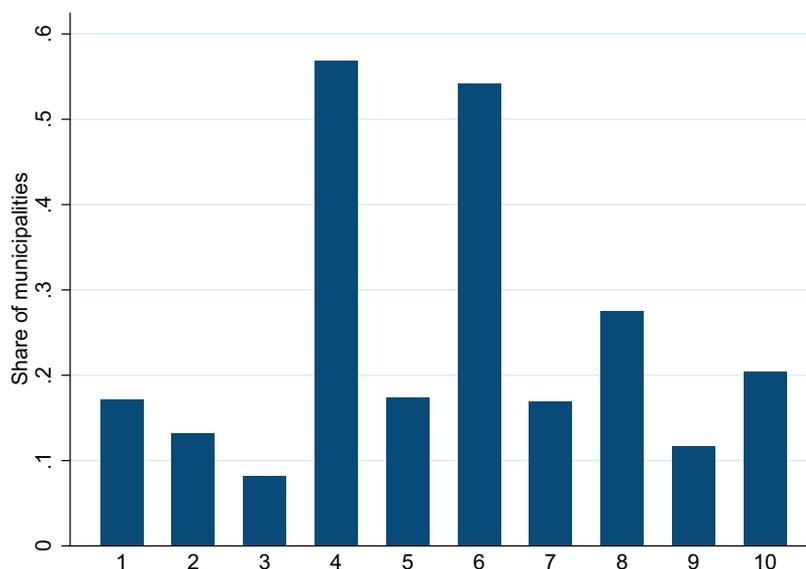
These two sources of variation enable us to deal with the two main threats to the identification strategy. First, the fact that the Home Office decides the timing of SPRAR tenders means that  $\text{Final}_{it}$  is exogenous to local circumstances, and municipal governments do not control it. We reinforce this idea through the robustness checks in [Supplementary Appendix D](#), where we show that the results are unchanged if we control for the small share (only 5%) of electoral mandates interrupted before the natural deadline. Second, the staggered schedule of municipal elections enables us to include tender fixed effects  $\lambda_t$ , which allows us to distinguish the effect of electoral incentives from the one of common shocks like, for example, changes in economic and political conditions. The inclusion of tender and municipality fixed effects implies that we identify the effect of  $\text{Final}_{it}$  by comparing the probability of opening a SPRAR in municipalities that are in the final year of the term during tender  $t$  and the probability in municipalities that are not in the final year of the term during tender  $t$ . We cannot implement a 'within term' analysis and control for year of election fixed effects because, within the same term, we cannot have municipalities simultaneously in the final year of the term (the treatment group) and municipalities not in the final year (the control group). Hence, we would not have variation in  $\text{Final}_{it}$  within the same term.

Finally, following the literature on electoral cycles ([Labonne, 2016](#); [Repetto, 2017](#)), in [Supplementary Appendix D](#), we show that the results are unchanged if we control for differential linear, quadratic and non-linear time trends across labor market areas (LMAs)<sup>9</sup>

7 As described in Section 3, some municipal characteristics are time-invariant. In the full model, municipal fixed effects  $\gamma_i$  already capture these variables. In other specifications, we remove municipal fixed effects and show that including these time-invariant variables does not change the results.

8 Results do not change if we cluster standard errors at the provincial or LMAs level. Results can be made available upon request.

9 LMAs are geographical areas where most labor force lives and works, and firms can find the labor force needed. Thus, LMAs are sub-regional areas constituted by municipalities with similar economic and social characteristics. LMAs do not correspond to any level of government.



**Figure 2.** Share municipalities in the final year of the term by tender.

*Notes:* The figure plots the share of municipalities in the final year of the electoral term by tender. The x-axis reports the number of the tender (see [Table B1 in Supplementary Appendix B](#) for more information), while the y-axis the share of municipalities in the final year of the electoral term.

*Sources:* Home Office.

and electoral groups<sup>10</sup> and the interaction terms between tender fixed effects and municipal and mayoral characteristics. We also use the routine of [de Chaisemartin and D'Haultfœuille \(2020\)](#) to show that the potential presence of negative weights in the estimation of the average treatment effects produced by two-way fixed effects models is not an issue in our analysis.

## 5. Results: electoral incentives and reception of refugees

We estimate [Equation \(1\)](#) using the sample of Italian municipalities from 2005 to 2017. Panel A of [Table 1](#) reports the baseline results obtained running model 1. Panel B reports the results of an alternative specification in which the main variable  $Final_{it}$  is replaced by four different dummy variables for the years 2–5 of the electoral term. Columns 1–3 report the results obtained using the sample of 7290 Italian municipalities over the years 2005–2017 and Columns 4–6 the results obtained considering only the municipalities that bid for opening a SPRAR at least once during the same period. The reason for keeping only the municipalities that bid at least once is that these municipalities differ from the other municipalities in terms of observable characteristics (see [Table A1 in Supplementary Appendix A](#)).

10 As described in [Table A2 in Supplementary Appendix A](#), we can divide municipalities into five electoral groups, depending on the first election date found in the data.

**Table 1.** Effect of electoral incentives on the reception of refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome = 1 mayor bids for opening SPRAR						
Panel A: treatment is final year of electoral term						
Sample	All municipalities			Open at least one refugee center		
Final	-0.008*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)	-0.047*** (0.008)	-0.052*** (0.009)	-0.061*** (0.009)
Mean outcome	0.034	0.034	0.034	0.200	0.200	0.200
R-squared	0.170	0.321	0.322	0.185	0.301	0.338
Observations	71,162	71,162	71,162	12,245	12,245	12,245
Number of municipalities	7,290	7,290	7,290	1,254	1,254	1,254
Panel B: treatment years 2–5 electoral term						
Sample	All municipalities			Open at least one refugee center		
Year 2 term	0.002 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.005 (0.013)	-0.007 (0.013)	-0.012 (0.012)
Year 3 term	0.003 (0.002)	-0.000 (0.002)	0.000 (0.002)	0.004 (0.011)	-0.007 (0.011)	-0.006 (0.012)
Year 4 term	-0.001 (0.002)	-0.003 (0.002)	-0.003 (0.003)	-0.002 (0.013)	-0.012 (0.013)	-0.012 (0.013)
Year 5 term	-0.007** (0.003)	-0.010*** (0.003)	-0.010*** (0.003)	-0.048*** (0.014)	-0.061*** (0.014)	-0.071*** (0.015)
Mean outcome	0.038	0.038	0.038	0.229	0.229	0.229
R-squared	0.170	0.321	0.322	0.185	0.301	0.338
Observations	71,162	71,162	71,162	12,245	12,245	12,245
Number of municipalities	7,290	7,290	7,290	1,254	1,254	1,254
Tender FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	No	Yes	Yes
Time invariant controls	Yes	No	No	Yes	No	No
Time variant controls	Yes	No	Yes	Yes	No	Yes

*Notes:* All Italian municipalities, years 2005–2017. Treatment variables: the treatment variable Final in Panel A is equal to 1 for mayors in the final year of the term and 0 otherwise. The treatment variables in Panel B are: Year term 2 = 1 for mayors in the second year of the term; Year term 3 = 1 for mayors in third year of the term; Year term 4 = 1 for mayors in fourth year of the term; Year term 5 = 1 for mayors in the fifth year of the term. The outcome variable is equal to 1 for mayors who bid for opening a refugees' reception center during tender  $t$ . Time invariant controls: share of graduates, share elderly (>65), share children (<5), log of income per capita, number of firms per capita, population density, area, altitude, latitude, longitude, unemployment rate, dummy variable for first level reception centers, number no-profit organizations per capita and daily newspapers circulation. Time variant controls: population, municipal share of migrants, dummy variable for past participation to SPRAR, dummy female mayor, age mayor, dummy unemployed mayor, political experience mayor, dummy graduate mayor, dummy left mayor, dummy independent mayor, dummy term limit and dummy for early interruption mandate. Robust standard errors clustered at the municipality level are in parentheses.

Significance at the 10% level is represented by \*, at the 5% level by \*\* and at the 1% level by \*\*\*.

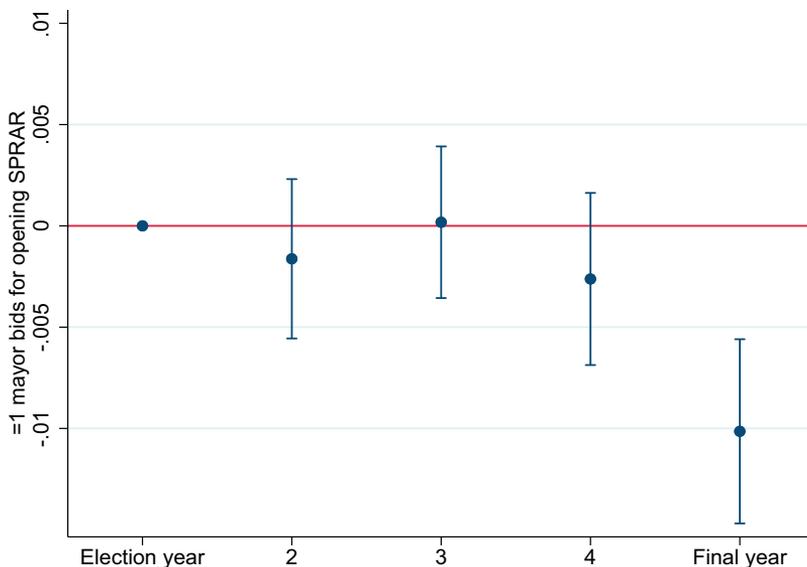
The results in Columns 1–3 of Panel A show that electoral incentives negatively impact refugees' reception. The coefficients are statistically significant at the 1% level and are stable across three different specifications. We find that mayors in the final year of the term have a lower probability of bidding for a SPRAR than mayors in the other years of the term, with a reduction of approximately 26% compared with the outcome variable's mean. A similar picture emerges if we consider the sub-sample of municipalities that bid to open a refugee center at least once in the period 2005–2017. A possible interpretation of this sub-sample is that it comprises municipalities where, on average, mayors reveal a preference for hosting refugees. Under this interpretation, our results would suggest that even mayors that, on average, are relatively more open to immigration act freely upon their preferences at the beginning of the term but not necessarily at the end when electoral incentives are more relevant.<sup>11</sup> We believe this provides further support for our theoretical assumption that voters put more weight on more recent events when evaluating politicians' performance. Finally, the results in Columns 1–6 of Panel B, also plotted in [Figure 3](#), show that the effect is concentrated in the final year of the term. The results also show that differences in behavior between the other years of the electoral term do not emerge.

Then, we investigate which factors drive this negative effect. We analyze the role of electoral competition and the share of anti-immigrant voters. We report the results of this heterogeneity analysis in Panel A of [Table 2](#). To build a measure of electoral competition, we assign to all the points in time in our data the difference in the vote shares (i.e. the margin of victory) between the first and the second mayoral candidates from the most recent municipal election. Municipal elections in our dataset go from 2001 up to 2016. Then, following [Barone et al. \(2016\)](#), we create an index of political competition, which is the municipal average margin of victory between the first and the second candidates in all municipal elections observed, with lower values indicating a higher political competition. Using this variable, we create a dummy variable called Political competition<sub>*i*</sub>, which is 1 for municipalities with an index of political competition below the median (i.e. high political competition) and 0 otherwise. Columns 2 of [Table 2](#) reports the coefficients of the interaction term Final<sub>*it*</sub> × Political competition<sub>*i*</sub>. The positive coefficient indicates that in areas where political competition is intense, the negative effect is smaller, with a reduction of approximately 21% compared with the mean of the dependent variable. These results indicate that political competition can play an important role in reducing the negative effect of electoral incentives and suggest that adopting institutions and policies that foster electoral competition may lead to more open immigration policies ([Barone et al., 2016](#)).

In Column 3 of Panel A of [Table 2](#), we interact Final<sub>*it*</sub> with the variable capturing the presence of anti-immigrant voters at the municipal level. We build this variable using data from the 2004, 2009 and 2014 European elections. We assign to all the points in time in our data the vote share taken by extreme-right parties in a municipality at the most recent European election.<sup>12</sup> Similarly to what was done with Political competition<sub>*i*</sub>, we calculate the municipal average across time. We then create the dummy variable Extreme-right

11 If we repeat the same analysis keeping only *mayors* who bid to open a refugee center at least once during their electoral terms, we get similar results even with this smaller sample composed of 7479 observations. For example, using the complete specification of Panel A, Column 6 of [Table 1](#), we get a coefficient equal to  $-0.051$  with a standard error equal to 0.017. If we replace municipal fixed effects with mayor fixed effects and cluster the standard errors at the mayor level, we get a coefficient equal to  $-0.065$  with standard errors equal to 0.017.

12 We use data from European elections because of two features of its proportional electoral system. First, voters usually vote sincerely. Second, political parties usually run alone, without forming coalitions, which allows getting data on the vote shares of every single party.



**Figure 3.** The effect of electoral incentive on the reception of refugees.

*Notes:* The figure plots the baseline effect of electoral incentives on the reception of refugees. The x-axis reports the years of the electoral mandate. More specifically: (a) election year is the first year of the electoral mandate (i.e. the beginning of the electoral mandate after the last elections). This year is used as default category in this graph; (b) 2, 3 and 4 represent years 2, 3 and 4 of the electoral mandate; (c) final year is the last year of the electoral mandate (i.e. just before the next elections). The y-axis reports the dummy variable equal to 1 for mayors who bids for opening a SPRAR center.

voting<sub>*i*</sub> taking value 1 for municipalities with an average share of extreme-right votes above the median (i.e. a high share of anti-immigrant voters) and 0 otherwise.<sup>13</sup> The coefficient of the interaction term  $Final_{it} \times Extreme\text{-}right\ voting_{it}$  is negative and significant. Municipalities with a share of anti-immigrant voters above the median experienced an even stronger negative effect of electoral incentives, with a reduction in the probability of bidding for a refugee center of approximately 24% of the outcome variable mean.<sup>14</sup> In Column 4 of Table 2, we show that the heterogeneity results persist even if we control for additional interaction terms between  $Final_{it}$  and other municipal characteristics.

The heterogeneity measures in Panel A of Table 2 are taken from the same years in which the SPRAR system was in place and are potentially endogenous. The fact that we use measures derived from averages over time should partly reduce this concern, as these variables should capture the long-term and structural characteristics of the municipality,

13 We have identified extreme-right parties using the following positions in the political spectrum indicated by Wikipedia: left, center-left, center, center-right, right and extreme-right. The variable *Extreme-right voting<sub>*i*</sub>* is built starting from the sum of the vote shares of the parties in the position 'right' (Alleanza Nazionale, Fratelli d'Italia, La Destra and Lega Nord) and 'extreme-right' (Alternativa Sociale, Fiamma Tricolore, Forza Nuova and Movimento Idea Sociale-Rauti). Using alternative ways to locate the parties in the spectrum (e.g. Itanes surveys) would lead to a similar aggregation.

14 Municipalities with more extreme-right preferences may be more likely to elect a right-wing mayor. However, the coefficients on  $Final_{it} \times Extreme\text{-}right\ voting_{it}$  are unchanged if we control for the interactions between  $Final_{it}$  and the political orientation of the mayor. Results are available upon request.

**Table 2.** Heterogeneity analysis

	(1)	(2)	(3)	(4)
Outcome = 1 mayor bids for opening SPRAR				
Panel A: current heterogeneity dimensions				
Final	-0.009*** (0.002)	-0.013*** (0.002)	-0.005** (0.002)	-0.002 (0.089)
Final × Political competition		0.007** (0.003)		0.008*** (0.003)
Final × Extreme-right			-0.008*** (0.003)	-0.008** (0.003)
Mean outcome	0.034	0.034	0.034	0.034
R-squared	0.322	0.322	0.322	0.324
Observations	71,162	71,162	71,162	71,162
Number of municipalities	7,290	7,290	7,290	7,290
Panel B: Past heterogeneity dimensions				
Final	-0.009*** (0.002)	-0.011*** (0.002)	-0.005** (0.002)	0.005 (0.088)
Final × Political competition		0.005* (0.003)		0.005* (0.003)
Final × Extreme-right			-0.008*** (0.003)	-0.008** (0.003)
Mean outcome	0.034	0.034	0.034	0.034
R-squared	0.322	0.319	0.323	0.321
Observations	71,162	70,818	71,037	70,703
Number of municipalities	7,290	7,254	7,277	7,242
Tender FE	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes
Time invariant controls	No	No	No	No
Time variant controls	Yes	Yes	Yes	Yes
Additional interactions	No	No	No	Yes

*Notes:* All Italian municipalities. Years 2005–2017. Treatment variables: the treatment variable Final is 1 for mayors in the final year of the term and 0 otherwise. The outcome variable is 1 for mayors who bid for opening a SPRAR center during tender  $t$ . Time variant controls: population, municipal share of migrants, dummy variable for past participation to SPRAR, dummy female mayor, age mayor, dummy unemployed mayor, political experience mayor, dummy graduate mayor, dummy left mayor, dummy independent mayor, dummy term limit and dummy for early interruption mandate. Variables interacted with Final: (1) Political competition is a dummy variable equal to 1 if the average municipal margin of victory is below the median. In Panel A, we use the average over municipal elections from 2001 up to 2016. In Panel B, municipal elections from 1993 up to 2000. (2) Extreme-right voting = 1 if the average vote share taken by extreme-right parties is above the median. In Panel A, we use the average over the European elections in 2004, 2009 and 2014. In Panel B, elections in 1999 and 2004. Additional interaction terms with Final included in Column 4 but not reported here: municipal share of migrants, daily newspapers circulation, unemployment rate, dummy variable for past participation to SPRAR, number of firms per capita, share of graduate, number no-profit organizations per capita, log of income per capita, share elderly (>65), share children (<5), dummy variable for first level reception centers, population, population density, past foreign population growth rate (average from previous electoral term) and past income growth rate (average from previous electoral term). Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\* and at the 1% level by \*\*\*.

which should be less influenced by the single opening of SPRAR centers in specific years. However, to further address this concern, we collect data on electoral competition and the share of extreme-right voters from years predating our sample. Specifically, we construct a new version of Political competition<sub>*i*</sub> using data on the municipal margin of victory in municipal elections from 1993 to 2000. Similarly, we construct a new version of Extreme-right voting<sub>*i*</sub> using data on extreme-right vote shares from the 1999 and 2004 European Elections. We report the results obtained with these measures in Panel B of [Table 2](#). Even though these new variables are measured in the past and thus do not necessarily capture the contemporaneous municipal political environment,<sup>15</sup> the results confirm those in Panel A.<sup>16</sup> The results in Panel B of [Table 2](#) reassure us that the results in Panel A are not due to the potential effect of SPRAR centers on the two main heterogeneity dimensions studied.

## 6. Unbalanced reception of refugees in the medium run

A possible criticism of the results of this article is that mayors who do not apply for a refugee center in the final year of the term are just postponing the possible application after the elections. If this were the case, the results of this article would not be an issue for refugee reception in the medium-long run since, eventually, all municipalities will bid to open a center. Here, we discuss and provide suggestive evidence of why we think the effect of electoral incentives can persist beyond the end of the electoral term and have consequences in the medium-long run, eventually leading to an unbalanced reception of asylum seekers and refugees.

First, [Figure E1 in Supplementary Appendix E](#) shows that the influx of migrants and the need to receive asylum seekers and refugees is not constant over time. [Figure B1 in Supplementary Appendix B](#) shows this is also the case for the SPRAR system. Since municipalities do not vote simultaneously, we can expect municipalities to host a different number of asylum seekers even in the medium-long run. For example, we can expect municipalities that in a year with a significant influx of migrants are not close to elections to host more migrants in the long run than municipalities that in the same year are closer to the next election. Besides, as shown in Section 5, heterogeneous political features of the different places can lead to electoral cycles of different intensity and sign. This heterogeneity can also lead to an unbalanced reception of migrants over time, even if municipalities were voting simultaneously.

We also provide suggestive evidence that the effect of electoral incentives can persist beyond the end of the electoral term and in the medium-long run. We study the correlation between the magnitude of the effect of electoral incentives on refugee reception in the past and refugee reception in the last year available in the data. As described in detail in [Supplementary Appendix E.1](#), we implement a two-step procedure following [Labonne](#)

15 It is important to highlight the potential tradeoff between using heterogeneity measures from the same years in which the SPRAR system was in place and using heterogeneity measures from past years. While the former captures the current status of the municipal political environment more accurately, they may be affected by the opening of SPRAR centers and thus be endogenous, potentially leading to biased estimates. Conversely, the latter cannot be affected by the opening of SPRAR centers and thus are exogenous but may not accurately capture the current municipal political environment, potentially leading to less precise estimates. Given this tradeoff, we think the best solution is to present the results obtained using both current and past heterogeneity measures.

16 The smaller number of observations in Columns 2–4 of Panel B of [Table 2](#) are due to missing values in these two past heterogeneity measures.

**Table 3.** Correlation magnitude electoral incentive and the reception of refugees in 2017

	(1)	(2)	(3)	(4)	(5)
Outcome	Share refugees in 2017	Share refugees in 2017	Share refugees in 2004	Open SPRAR center last tender	Open SPRAR center last tender
Magnitude electoral incentives	-5.970* (3.106)	-5.642* (2.987)	-1.317 (1.186)	-0.211* (0.109)	-0.208** (0.103)
Share refugees in 2004	0.937*** (0.041)	0.908*** (0.048)			
Mean outcome	39.88	39.88	25.76	0.0939	0.0939
R-squared	0.691	0.714	0.837	0.406	0.451
Observations	6,407	6,407	6,407	6,709	6,709
LMA FE	Yes	Yes	Yes	Yes	Yes
Time invariant controls	No	Yes	Yes	No	Yes
Time variant controls	No	Yes	Yes	No	Yes

*Notes:* All Italian municipalities, year 2017. Treatment variables: Magnitude electoral incentives = magnitude of the effect of electoral incentives on the probability of not opening a refugee center during the tenders in years 2005–2016. Outcome variables: (1) in Columns 1 and 2, Share refugees in 2017 = migrants from countries of origin of refugees every 1,000 inhabitants that live in a specific municipality in 2017; (2) in Column 3, Share refugees in 2004 = migrants from countries of origin of refugees every 1,000 inhabitants that live in a specific municipality in 2004; (3) in Columns 4 and 5, Open SPRAR center last tender = 1 if municipality  $i$  opens a refugee center during the last tender available in the data. Time invariant controls: share of graduate, share elderly (>65), share children (<5), log of income per capita, number of firms per capita, population density, area, altitude, latitude, longitude, unemployment rate, dummy variable for first level reception centers, number no-profit organizations per capita and daily newspapers circulation. Time variant controls: population, municipal share of migrants, dummy variable for past participation to SPRAR, dummy female mayor, age mayor, dummy unemployed mayor, political experience mayor, dummy graduate mayor, dummy left mayor, dummy independent mayor, dummy term limit and dummy for early interruption mandate. Local market areas (LMAs) FE included in all columns. Robust standard errors clustered at LMA level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\* and at the 1% level by \*\*\*.

(2016). First, we get a municipality-specific estimate of the magnitude of the effect of electoral incentives on refugee reception for tenders 1–8 (i.e. we exclude the last two tenders 9 and 10, see [Table B1 in Supplementary Appendix B](#)). The estimate obtained,  $\hat{\delta}_i$ , measures the magnitude of the effect of electoral incentives on the probability of not bidding for the opening of a SPRAR center for municipality  $i$  during tenders 1–8. This parameter has a mean of 0.009 and a standard deviation of 0.12, where positive values refer to municipalities in which electoral incentives negatively impact the probability of bidding for a SPRAR. Conversely, negative values refer to municipalities with a positive impact.

Second, we estimate the correlation between  $\hat{\delta}_i$  and the municipal share of refugees every 1,000 inhabitants measured in 2017<sup>17</sup> and the probability that a mayor will open a

17 We calculate the 2017 municipal share of refugees as the share of migrants over the total municipal population considering the migrants from countries asylum seekers and refugees are more likely to arrive. We used data on legal migrants from Istat and obtained information about the more likely countries of origin of asylum seekers and refugees from the ‘Atlante SPRAR’. For the municipalities for which the 2017 information is missing, we have used the 2016 observation.

**Table 4.** Correlation magnitude electoral incentives and heterogeneity dimensions

	(1)	(2)	(3)	(4)
Outcome	Political competition		Extreme-right voting	
Magnitude electoral incentives	-0.180** (0.073)	-0.152** (0.068)	0.262** (0.108)	0.082** (0.035)
Observations	6,860	6,860	6,860	6,860
R-squared	0.002	0.222	0.003	0.746
LMA FE	No	Yes	No	Yes
Time invariant controls	No	Yes	No	Yes
Time variant controls	No	Yes	No	Yes

*Notes:* All Italian municipalities. Variables in the table: (1) political competition is a dummy variable equal to 1 if the average municipal margin of victory is below the median. We use the average over municipal elections from 2001 to 2016; (2) extreme-right voting = 1 if the average vote share taken by extreme-right parties is above the median. We use the average over the European elections in 2004, 2009 and 2014; (3) magnitude electoral incentives = magnitude of the effect of electoral incentives on the probability of not opening a refugee center during the tenders in years 2005–2017. Time invariant controls: share of graduate, share elderly (>65), share children (<5), log of income per capita, number of firms per capita, population density, area, altitude, latitude, longitude, unemployment rate, dummy variable for first level reception centers, number no-profit organizations per capita and daily newspapers circulation. Time variant controls: population, municipal share of migrants, dummy variable for past participation to SPRAR, dummy female mayor, age mayor, dummy unemployed mayor, political experience mayor, dummy graduate mayor, dummy left mayor, dummy independent mayor, dummy term limit and dummy for early interruption mandate. LMAs FE included in even columns. Robust standard errors clustered at LMA level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\* and at the 1% level by \*\*\*.

SPRAR center during the last two tenders available (i.e. tenders 9 and 10).<sup>18</sup> We report the results in [Table 3](#).<sup>19</sup> Columns 1 and 2 indicate that a 10 percentage point increase in the intensity of the effect of electoral incentives in the past brings to a decrease in the share of refugees every 1000 inhabitants in 2017, with a reduction which is approximately 1.4% compared with the mean of the outcome variable. Column 3 shows no effect for the share of refugees every 1000 inhabitants measured in 2004, suggesting that this unbalanced reception was not in place in the past.<sup>20</sup> Columns 4 and 5 show that an increase of 10 percentage points in the intensity of the effect of electoral incentives in the past

18 Given that tender 10 was restricted only to municipalities that never participated in the SPRAR system in the past, we have kept both tenders 9 and 10 as the last available tenders.

19 The smaller number of observations in the tables in this section is because to run Equations (2) and (3) in [Supplementary Appendix E.1](#) we had to exclude municipalities for which we do not observe any final year of the electoral term in all the tenders 1–8. Besides, we lose observations for municipalities for which we could not recover data on the share of refugees every 1000 inhabitants or for which we do not have information on SPRAR tenders 9 and 10.

20 [Table E1 in Supplementary Appendix E](#) shows the results of a placebo test in which we use as the dependent variable the share of economic migrants, measured as the share of migrants from countries from which asylum seekers and refugees are less likely to arrive. We do not find any correlation between the magnitude of the effect of electoral incentives and this dependent variable.

decreases the probability of opening a refugee center during the last two tenders by 2.1 percentage points.<sup>21</sup>

This evidence suggests that the effect of electoral incentives can persist beyond the end of the term, given that municipalities in which electoral incentives affected refugee reception more strongly in the past host a smaller share of refugees in 2017 and have a lower probability of opening a refugee center in the last two tenders. Interestingly, [Table 4](#) shows that the magnitude of the effect of electoral incentive on refugee reception estimated taking into account all the tenders in the dataset negatively correlates with Political competition<sub>*i*</sub> and positively correlates with Extreme-right voting<sub>*i*</sub>. These correlations suggest that Political competition<sub>*i*</sub> contributes to reducing the imbalance in the medium run and Extreme-right voting<sub>*i*</sub> contributes to generating an unbalanced reception of refugees in the medium run.

## 7. Conclusion

The results of this article have a few messages and policy implications. First, the results indicate that the fear of losing popular support induces politicians to give up financial resources, which could benefit the local economy. However, the evidence on political competition suggests that introducing institutions and policies that foster political competition may offset this opportunistic behavior ([Barone et al., 2016](#)). Second, this article provides additional insights on whether local or national governments would better manage policies like immigration. [Gamalerio et al. \(2021b\)](#) and [Campo et al. \(2021\)](#) suggest that native voters better accept a decentralized policy like SPRAR once the refugee centers are operative, potentially leading to better integration of migrants. However, our article suggests that politicians' electoral incentives and local political hostility may complicate the development of such decentralized policies, with potential consequences for asylum seekers and refugees' integration. As described in [Supplementary Appendix B](#), these results are consistent with historically low participation in the SPRAR program. The results also suggest that local resistance to the opening of reception centers may need to be compensated with monetary benefits beyond grants that cover the costs of the reception. Effectively, as reported in [Supplementary Appendix B](#), this is what the Italian government has done to incentivize participation in the SPRAR program. Third, the suggestive evidence that the effect of electoral incentives can persist beyond the end of the electoral term and eventually lead to an unbalanced reception suggests that taking into account the political determinants of immigration policies is necessary to develop fair and effective asylum policies ([Thielemann et al., 2010](#); [Fernández-Huertas Moraga and Rapoport, 2014, 2015](#); [Dustmann et al., 2017](#)).

These results call for further research along different lines. First, this article focuses on the role of political competition and extreme-right voters. It would be interesting if future research could analyze which other factors shape immigration policies, focusing on the distinction between winners and losers of these policies. For example, [Gamalerio et al.](#)

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21 This result can be explained by the fact that participation in the SPRAR system during the last two tenders is positively correlated with participation in the past tenders, as shown in [Table E2 in Supplementary Appendix E](#). This evidence is consistent with the fact that exits from the SPRAR system are not frequent ([Figure E2 in Supplementary Appendix E](#)), and thus municipalities tend to remain in the system once they have entered it. Consequently, municipalities that did not open a SPRAR center in the past are less likely to open a center today.

(2021a) highlight how different social classes may be affected differently by immigration and how these different expectations affect the choices of governments about whether to implement more open immigration policies or not. Second, we think it would be interesting to study if the results of this article also apply to other local contexts different from Italian municipalities. Third, future research may consider analyzing whether the electoral behavior produced by Italian mayors also characterizes national governments, as suggested in Figure 1. On this line, Fasani and Frattini (2019) provide evidence of a political cycle in enforcing EU border control policies by part of Frontex (European Border and Coast Guard Agency). Finally, our analysis indicates that Italian municipal governments give up fiscal resources to avoid losing electoral support. However, we do not provide direct evidence of the economic costs of this behavior. Future research may consider estimating these costs directly. On this line, Gamalerio et al. (2021b) provide evidence on how SPRAR centers positively affect ‘compositional amenities’ and population growth, suggesting that the economic costs of not opening a SPRAR may go beyond the simple giving up of the fiscal grants. In contrast, Batut and Schneider-Strawczynski (2022) show that opening small reception centers in France negatively affected local economic activity. The results of this article, combined with the ones by Gamalerio et al. (2021b) and Batut and Schneider-Strawczynski (2022), call for future research on the socio-economic consequences of refugees’ reception.

## Supplementary material

Supplementary data for this article are available at *Journal of Economic Geography* online.

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