



When the neighbors are watching: Immigrant integration policy and housing wealth[☆]

Mario F. Carillo^{a,b}, Lavinia Piemontese^{c,*}, Francesco Flaviano Russo^{d,e}

^a *Universitat Autònoma de Barcelona, Spain*

^b *IPEG, Canada*

^c *Università di Bologna, Italy*

^d *Università Federico II, Italy*

^e *CSEF, Italy*

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ABSTRACT

We hypothesize and establish empirically that the effects of migrant integration policies on housing wealth are influenced by public attention to immigration. Using a unique policy converting temporary refugee reception centers into integration facilities, we conduct an event study analysis. Our results show that timing matters: implementation during times of high public attention to immigration reduces housing wealth, while implementation during times of low attention has no impact. These findings suggest that the backlash effect of integration policies estimated in the literature may be driven by public perceptions of migration crises.

1. Introduction

Wars, pandemics, and climatic shocks generate humanitarian emergencies with profound effects on human displacement. These so-called “Migration crises” have been shown to have significant adverse economic and political consequences on receiving countries (Bratti et al., 2020; Campo et al., 2024; Gamalerio and Negri, 2023; Gamalerio et al., 2023). The burden these crises place on host countries requires responsive migration policies that move from emergency relief to long-term integration strategies as the migration peaks stabilize. Understanding their socioeconomic effects is therefore important, yet little is known about how such policies operate, or how their impact changes as crises evolve. This paper casts light on these issues.

We exploit a unique policy intervention, implemented in the province of Bologna, northern Italy, which changed the purpose of preexisting refugee reception centers. These structures provided basic assistance to refugees, such as food and shelter, designed to hosting them temporarily. The intervention transformed them by incorporating

a range of integration services, including language and vocational courses, designed to facilitate social integration. The staggered roll-out of the implementation provides useful variation to identify the economic effects of integration policies. In addition, part of these conversions coincided with unexpected migrant waves to Italy’s southern coast, far from the study area, which significantly affected citizens’ perception of the crisis. We investigate how the interaction between local integration policy, as captured by these conversions, and citizens’ awareness of the migration crisis affected housing wealth.

Our setting offers three main advantages. First, focusing on the conversion to integration centers allows us to disentangle the effect of the integration policy from changes in immigrants inflows and from any potential confounding effect associated with centers’ establishment. Second, migrants hosted in the centers had the refugees legal status without the right to work, allowing us to estimate the policy’s effect with limited interference from labor market impacts. Third, the staggered timing of the conversions allows us to determine whether the timing of the policy implementation plays a significant role.

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* Corresponding author.

E-mail address: lavinia.piemontese@unibocconi.it (L. Piemontese).

We compiled a dataset on the centers' opening and conversion dates (2014–2020), along with their exact locations, and merged this with newly collected monthly data on real estate sale and rent prices, and housing market advertisements at the neighborhood level, which we use as a proxy of housing wealth. Since conversions during migration crises may occur in neighborhoods exhibiting systematic differences or different trends, we study the effect of conversions in an event-study framework with a difference-in-differences specification, interacting the timing of major waves of refugee arrivals in Italy with the timing of the conversion.

Our key finding reveals that the effects of converting centers from temporary reception to integration centers are strictly linked to the timing of the policy implementation. In particular, the integration policy has adverse effects on real estate prices only if the conversion occurs during a period characterized by high attention to migration, due to major refugee arrivals. We estimate an average loss of 14,000 euros per apartment. This corresponds to two-thirds of the median annual salary and more than half of average homeowner income (*Agenzia delle Entrate, Dipartimento delle Finanze MEF, 2023*). Consistent with this finding, we observe an average decrease of about 1000 euros per year in rental prices. Conversely, we find no effect if the conversions are implemented during periods of low attention. This evidence suggests that integration policies for immigrants do not necessarily have negative effects; rather, their impact depends on the timing of their implementation. Our results are robust across different estimation strategies, including the TWFE, the multiple treatment model of *De Chaisemartin and D'Haultfœuille (2024)*, the stacked DID estimator *Cengiz et al. (2019)*, and a two-stage Difference-in-Differences model (*Gardner, 2022*).

Next, we explore the mechanisms underlying these findings. As we find no change in the number of sale or rental advertisements, we rule out shifts in housing supply. Moreover, because the conversions concern already established centers and have no effect on the number of migrants hosted, the observed impacts are likely driven by changes in non-migrants' housing demand. We argue that this reflects worsening perceptions of immigration. Supporting this view, we find similar negative effects on real estate prices during periods of heightened attention to immigration, proxied by sharp peaks in Google Trends searches. We also document the same negative effects when conversions coincide with periods of increased terrorist attacks by foreigners in European countries. In addition, heterogeneity analyses reveal stronger effects in municipalities with greater far-right support and larger immigrant shares. These patterns suggest that demand shifts partly reflect existing or resurgent anti-immigrant sentiment, amplified by the migration crisis and related concerns. We find no consistent evidence of heterogeneous effects by unemployment levels, providing limited support for a labor market competition channel. Overall, the results underscore the importance of policy timing to avoid reinforcing cycles in which concerns about the migration crisis exacerbate adverse economic effects, which in turn may fuel anti-immigrant sentiment, especially in areas with larger immigrant populations.

Our paper contributes to three branches of literature. First, while a recent set of works has focused on the economic consequences of immigrants inflows in hosting countries due to dispersal policies, we highlight the role of integration policies without changes in local immigrant presence. In this literature, a first set of works explores real estate markets as outcomes. For instance, *Hennig (2021)*, *Buccione and Zurla (2022)*, and *Brulhart et al. (2023)* find negative effects of the opening of refugees reception centers of real estate prices, respectively in the city of Berlin, in Italy and in Switzerland. This negative effect is driven by the worsening of neighborhood amenities and the increase of anti-immigrant sentiments. In a related contribution, *Sà (2015)* finds a negative effect of immigration on house prices in the UK, driven by the displacement of high-income residents. *Accetturo et al. (2014)* find comparable results at the neighborhood level for 20 large Italian cities. Conversely, *Ottaviano and Peri (2006)* and *Saiz (2007)*

find higher house prices as a result of a bigger immigrants' share, in the US context, and *Sanchis-Guarner (2019)* for Spain. This is due to the increased city diversity that has a positive effect on natives' productivity. The evidence for Colombia described by *Depretis-Chauvin and Santos (2018)* is instead mixed: an inflow of forced migrants due to the *marcos* war determined, in the host cities, both an increase of rental prices at the bottom of the distribution due to the increased demand, and a decrease at the top due to a worsening of crime and public good provision. A second set of works focuses on labor market performance as outcome, include *Edin et al. (2003, 2004)* for Sweden, and *Damm (2009)* for Denmark. We contribute to this literature in two ways. By focusing on the conversion to integration centers, rather than on the opening of reception centers, our research identifies the effect of integration policy without additional effects associated with changes in the supply of migrant or centers. Moreover, the staggered introduction allows estimating the economic effects of integration policy in various scenarios according to migrants inflows and citizens' awareness.

Second, a growing body of literature examines the effects of immigrants' perceptions. While this research primarily focuses on perceptions arising from local variations in the presence of immigrants, we emphasize changes in attention absent local fluctuations in immigrant presence. For instance, *Ajzenman et al. (2023)* find that, even if there is no effect of immigration on crime, immigration raises crime-related concerns, which induces higher investments in home security. And *Hangartner et al. (2019)*, show that refugee arrival enhances hostility towards migrants. Other works studying the effect of proximity to immigrants on immigrants' perception include *Dustmann and Preston (2001)*, *Mayda (2006)*, *Facchini and Mayda (2009)*, *Card et al. (2012)*, *Bansak et al. (2016)*, *Slotwinski and Stutzer (2019)*. Our findings contribute to these works by highlighting that the economic effects of integration policies effects can be affected in important ways by attention to migration as driven by the migratory crisis, rather than by proximity to immigrants. This result is consistent with works emphasizing the importance of attention in other contexts (*Durante and Zuravskaya, 2018*), and its sensitivity to media bias (*Couttenier et al., 2024; Enikolopov and Petrova, 2015*). Third, our findings speak to studies on the political effects of immigration policies. These include, among others, *Dustmann et al. (2019)* for Denmark, and *Campo et al. (2024)* and *Berti Ceroni et al. (2024)* for Italy. *Dahlberg et al. (2012)* explores the effect of dispersal policies on preferences for redistribution. *Dipoppa (2025)* emphasizes that, while many immigration policies have been shown to induce natives' backlash, those highlighting migrants' vulnerability can foster solidarity. Our findings that the adverse economic effect of integration can be explained by extreme public perceptions of the migration crisis without changes in local migrants may inform on a novel mechanism through which native's backlash may occur.

2. Institutional background

Our analysis relies on the interplay of two policies: a national dispersal policy and a local integration policy unique to the Bologna province.

National Dispersal Policy. The dispersal policy was designed in late 2014 by the Ministry of the Interior to manage the steady arrival of refugees, escaping in large numbers from sub-Saharan and Middle East countries. The vast majority of refugees, as of 2017, was hosted in reception centers managed by local NGOs under the supervision of prefectures and funded by the central government.¹ These centers were intended to be temporary facilities designed to cope with the emergency, so they provided only basic services. Beyond food and shelter, their guests were helped in the application to obtain refugee status, a process that could be as long as two years, and in which

¹ These centers are called CAS, i.e., *Centro Accoglienza Temporanea*.

the applicant is not allowed to find a job or to move out. In this context, the Ministry of the Interior assigned each Italian prefecture a number of refugees proportionate to the province's population, along with the necessary funds. The prefecture was responsible for designing the reception system: Bologna structured it by accommodating little groups of refugees in small-capacity facilities, often rented apartments.

One potential concern is that these facilities may be located in areas with specific real estate price dynamics. Although our identification strategy does not rely on the exogenous location of reception centers, two important points are worth noting. First, interviews with institutional actors involved in the process reveal that the NGO responsible for establishing and managing the centers often had to act on very short notice, typically with only a few days to identify and secure a suitable location. Consequently, the locations of reception centers were dispersed across the entire province. Second, previous studies highlight the quasi-random allocation of reception centers, as shown by [Campo et al. \(2024\)](#) for Italy as a whole and by [Berti Ceroni et al. \(2024\)](#) for Bologna specifically. As we explain below, consistent with this evidence, we find no systematic differences in real estate prices between areas with and without reception centers.

Local integration policy. In 2017, a consortium of municipalities in the province of Bologna, approximately 80% of the municipalities, designed an integration policy aimed at improving the services offered to refugees. The policy centered on the gradual conversion of temporary reception centers into permanent ones under the *Sistema di Accoglienza e Integrazione* (SAI). These centers were equipped to provide language courses, job training, and other integration-focused services, in addition to the basic provisions already offered in the reception facilities such as accommodation, food, and legal aid. The policy was officially approved in September 2017, implemented from January 2018, and suspended in December 2019 following a national immigration law reform. The roll-out of this policy occurred within the framework of the national dispersal policy described above, which introduced province quotas for hosted migrants and a safeguard clause protecting compliant municipalities from further emergency placements.

Locally, the conversion process was coordinated by the Municipality of Bologna, the Public Social Services Agency (PSSA), and the Prefecture. Eligible centers, typically larger and better connected facilities, were prioritized, while small or temporary structures such as hotels were excluded. NGOs managing the centers remained the same, and so did the staff and most of the residents, ensuring continuity in operations. The change was administrative in nature, affecting the center's status, funding channel, and service standards. In total, 49 of the 126 existing reception centers were converted during the policy window.

Beyond its service-oriented goals, the integration policy also emphasized transparency and engagement with the host community. Art. 6 of the operating guidelines mandated coordination between municipalities, NGOs, and local associations to promote public awareness and foster a culture of hospitality.² Consistent with this, each center had a permanent staff member from the managing NGO on-site daily, serving as a point of reference for neighbors, landlords, and local organizations. This person maintained continuous dialogue with the surrounding community. As confirmed through interviews with NGO managers and municipal officials, any center's change from reception to integration was promptly communicated to the residents of the same and neighboring apartment blocks. This was done through both organized outreach and informal interactions, making the policy change visible and salient to the local population.

² The official document outlining the operating guidelines of the policy is available at: [Operational Agreement for the Management of the Metropolitan SPRAR Project of Bologna](#).

3. Data, descriptive statistics, and balancing tests

We assemble a novel monthly dataset at the level of an *area*, corresponding to a fine geographical partition of the territory of a municipality. In the province of Bologna, there is a total of 293 areas, located in 55 municipalities. For the city of Bologna itself, we have 34 areas. We combine data on real estate prices, hosting centers location and characteristics (reception or integration), data on refugees sea arrivals in the country, Google Trends national searches data, and information on terrorist attacks perpetuated by foreigners in other EU countries. In our main regressions, we focus on the period starting in January 2014 and ending in December 2020. We use information related to years 2012–2013 to compute descriptive statistics related to the pre-migration crisis period (see [Tables 1](#) and [A.1](#)).³

Real estate prices. We gather residential real estate prices for 2012–2020 from Immobiliare.it, the largest Italian real estate platform (7.8 million monthly web ad views; 2.6 million app users).⁴ Prices are monthly averages at the area level, we use weighted average prices across property types (apartments, penthouses, villas, buildings, rural dwellings), with weights given by the number of ads. Not all types are available in every area or month, thus, prices of apartments drive most of our results.

The median sale price per square meter, across all micro-areas and years, is 1687 euros (average 1782 euros). For what concerns monthly rents, the median is 7.5 euros per square meter per month (average 7.7 euros). Given an average house size of 93 square meters (computed over all residential real estate in the study area), we are considering houses that sell for a median posted price around 157(k) euros and that are rented for a median of 700 euros per month.

Reception and Integration centers. We collect information on the universe of reception and integration centers located in the province of Bologna from the prefecture and from the PSSA of Bologna. We observe the exact address of every center, so we can locate them within the area. We have a total of 126 reception centers, covering a total of 75 micro-areas, out of which 49, located in 27 areas, were converted into integration centers by the end of the study period. Moreover, we have information about the opening date, the closing date (if any) and, in case of conversion, the conversion date to integration center. Finally, we observe the maximum capacity of each center, which coincided with the number of hosted immigrants in any given month.

Attention to immigration. We explore three proxies of attention to immigration. In our baseline analysis, we use the monthly number of people arriving to the country—not to our study area—by sea. These events are symbolic of broader migration challenges and tend to provoke stronger public reactions. Moreover, spikes in sea arrivals are often framed within a “crisis” narrative, further heightening their salience in the public debate. As such, fluctuations in sea arrivals can be seen as a reliable indicator of heightened attention toward migration issues. This variable, provided by the Ministry of the Interior, exhibits a median number of arrivals of 1.9k and average of 5k over the years of the conversions (2017–20). We compute a binary indicator taking value one if the number of arrivals in a given month is above the median.

³ Replication materials, including the data and all code required to reproduce the results in this paper, are available in [Carillo et al. \(2026\)](#).

⁴ Our data are “asking” prices, which are generally higher than transaction prices. Official registers from the Ministry of the Interior provide transaction prices only at coarser time and geographical scales. Thus, while our data may overstate levels, any bias would work against us: actual transaction prices for given asking prices are lower, making our estimates a conservative lower bound.

As a second proxy, we use the monthly national-level Google searches of the topic “immigration”.⁵ We capture the peak of attention by using a dummy taking value one if searches are above the 90th percentile. By analyzing spikes in arrivals and Google searches, we effectively capture the *increase* in attention, rather than its level. This increase may exhibit some degree of persistence, which is consistent with our empirical findings.

Finally, as a third proxy, we use the number of terrorist attacks by foreigners across European countries from the Global Terrorist Database,⁶ coding again a dummy in case the attacks are above the 90th percentile. The idea is that terrorist attacks rise concerns from the public that might trigger strong emotional responses.

Balance tests. We test for pre-determined differences in outcome variables across three groups of areas: (i) those with versus without a reception center; (ii) among areas with a reception center, those that experienced at least one conversion versus those that did not; (iii) among areas with a conversion, between those where at least one occurred during a period of high immigration arrivals and those where it did not. Our identification strategy relies on the variation captured in the third comparison. Our outcomes of interest are sale price, rent price, number of advertisements, number of immigrants (all measured in logs), and number of reception centers. These serve as the dependent variables in simple regression models, where the sole regressor is an indicator of the group classifications of areas mentioned above. Standard errors are clustered at the municipality level. We run these regressions over two distinct time periods: the pre-crisis period (2012–13) and the pre-conversion period (2014–17).

Table 1 presents the results. When comparing areas with at least one reception center to those without (group i) during both the pre-crisis and pre-conversion periods, the only statistical difference pertains to the number of advertisements (Columns 1–3, Panel A of Table 1). Compatibly with the fact that NGOs established reception centers in very short time spans, this evidence suggests that reception centers were opened in areas with greater availability of real estates. Conversely, Panel B suggests that areas with at least one conversion (group ii) tend to have higher prices and more ads than those without any conversions (Columns 1–3). This suggests that conversions were not disproportionately targeted toward poorer or more disadvantaged neighborhoods. While this implies some cross-sectional differences, our identification strategy does not rely on random assignment of conversions across all areas. It rather exploits within-area variation in the timing of conversions and their interaction with national immigration salience, not the occurrence of conversion per se. Importantly, when we focus on areas that experienced at least one conversion (group iii), we find no significant differences between those where the conversion occurred during high-arrivals periods and those where it did not (Panel C, Columns 1–3). Overall, this evidence supports the internal validity of our empirical setting.

4. Empirical analysis

4.1. Identification strategy

Our identification exploits two sources of variation: (i) the exact timing of each center’s conversion from reception to integration, and (ii) changes in attention to immigration, which, in our baseline specification, are proxied by refugee arrivals to Italy, which is largely

⁵ According to Google trend documentation, “topics (instead of exact keywords) are generally considered to be more reliable for Google Trends data. They pull in the exact phrase as well as misspellings and acronyms, and cover all languages”.

⁶ Data source: START (National Consortium for the Study of Terrorism and Responses to Terrorism). *Global Terrorism Database, 1970–2020* [Data file]. 2022. <https://www.start.umd.edu/data-tools/GTD>.

driven by exogenous push factors in countries of origin as well as sea and weather conditions. Our identification relies on the *interaction* between these two components, i.e., conversion timing and high/low sea arrivals. Specifically, we compare the housing market response to conversions that occurred during periods of high sea arrivals of refugees with those that occurred during periods of lower arrivals.

In what follows, we argue that while each component may not be fully exogenous on its own, their interaction provides a plausibly exogenous source of variation with respect to local housing market dynamics. First, the timing of conversions was driven by administrative procedures, not local migration inflows or real estate trends. As explained in Section 2, only eligible centers (usually large and well connected) could submit a new service plan, which local social services then approved sequentially as part of their broader responsibilities. As a result, conversion dates are unlikely to be correlated to migration pressures or housing trends.

To support this claim empirically, we conduct several diagnostic tests. We first examine whether conversion timing is systematically related to refugee sea arrivals. Correlation coefficients between the conversion dummy and both (a) a binary high-arrivals indicator and (b) the continuous arrivals series are indistinguishable from zero.⁷ Second, as shown in Table 1 (Panel C), among areas that did experience a conversion, those converted during high-arrivals periods are statistically indistinguishable from those converted during low-attention periods across a range of characteristics, including housing prices, number of ads, and migrant presence.

4.2. Baseline model

Our empirical model is designed to evaluate whether the conversion of reception centers into permanent integration centers influences housing wealth according to differences in the attention to immigration as determined by refugees inflows in the country. We employ the following event-study estimation strategy with a two-way fixed effects (TWFE) structure:

$$Y_{it} = \alpha + \sum_{j=-10}^{10} \beta_j \text{conversion}_{it+j} + \sum_{j=-10}^{10} \gamma_j (\text{conversion}_{it+j} \times \text{high arrivals}_t) + \delta \text{center}_{it} + \eta (\text{center}_{it} \times \text{high arrivals}_t) + \theta_i + \theta_t + \theta_{qt} + \sigma t_n + \epsilon_{it} \tag{1}$$

where Y_{it} denotes the outcome of interest, that is the logarithm of the average house sale price or rental in area i at month t . The variable conversion_{it} is a dummy variable equaling one if, in area i at time t , at least one conversion to a reception center took place. The variable high arrivals_t is a dummy equal to one when the number of sea arrivals at time t exceeded the median (computed from 2017, the year of implementation of the first conversion policy). Both conversion_{it+j} and its interaction with high arrivals, are interacted with time-period dummies, referring to 10 periods before and 10 periods after the conversion, allowing us to test for differential pre-trends. We also control for the effect of opening a reception center in area i in month t , denoted as center_{it} , and its interaction with high attention $_t$. Finally, the regression includes a comprehensive set of fixed effects at the area, month, and quarter-neighborhood levels, denoted as θ_i , θ_t , and θ_{qt} , respectively, along with neighborhood-specific time trends t_n .⁸ Standard errors are clustered at the municipal level.

⁷ In our sample, the conversion indicator is essentially uncorrelated with refugee arrivals. Specifically, the correlation with a high-arrivals indicator is $\rho = -0.000$ (s.e. = 0.005), while the correlation with the continuous arrivals measure is $\rho = 0.000$ (s.e. < 10^{-6}); both estimates are statistically indistinguishable from zero.

⁸ A neighborhood n encompasses several areas i .

The parameter of interest is γ_j , the coefficient on the interaction term between conversion $_{it}$ and high arrivals $_{it}$, which identifies the impact of conversions that occurred during periods of high sea arrivals relative to the effect of conversions occurred in low-arrivals periods. This specification enables us to estimate the effect of the conversion to integration centers according to the attention of immigration controlling for various potential confounding effects.

To validate the TWFE results, consistent with the recent literature, we estimate the model with various methods, including the stacked difference-in-difference model (Cengiz et al., 2019), the two-stage difference-in-difference model (Gardner, 2022), and the multiple treatment model (De Chaisemartin and D'Haultfœuille, 2024).

4.3. Results

Event Study. Fig. 1 presents the results from the estimation of Eq. (1). Irrespective of the chosen estimation method, we observe a decrease in real estate sale prices when the conversions to integration centers occur during periods of high arrivals (top panel of Fig. 1a), while no effect is evident in cases of conversion during periods with low arrivals (bottom panel of Fig. 1a). The effect peaks approximately 4 months after the conversion and, consistent with the identification assumptions, does not exhibit differential pre-trends. As our model includes time fixed effects and the outcome is in logarithms, the estimates indicate differential growth rates of treated versus non-treated units. Consequently, while the effects on growth are absorbed six months or more after the conversion, the overall effect on the levels of our outcome persists. This result can be explained by the fact that the spike in arrivals induced an increase in public attention that persists in subsequent months. Despite the fact that rental prices are relatively more short-term oriented and thus more volatile, we find, reassuringly, similar effects. Specifically, we document a 10%-decrease of rent prices, peaking after about 4 months (Fig. 1b).

Overall, our findings show a sustained negative effect due to the conversion of temporary to permanent immigrant reception centers in times of high refugee arrivals. The relevance of our findings is underscored by the fact that conversions often entail enhanced integration services, which aim to mitigate the socioeconomic impact of immigrants. Indeed, the absence of an effect in cases of conversions made in periods of low arrivals supports this interpretation.⁹

Difference in Differences. To facilitate the interpretation of our results and get a sense of their average magnitude, we estimate the average post-treatment effect using a difference-in-differences specification.¹⁰ The results are summarized in Table 2. Column 1 indicates that the treatment, on average, led to approximately a 6% decrease in sale prices, which translates to a decrease of about 150 euros per square meter. For an average of 93 square meters per house, this amounts to 14(k) euros per apartment, which corresponds to approximately 2/3 of the median annual salary in Italy and more than one half of the average salary of Italian home owners. Column 2 of Table 2 shows limited effects on the number of ads, which serves as a proxy for the supply of housing, in turn suggesting that demand channel is driving the results (see Section 4.4). Column 3 reveals a negative and statistically significant effect of the conversion in periods of high arrivals on rent prices, of approximately 4%. Given a median rent of

⁹ Estimating the impact of attention peaks that occur much later than conversions, and that would interact with the specific services offered by the centers, requires a distinct identification strategy and data. Thus, this analysis falls outside the scope of the paper, which focuses on the conversion policy across various attention scenarios.

¹⁰ Specifically, we estimate the model: $Y_{it} = \alpha + \beta \text{PostConversion}_{it} + \gamma(\text{PostConversion}_{it} \times \text{high arrivals}_{it}) + \delta \text{center}_{it} + \eta(\text{center}_{it} \times \text{high attention}_{it}) + \theta_i + \theta_t + \theta_{qt} + \sigma \tau_n + \varepsilon_{it}$. Where PostConversion is a dummy variable taking the value one from the time a conversion occurs.

750 euros per month, the effect amounts to roughly 1000 euros per year, which is again substantial, especially because residential leases typically last 8 years and are seldom renegotiated. Finally, Column 4 of Table 2 indicates not significant and minimal effects on the number of migrants in the centers, suggesting that our estimates are not influenced by changes in the number of individuals hosted in the facilities due to conversions during times of high attention.

Robustness checks. Table A.2 reports a series of sensitivity checks. Column (1) presents the most parsimonious specification with only area and month fixed effects, Column (2) adds macroarea-specific linear time trends, and Column (3) reproduces the baseline specification (Table 2, column 1) for reference. The estimated average effect is economically sizable, with both the magnitude and precision improving once macroarea-specific trends are included. This pattern suggests that differential macro-area price trends attenuate the estimated treatment effect. Accounting for macroarea-specific linear trends therefore yields larger and more precise estimates. The addition of macroarea-quarter fixed effects has little impact on the point estimates. Column (4) introduces an interaction between initial price levels (Jan. 2014) and a post-treatment indicator to allow for heterogeneous dynamics across areas with different pre-treatment conditions. This specification addresses concerns about mean reversion, as conversions were more common in initially high-priced areas (Table 1, Panel B) that might have experienced natural price declines unrelated to the policy. The estimates remain robust, and, if anything, slightly stronger, suggesting that any reversion would bias the results downward (Table A.2, column 4).

In all specifications, we assess robustness to spatial correlation by estimating Conley (1999) standard errors with a conservative 35 km cutoff, approximately equal to the study area's radius. As reported in Table A.2, these standard errors (shown in square brackets) are very similar to, and often smaller than, those clustered at the municipality level, suggesting that spatial correlation is unlikely to drive our results.

4.4. Mechanisms and interpretation

We now turn to an exploration of the mechanisms, distinguishing between the supply and the demand side. For what concerns supply-side mechanisms, two important pieces of evidence suggest that changes in housing supply do not drive our results. First, as the conversion policy repurposed preexisting centers, rather than opening new ones, there is no mechanical effect on housing supply. Second, we find no significant increase in the number of real estate advertisements and, in fact, in some specifications, the number of ads slightly decreases (column 2 of Table 2).

On the demand side, we find no significant change in the number of migrants after conversions (Table 2, column 4), suggesting that increased housing market pressure from NGOs is unlikely to be the operative mechanism. Instead, shifts in native residents' housing demand remain the most plausible channel.

Real estate prices decrease when conversions take place in period of high immigrants inflows, which points towards a "negative perception" channel. Locals may react negatively to a long-term integration commitment (proxied by the conversion), especially in sensitive times when the immigrants inflow increases, perhaps because of the fear of a further inflow of new immigrants in the near future in that area as a result of migratory chains. In addition, xenophobic sentiments might be reactivated during periods of high attention to immigration triggered by new arrivals.

We next deepen our analysis of the perception mechanism by considering alternative indicators beyond arrivals. First, if peaks of the migration crisis heightened concerns, individuals may have increased their online searches about immigration. We thus explore Google Trends searches for the topic "immigration". We capture the peak in attention with a dummy taking value one if Google Trends searches for

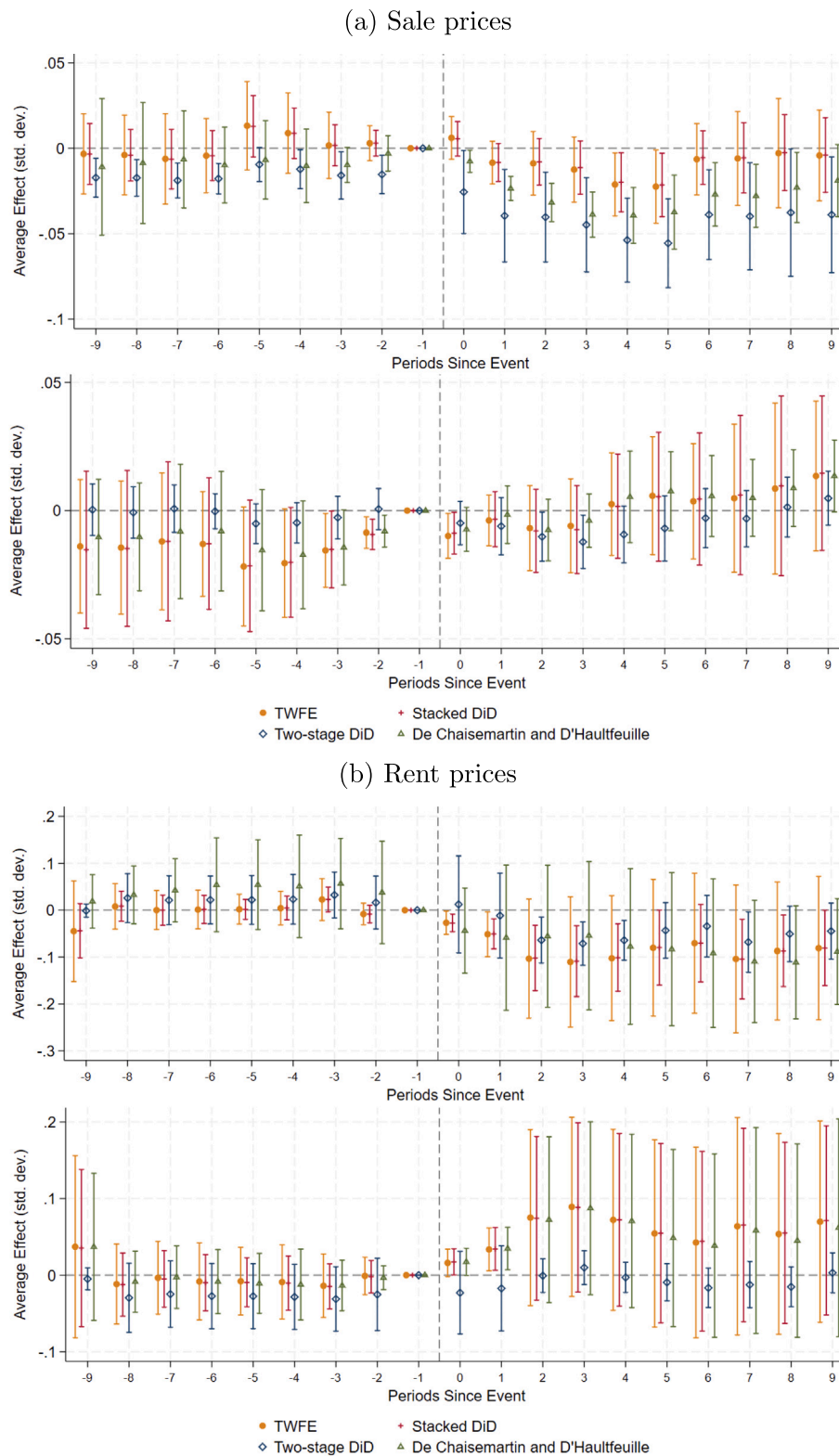


Fig. 1. Housing wealth, integration centers, and the attention to immigration: Event study.

Notes: Results for the estimation of Eq. (1). The top diagram of each sub-figure plots the coefficients of the interaction terms between conversion and high-arrivals, i.e., γ_j . The bottom sub-figure plots the coefficients of variable conversion, i.e., β_j , indicating the average effect of conversions made in low-arrivals periods. We consider four different methods: (i) TWFE specification in yellow, (ii) Two-stage DiD in blue, (iii) Stacked DiD in red, (iv) De Chaisemartin and D'Haultfeuille method in green. In Panel (a) the dependent variable is real estates sale price (in logs), while in Panel (b) the dependent variable is real estates rent price (in logs). All coefficients are plotted along with their 95% confidence bands.

Table 1
Balance tests.

	(1) (log-) price sale	(2) (log-) price rent	(3) (log-) n. ads	(4) (log-) n. migrants	(5) n. centers
Panel A: With vs. without centers					
pre-crisis (2012–2013)	0.014 (0.031)	0.027 (0.020)	1.206*** (0.240)		
pre-conversion (2014–2017)	0.045 (0.030)	0.028 (0.022)	1.156*** (0.231)		
Panel B: With vs. without conversions (at least one center)					
pre-crisis (2012–2013)	0.333*** (0.102)	0.186** (0.069)	1.908*** (0.476)		
pre-conversion (2014–2017)	0.323*** (0.109)	0.209** (0.085)	1.690*** (0.437)	0.535 (0.416)	0.734* (0.432)
Panel C: Conversion in high vs. low arrivals (at least one conversion)					
pre-crisis (2012–2013)	0.061 (0.091)	0.038 (0.048)	0.249 (0.494)		
pre-conversion (2014–2017)	0.076 (0.088)	0.037 (0.060)	0.113 (0.447)	0.349 (0.306)	0.110 (0.217)

Notes: Balance tests across three groups of areas: (i) areas with vs. without reception centers (Panel A); (ii) areas with vs. without conversions, conditional on having at least a reception center (Panel B); and (iii) areas with conversions during high vs. low sea arrivals periods, conditional of having at least one reception center converted into an integration center (Panel C). Columns (1) and (2) include as dependent variable sale and rent prices respectively (both in logarithmic form), Column (3) uses as dependent variable the logarithm of the total number of sale and rent advertisements published in the platform Immobiliare.it. Column (4) uses as dependent variable the logarithm of the migrants hosted in reception centers, and Column (5) uses number of reception centers. Regressions cover pre-crisis, i.e., years 2012–2013, and pre-conversion periods, i.e., years 2014–2017. OLS estimates with standard errors clustered at the municipality level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 2
Difference-in-differences specification.

	(1) (log-) Sale Price	(2) (log-) 1 + N. Ads	(3) (log-) Rent Price	(4) (log-) 1 + N. Migrants
Conversion × High Arrivals	−0.0559*** [0.015]	−0.0458 [0.080]	−0.0364*** [0.014]	−0.0425 [0.091]
Observations	24,612	24,612	24,612	24,612
Adjusted R-squared	0.555	0.448	0.379	0.933
Mean DV	7.362	3.35	2.02	.534
Number of Municipalities	55	55	55	55
Number of Areas	293	293	293	293

Notes: Results of the estimation of $Y_{it} = \alpha + \beta \text{PostConversion}_{it} + \gamma (\text{PostConversion}_{it} \times \text{high arrivals}_t) + \delta \text{center}_{it} + \eta (\text{center}_{it} \times \text{high arrivals}_t) + \theta_t + \theta_t + \theta_{qt} + \sigma_t + \epsilon_{it}$. All regressions include fixed effects at the area, month, and quarter-neighborhood levels, and neighborhood-specific time trends. The sample is related to the period 2017–2020. Column (1) and (3) use as dependent variable sale and rent prices respectively (both in logarithmic form). Column (2) uses as dependent variable the logarithm of the total number of sale and rent advertisements published in the platform Immobiliare.it. Column (4) uses as dependent variable the logarithm of the migrants hosted in reception centers. OLS estimates with standard errors clustered at the municipality level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 3
Mechanisms analysis: Immigration perception.

	(1) (log) Sale Price	(2) (log) 1 + N. Ads	(3) (log) Rent Price	(4) (log) 1 + N. Migrants
Panel A: High salience based on Google Trends (Immigration)				
Conversion × High Google Trends (Immigration)	−0.0563*** [0.012]	−0.0865 [0.100]	−0.0257** [0.010]	0.0031 [0.108]
Observations	24,612	24,612	24,612	24,612
Adjusted R-squared	0.554	0.448	0.379	0.933
Panel B: High salience based on Terrorist Attacks by Foreigners				
Conversion × High Terrorist Attacks by Foreigners	−0.0092*** [0.003]	−0.0450*** [0.012]	−0.0074 [0.005]	0.0366 [0.044]
Observations	24,612	24,612	24,612	24,612
Adjusted R-squared	0.554	0.448	0.379	0.933

Notes: Results of the estimation of $Y_{it} = \alpha + \beta \text{PostConversion}_{it} + \gamma (\text{PostConversion}_{it} \times \text{high perception}_t) + \delta \text{center}_{it} + \eta (\text{center}_{it} \times \text{high perception}_t) + \theta_t + \theta_t + \theta_{qt} + \sigma_t + \epsilon_{it}$. All regressions include fixed effects at the area, month, and quarter-neighborhood levels, and neighborhood-specific time trends. The sample is related to the period 2017–2020. Column (1) and (3) use as dependent variable sale and rent prices respectively (both in logarithmic form). Column (2) uses as dependent variable the logarithm of the total number of sale and rent advertisements published in the platform Immobiliare.it. Column (4) uses as dependent variable the logarithm of the migrants hosted in reception centers. In Panel A, high perception, is a dummy that takes a value of one if Google Trends searches for the topic “immigration” in month t exceed the 90th percentile. In panel B, high perception, is a dummy equal to one if, in month t , the number of terrorist attacks by foreigners across Europe exceeds the 90th percentile. OLS estimates with standard errors clustered at the municipality level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4
Heterogeneity analysis.

	(1) high	(2) Unemployment low	(3) high	(4) Immigrant's share low	(5) high	(6) Far-right support low
Panel A (log-) Sale Price						
Conversion × High Arrivals	-0.0585*** (0.0192)	-0.0547* (0.0329)	-0.0572*** (0.0161)	-0.0447 (0.0281)	-0.0584*** (0.0175)	-0.0548 (0.0346)
Observations	10 920	11 256	11 004	11 172	11 088	11 088
Adjusted R-squared	0.628	0.466	0.587	0.531	0.567	0.563
Panel B (log-) Rent Price						
Conversion × High Arrivals	-0.0628** (0.0302)	0.0322 (0.0252)	-0.0416** (0.0155)	0.0334** (0.0130)	-0.0496** (0.0209)	-0.0031 (0.0318)
Observations	10 920	11 256	11 004	11 172	11 088	11 088
Adjusted R-squared	0.434	0.193	0.458	0.208	0.408	0.203

Notes: Results of the estimation of $Y_{it} = \alpha + \beta \text{PostConversion}_{it} + \gamma (\text{PostConversion}_{it} \times \text{high arrivals}_{it}) + \delta \text{center}_{it} + \eta (\text{center}_{it} \times \text{high perception}_{it}) + \theta_1 + \theta_2 + \theta_{qm} + \sigma t_n + \epsilon_{it}$. All regressions include fixed effects at the area, month, and quarter-neighborhood levels, and neighborhood-specific time trends. The sample is related to the period 2017–2020 is split in three groups. In Columns (1) and (2) unemployment high (low) refers to a sample restriction to cities with unemployment above (below) the cross-sectional median value in 2011. In Columns (3) and (4) high (low) immigration refers to a sample restriction to cities with immigration shares above (below) the cross sectional median in 2011. In Columns (5) and (6) high (Low) Right support refers to municipalities with votes for right-wing parties in the 2013 national political elections above (below) the cross sectional median. Right-wing parties include *Lega, Fratelli d'Italia, Casa Pound, Forza Nuova e Italia agli Italiani*. Panel A uses as dependent variable the logarithmic form of real estates sale prices, while Panel B uses the logarithmic form of real estates rent prices. OLS estimates with standard errors clustered at the municipality level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

the topic “immigration” exceed the 90th percentile. We then use this variable in the same model we estimated before by interacting it with the conversions. Panel A of Table 3 shows the results of these interaction terms. Columns (1) and (3) of Panel A show a robust negative effect on housing prices and rents, respectively, when conversions coincide with high search volumes. Columns (2) and (4) show no positive effects on the number of ads and number of migrants. These findings support the perception-driven demand hypothesis.

Next, we explore additional data based on terrorist attacks by foreigners, which have been shown to induce anti-immigrant attitudes that even diffuses across European countries (Böhmelt et al. 2020). We capture the peak in adverse attitudes with a dummy that equals one if the monthly number of terrorist attacks by foreigners across Europe exceeds the 90th percentile. As before, we interact this variable with conversions. To account for potential seasonality of terrorist attacks (many attacks are carried out during holidays), we control for the same variable calculated using attacks by non-foreigners interacted with conversions. Columns (1) and (3) of Panel B show again negative coefficients when conversions follow terrorist attacks involving foreigners. While the coefficient for rent prices is not precisely estimated, the one for sale prices is negative and highly significant. Columns (2) and (4) show no positive effects on the number of ads nor any effects on the number of migrants respectively, again minimizing the role of supply-driven explanations.

Importantly, the perception shocks are national or international in scope and do not reflect local migrant inflows, consistently with the fact that the number of migrants hosted in the centers remains stable. These findings support a *perception-based demand channel* — housing demand among natives falls due to fear, stigma, or political backlash triggered by salient events, even when local conditions remain unchanged.

Finally, one alternative mechanism is that weather shocks, which affect immigrant arrivals, may also influence housing demand (Casarano and Natoli, 2023). Several factors make this channel unlikely in our setting: (i) our specifications absorb seasonal variation through month fixed effects; (ii) arrivals occur in southern Italy, far from the study area; and (iii) no conversions took place during peak summer months. In addition, arrival flows are relatively evenly distributed across months, and other immigrant perception-based proxies are unlikely to be related to weather. Taken together, these considerations suggest that weather shocks are not driving our results.

We further corroborate our interpretation by examining treatment effect heterogeneity across pre-existing municipal characteristics. Results are reported in Table 4, with Panel A presenting estimates for

sales prices and Panel B for rental prices. Specifically, we re-estimate our baseline specification for sales and rental prices (Table 2, columns 1 and 3) on samples split by far-right support, unemployment, and immigrant presence.¹¹ We use far-right support as a proxy for anti-immigrant sentiment. We expect areas with higher far-right support to react more strongly to the conversion policy. To test this, we split the sample at the median voting share for far-right parties using data from the 2013 political elections in the ELIGENDO database.¹² The results, reported in columns 5 and 6 of Table 4, show that the conversion effect is stronger in municipalities with greater support for far-right parties, consistent with theories predicting that salience effects are amplified where prior beliefs or prejudices are more entrenched. Next, we split the sample at the median pre-existing immigrant share, measured using 2011 Census data. If the estimated effects are perception-driven, one might expect stronger reactions where the immigrant presence is larger, since very small immigrant shares are less likely to be perceived as a threat. Columns 3 and 4 of Table 4 support this view: only in areas with higher immigrant shares the estimated coefficients are negative and statistically significant both for sale and for rent prices. This pattern is consistent with prior work on the role of immigrant presence in shaping perceptions (e.g., Ajzenman et al., 2023; Hangartner et al., 2019). Lastly, we split municipalities by unemployment rates, using the median from the 2011 Census. Columns 1 and 2 of Table 4, Panels A and B, show no systematic evidence of differential effects across the two outcomes by unemployment levels, providing limited support for a labor-competition channel.

5. Conclusion

Using a unique policy in Bologna, this paper shows that the economic impact of migrant integration measures depends critically on their timing. Converting temporary shelters into permanent integration centers during a migration crisis lowers housing prices, while conversions undertaken before a crisis show no such effect. These results

¹¹ Table A.1 reports balance tests comparing municipalities with high versus low levels of these characteristics. We find no pre-crisis or pre-conversion differences in real estate prices, immigrant presence, or number of centers. The only significant difference is a slightly higher number of centers in municipalities with above-median unemployment prior to our study period.

¹² We classify as far-right anti-immigrant parties: Lega Nord, Fratelli d'Italia, CasaPound, Forza Nuova, and Italia agli Italiani.

Table A.1
Municipality-level comparison.

	(1) (log-) price sale	(2) (log-) price rent	(3) (log-) n. ads	(4) (log-) n. migrants	(5) n. centers
Panel A: municipalities with low/high far-right support					
pre-crisis (2012–2013)	-0.131 (0.152)	0.011 (0.079)	0.224 (0.638)		
pre-conversion (2014–2017)	-0.127 (0.161)	-0.003 (0.094)	0.165 (0.169)	0.236 (0.577)	0.163 (0.148)
Panel B: municipalities with low/high unemployment					
pre-crisis (2012–2013)	-0.076 (0.149)	-0.016 (0.083)	0.141 (0.656)		
pre-conversion (2014–2017)	-0.095 (0.16)	-0.027 (0.098)	0.295* (0.152)	0.343 (0.572)	0.250* (0.136)
Panel C: municipalities with low/high immigrant's share					
pre-crisis (2012–2013)	-0.026 (0.143)	0.007 (0.08)	0.152 (0.65)		
pre-conversion (2014–2017)	-0.043 (0.154)	0.002 (0.094)	0.208 (0.163)	0.229 (0.581)	0.207 (0.142)

Notes: Balance tests across three groups of municipalities: (i) municipality with low/high far-right support, grouped according to an indicator defined using the cross-sectional median of vote share for far-right parties in the 2013 national elections (Panel A); (ii) municipalities with low/high unemployment, grouped according to an indicator defined using the cross-sectional median value of unemployment from the 2011 census (Panel B); and (iii) municipalities with low/high immigrant share, grouped according to an indicator defined using the cross-sectional median value of immigrants' population from the 2011 census (Panel C). Columns (1) and (2) include as dependent variable sale and rent prices respectively (both in logarithmic form), Column (3) uses as dependent variable the logarithm of the total number of sale and rent advertisements published in the platform Immobiliare.it. Column (4) uses as dependent variable the logarithm of the migrants hosted in reception centers, and Column (5) uses number of reception centers. Regressions cover pre-crisis, i.e., years 2012–2013, and pre-conversion periods, i.e., years 2014–2017. OLS estimates with standard errors clustered at the municipality level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.2
Robustness checks.

	(1)	(2)	(3)	(4)
Panel A: (log-) Sale Price				
Conversion × High Arrivals	-0.0316 (0.028) [0.019]*	-0.0559 (0.015)*** [0.012]***	-0.0559 (0.015)*** [0.012]***	-0.0612 (0.011)*** [0.012]***
Macro-Area Time Trend	NO	YES	YES	YES
Macro-Area × Quarter FE	NO	NO	YES	YES
Initial Price × Post Treatment Dummy	NO	NO	NO	YES
Observations	24,612	24,612	24,612	24,612
Adjusted R-squared	0.275	0.555	0.555	0.555
Panel B: (log-) Rent Price				
Conversion × High Arrivals	-0.0308 (0.025) [0.022]	-0.0366 (0.013)*** [0.018]**	-0.0363 (0.014)** [0.018]**	-0.0410 (0.012)*** [0.021]**
Macro-Area Time Trend	NO	YES	YES	YES
Macro-Area × Quarter FE	NO	NO	YES	YES
Initial Price × Post Treatment Dummy	NO	NO	NO	YES
Observations	24,612	24,612	24,612	24,612
Adjusted R-squared	0.275	0.555	0.555	0.555

Notes: Results of the estimation of $Y_{it} = \alpha + \beta \text{PostConversion}_{it} + \gamma (\text{PostConversion}_{it} \times \text{high arrivals}_{it}) + \delta \text{center}_{it} + \eta (\text{center}_{it} \times \text{high perception}_{it}) + \theta_i + \theta_t + \theta_{qt} + \sigma_{\tau_{it}} + \epsilon_{it}$. The sample is related to the period 2017–2020. Column (1) includes fixed effects at the area and month level. Column (2) adds neighborhood-specific time trends. Column (3) adds and quarter-neighborhood levels. Column (4) controls for the interaction between real estate prices (in logs.) observed in January 2014 and a dummy that indicates the conversion of a center from reception to integration. Panel A uses as dependent variable the logarithmic form of real estates sale prices, while Panel B uses the logarithmic form of real estates rent prices. OLS estimates with standard errors clustered at the municipality level in round brackets and Conley (1999) spatial standard errors in square brackets to account for spatial autocorrelation. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

suggest that reactive policies, implemented under crisis conditions, can impose economic costs and intensify local resistance, while anticipatory measures avoid such consequences. More broadly, the findings highlight the importance of timely, well-planned integration policies in reducing social tensions and fostering cohesion between natives and newcomers.

6. Exhibits

See Fig. 1 and Tables 1–4

CRedit authorship contribution statement

Mario F. Carillo: Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Lavinia Piemontese:** Writing – review & editing, Writing – original draft, Investigation, Formal analysis, Data curation, Conceptualization. **Francesco Flaviano Russo:** Writing – review & editing, Writing – original draft, Formal analysis, Data curation, Conceptualization.

Appendix 7

See Tables A.1 and A.2.

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