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Tito Boeri
Marta De Philippis
Eleonora Patacchini
Michele Pellizzari

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Forschungsinstitut
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Institute for the Study
of Labor

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Tito Boeri
IGIER-Bocconi University,
CEPR, fRDB and IZA

Marta De Philippis
LSE and fRDB

Eleonora Patacchini
Universita' di Roma La Sapienza,
CEPR, EIEF and IZA

Michele Pellizzari
IGIER-Bocconi University
fRDB and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Moving to Segregation: Evidence from 8 Italian Cities*

We use a new dataset and a novel identification strategy to analyze the effects of residential segregation on the employment of migrants in 8 Italian cities. Our data, which are representative of the population of both legal and illegal migrants, allow us to measure segregation at the very local level (the block) and include measures of house prices, commuting costs and migrants' linguistic ability. We find evidence that migrants who reside in areas with a high concentration of non-Italians are less likely to be employed compared to similar migrants who reside in less segregated areas. In our preferred specification, a 10 percentage points increase in residential segregation reduces the probability of being employed by 7 percentage points or about 8% over the average. Additionally, we also show that this effect emerges only above a critical threshold of 15-20% of migrants over the total local population, below which there is no statistically detectable effect. The negative externality associated with residential segregation arises only for the employment prospects of immigrants, whether legal or illegal. We do not find evidence of either spatial mismatch or skill bias as potential explanations of this effect. Statistical discrimination by native employers is the remaining suspect.

JEL Classification: J15, J61, R23

Keywords: migration, residential segregation, hiring networks

Corresponding author:

Michele Pellizzari
Department of Economics
Bocconi University
Via Roentgen 1
20136 Milan
Italy
E-mail: michele.pellizzari@unibocconi.it

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1 Introduction

Residential segregation of migrants and minorities is a common feature of most cities in the developed world (Borjas, 1995; Card and Rothstein, 2007; Cutler and Glaeser, 1997; Cutler, Glaeser, and Vigdor, 1999; Ross, 1998) and its role in the process of economic and social integration is at the forefront of the political debate. In the US, this debate, at both a policy and academic level, is long standing and the literature, both theoretical and empirical, offers mixed results.

For example, Munshi (2003) studies Mexican migrants in the US and finds that a larger number of migrants in one's same US location improves employment and wage outcomes. On the other hand, Kling, Liebman, and Katz (2007) exploit a randomized experiment (*Moving to Opportunity*) that offered families that lived in poor neighborhoods the opportunity to move in less segregated areas and show no significant effects on adult economic self-sufficiency (although program participants did take the opportunity to move).

In Europe, residential segregation is attracting increasing attention, mostly due to the political pressures associated to large immigration flows in most European countries (OECD, 2009). Due to the paucity of data, there are only a few studies that focus on Europe and they produce mixed results, as for the US. Clark and Drinkwater (2000, 2002) document that in the UK the poor areas where ethnic minorities live are associated with higher unemployment and lower self-employment. On the other hand, using a natural experiment (i.e. a spatial dispersal policy under which refugees were randomly assigned to locations), Edin, Fredriksson, and Åslund (2003) and Damm (2009), for Sweden and Denmark, respectively, find strong evidence that the size of ethnic enclaves are positively correlated with earnings and job finding rates[1].

In this paper we contribute to this debate by producing empirical evidence using a new and unique survey conducted in 2009 in 8 cities located in the North of Italy. Our data allow us to improve on previous studies along four dimensions.

First, thanks to a particular sampling frame which randomly draws blocks from the continuum of map locations within cities (see Section 2), our survey covers both legal and illegal migrants. Around 20% of migrants in our data are illegally resident in the country and they are far from being a random subgroup of the entire population. Compared to the legally resident, illegal migrants appear to be on average men, younger, slightly less educated, less proficient with the Italian language and more likely to rely on informal networks to look for employment. Despite this heterogeneity with respect to the population of legal migrants, we do not find significant differences in the estimated effect of residential segregation on employment when we exclude illegal immigrants from our sample.

[1]Frijters and Wheatley-Price (2005) and Battu, Seaman, and Zenou (2011) look at immigrant job search methods with UK data. They find that, although personal networks are a popular method of finding jobs among ethnic minorities, they are not necessarily the most effective one.

Second, the data are available at a very detailed level of geographical disaggregation, namely we can identify the exact city block where each interviewed person resides. Hence, we can define residential segregation more accurately than in most previous studies, i.e. at the level of the individual block. Bayer, Ross, and Topa (2008), indeed, shows that this is the relevant definition. Additionally, our data include two important control variables that are often not available in other studies and that allow us to rule out some of the potential alternative explanations for our main finding. The first is an objective measure of the linguistic abilities of migrants, as a formal test of the knowledge of the Italian language was administered at the end of the personal interviews. The second is the time it takes to travel by public transport from each block to the center of the cities.

Third, by merging our survey with data from the national census, we are able to measure various physical attributes of the buildings in each block, which are valid instruments for residential segregation, once conditioning on local house prices, as detailed in Section 3.

Fourth, we allow for discontinuities in the relationship between residential segregation and labor market outcomes. This enables us to identify a critical threshold value above which residential segregation is harmful to the employment of migrants. This result is important to assess the scope for relocation policies within cities.

Italy is a particularly interesting case to study, as the Italian population of migrants increased by a factor of 5 between 1990 and 2010. In the OECD area only Spain attracted a larger number of migrants relative to the native population over the same time period. Moreover, migrants appear to be highly segregated in terms of their residential locations. Based on official data from the 2001 census, the coefficient of variation of the number of resident migrants across census tracts is twice as large as that of natives (1.793 against 0.966 for natives).

The focus on a limited group of 8 cities in Northern Italy allowed us to design the sampling frame very carefully and to use a comprehensive questionnaire for the interviews, thus providing a valuable data set to analyze what lies behind the observed effects of residential segregation on job finding.

Our main results show that migrants who reside in areas with a high concentration of non-Italians are less likely to be employed compared to similar migrants who reside in less segregated areas. The magnitude of these effects is non negligible: in our preferred specification a 10 percentage points increase in residential segregation reduces the probability of being employed by 7 percentage points or about 8% over the average. Additionally, we also show that this effect varies discontinuously around a key threshold value of 15-20% of migrants over the total local population. Below the threshold there is no statistically detectable effect, while a negative and significant impact of residential segregation emerges above the threshold.

Thanks to the richness of our data we are able to rule out two popular explanations for negative employment effects of residential segregation, namely spatial mismatch (Kain, 1968;

Ross, 1998; Weinberg, 2000, 2004) and sorting by skills (Hirsch and Macpherson, 2004). Our results do not vary significantly as we add time to travel to the city center or language test scores to the control set. Moreover, we also find that high concentration of migrants in certain areas of the cities does not affect natives' employment.

Overall, we believe that our results are more consistent with discrimination by native employers who make use of the information about residential locations to infer immigrants' unobservable skills.

The plan of the paper is as follows. Section 2 describes the data, section 3 outlines the identification strategy, Section 4 presents our empirical results, including a large array of robustness checks. Finally, Section 5 briefly characterizes the normative implications of our results and concludes.

2 Data and descriptive evidence

Our analysis is based on data from a new survey of immigrants, which was carried out between October and November 2009 in eight cities in Northern Italy: Alessandria, Brescia, Bologna, Lucca, Milano, Prato, Rimini and Verona. The cities were chosen non-randomly to represent agglomerations of different sizes (large, medium-sized and small) while at the same time guaranteeing a good degree of representativeness of the entire population of the North of Italy, where more than 60% of the migrant population is located.

[insert Figure 1 here]

Figure 1 shows the locations of the 8 surveyed cities on the map of Northern (and Central) Italy and Table 1 reports some key characteristics of these cities comparing them to the averages in the country. Milan is the largest city in the sample, and also one of the largest in Italy (together with Rome and Naples), Bologna, Brescia and Verona are middle-sized, while Alessandria, Lucca, Prato and Rimini can be classified as small cities. In terms of income per capita, the 8 surveyed cities are rather homogeneous, with the exception of Milan, whose 21,000 euros of annual gross income per capita place it among the richest cities in the country. Average age is generally higher than the country average, with Bologna having the oldest population and Prato the youngest. Given the large regional differences in labor market performance, both the unemployment and the employment rates of the 8 surveyed cities are, respectively, lower and higher than the country average and around the average of the Northern regions. Overall, the figures in Table 1 suggest that our sample offers a good representation of the population of the North of Italy.

[insert Table 1 here]

In Table 2 we also show the incidence of migrants in each of the surveyed cities. In column 1 we report the official shares of the non-Italian residents in the entire city. These figures are computed from the local population registers, hence, they can only capture legal migrants. The 8 surveyed cities are characterized by high levels of migration, well above the country average (around 6%) or the average in the northern regions (7%).

The particular sampling frame of our survey (see Section 2.1 below) guarantees that both legal and illegal migrants are covered. In columns 2, 3 and 4 of Table 2 we show the distribution of migrants by legal status on the basis of our preferred definition (definition 1). Under such a definition we code as illegal migrants those who declare not to have a permit of stay or refuse to answer the question on legal status and those who declare not to have access to the Italian health system or not to have the required documents to go back to their home country. In all cities, undocumented migrants represent a sizable proportion of total migration: from 12% in Bologna to over 29% in Brescia.

Since around 6% of such individuals are from EU countries of recent access (e.g. Bulgaria and Romania) and can get the Italian permit of stay with fewer restrictions, we also consider a more restrictive definition (definition 2) that replicates the first one but excludes all immigrants from New Member States from the pool of illegal. Moreover, for robustness we analyze two more definitions that only use information on permits of stay. Namely in definition 3 (4) illegal immigrants are all those (non-EU) respondents without a permit of stay or not answering to the question. In the main analysis we will concentrate exclusively on definition 1 and in the robustness checks section (Section 4.2) we investigate the implications of adopting different definitions.

[insert Table 2 here]

2.1 The sampling procedure

The sampling procedure of our survey was designed with the intent to reach particularly hard-to-trace segments of the population, namely immigrants, both legal and illegal. Migrants are grouped into three macro regions of origin and the survey guarantees representative results only within these three subpopulations: European new member states (NMS)[2], Western Balkan countries (WBS) [3] and all other countries of origin.[4]

The sampling strategy consists of three main steps: in the first stage, we sample neighborhoods in each of the 8 cities and then, in the second stage, we select one block of buildings in

[2]Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovak Republic and Slovenia.

[3]Albania, Bosnia, Croatia, Macedonia, Kosovo, Montenegro and Serbia.

[4]The focus on EU New Member States and the Western Balkan countries was imposed by the European Bank for Reconstruction and Development (EBRD), the sponsor of the study.

each of the sampled neighborhoods where, in the final stage, the individuals to be interviewed are chosen.

The selection of the neighborhoods is based on a mere geographical criterion and it is aimed at identifying those areas where the probability of finding foreign residents is highest. The neighborhoods are, therefore, selected with sampling probabilities that are proportional to the share of legal migrants resident in the neighborhood, as measured by the official population registers. Subsequently, one block of buildings is chosen in each neighborhood, by picking random points from the official city maps released by the city councils[5]. The selection of the blocks is performed on the basis of a simple algorithm that randomly selects points on the maps and then picks the closest block, where blocks are defined as portions of urban surface that are built-up and continuous, i.e. not interrupted by areas for traffic circulation or allocated for public use (e.g. parks).[6]

In each selected block a census of residential units is carried out, so that we know the total number of households in each block as well as their origin. This census is based on a combination of conversations with the buildings' janitors and short door-to-door visits. Within each block 4 persons are randomly selected for each of the population groups that we consider (NMS, WBS, other non-Italians and Italians), so that a maximum of 16 persons are eventually interviewed. Obviously, in most blocks there are fewer that 16 interviews because there were fewer than 4 persons in some of the population groups.[7] This particular process of selection is meant to guarantee a sufficient number of cases to allow meaningful analyses by population groups.

[insert Table 3 here]

Table 3 summarizes the sampling procedure. Each city is divided into 3 districts: central, mid-central and peripheral. The first three columns of the table indicate the number of sampled neighborhoods[8] in each city and district. The fourth column simply sums over the first three and reports the total number of sampled neighborhoods. The average number of interviews/observations per neighborhood is shown in column 5. In columns 4 and 5 we also show

[5]The website http://v.controul.com/app/ shows exactly which blocks were chosen in each neighborhood.

[6] In order to increase sample size, while at the same time maintaining the distribution of population groups (natives and immigrants) additional blocks are selected based on a proximity criterion. Namely, we also include in the survey blocks that are adjacent to one (or more) of the randomly selected blocks where the share of dwellings occupied by immigrant households is higher than a fixed threshold. Since the randomly selected blocks that satisfy the threshold criterion are usually adjacent to several other blocks, only the one adjacent block with the highest incidence of immigrants is selected.

[7]Only individuals older than 18 are eligible for the interview and no more than one person per household is selected.

[8]Ignoring the additional block selected with the proximity criterion, the number of blocks is equivalent to the number of neighborhoods.

in parentheses the total number of neighborhoods in the city (column 4) and the average population in the neighborhoods (column 5), so as to give an indication of the coverage of our sample[9].

The census of the residential units in each block is a particularly precious source of information. Official population registers from the city councils only consider legal immigrants, whereas our census includes both legal and illegal residents, living, either permanently and temporarily, in the considered blocks. We use the block census to construct our measure of residential segregation.

Although the survey includes both migrants and natives, for this study we only consider the subsample of migrants.[10] Interviewees are asked questions on individual and family characteristics, reasons behind migration, living and work conditions, cultural integration and compliance with immigration laws.[11] Additionally, every interviewed person is asked to take an optional language test consisting of a series of questions of growing complexity.[12]

2.2 Descriptive statistics

Given the peculiar sampling structure of our study, we start by comparing our data with other surveys that might be used to conduct studies of migration, namely the official Labour Force Surveys (LFS) and a survey of migrants carried out by the institute *Iniziative e Studi sulla Multietnicità* (ISMU), which is relatively popular in Italy (Dustmann, Fasani, and Speciale, 2010).

[insert Table 4 here]

While the LFS data only capture legal migrants, being sampled from the population registers, the ISMU survey also includes illegal migrants but its sampling frame is radically different from ours (Cesareo and Blangiardo, 2009).[13] In particular, the ISMU survey was carried out between October 2008 and February 2009 in 32 cities all over Italy. Immigrants were interviewed in places where they usually meet or go to seek assistance, such as language schools, immigrant assistance centers and trade unions. The advantage of this sampling method is that

[9]Notice that sampled blocks are much smaller than sampled neighborhoods and have, on average, a population of 208 residential units.

[10]Some information on natives will be used in Table 11.

[11]Especially for the questions about legal status, the interviewers were very carefully instructed to insist on the fact that the survey was carried out exclusively for research purposes, that the data would remain fully anonymous and that none of the institutions involved in the organization of the survey was in any way connected with the immigration authorities, the police or the Ministry of Internal Affairs (which is the institution that issues work and residence permits).

[12]To encourage taking the test a small amount of 5 euros was offered.

[13]Unfortunately, the official Labor Force Survey is not representative at the level of the single municipality and the data for Table 4 are restricted to the North of Italy.

it makes it much easier to reach illegal immigrants, thus allowing for larger sample sizes.[14] However, such an advantage comes at the cost of representativeness, as migrants who are likely to be found in the places covered by the ISMU survey might be very different from the rest.

By construction, migrants are over-represented in our data compared to the LFS, both overall and for each of the subgroups that we consider (NMS, WBS and others), which are equally represented (also by construction).[15] Also, we find slightly more illegal migrants compared to ISMU, although the difference is minor. Female migrants are under-represented in our data compared to both the LFS and ISMU, while the education distribution is remarkably similar. Our interviewees are also more likely to be in employment, a result that is due for the most part to the presence of illegal residents, who are necessarily employed in the shadow sector.

We now focus exclusively on our data and Table 5 provides a description of the main variables used in our empirical exercise of Section 4.

[insert Table 5 here]

On average, migrants are quite younger than natives, with an average age of about 38 years old, which compares to about 43 for Italians. Moreover, the incidence of females is much lower than among natives (47% against 52%). Immigrants into Italy do not appear to be a particularly low-skilled group; more than half of them have at least a degree of secondary education. About 20 percent of our surveyed immigrants are illegal, according to our preferred definition (definition 1). As already mentioned, the language test was optional and approximately 14% of the individuals in the sample refused to take it. The questionnaire also includes several questions on ownership of durables, which can be used as proxies of wealth: slightly more than half of the sample own at least one car, 60% has internet connection at home and almost everybody has a cellular phone. In terms of labor market performance, roughly 88% are employed, which compares to a much lower employment rate for natives (about 50% in Northern Italy. See Table 1). Almost 60 percent obtained their jobs through friends and a third of the interviewees regularly work on Sundays.

We measure residential segregation with the percentage of non-Italian households living in the considered blocks. On average there are over 17% of non-Italian households in the surveyed blocks, with a standard deviation of more than 10 percentage points. We can also define segregation more restrictively as the percentage of households from one's same area in the block. The mean of this variable in our sample is just below 6%, with a standard deviation of 8 percentage points.[16]

[14]The ISMU survey consists of 12,000 interviews to both legal and illegal immigrants.

[15]The ISMU survey covers only immigrants.

[16]All our results have also been replicated using the origin-specific definition of segregation but none of the estimated effects is significant when using the more restrictive one. Hence, in the main text we only use the percentage of non-Italians living in the blocks as measure of residential segregation.

In order to get a first glance at the pattern of residential segregation in our data, Table 6 reports the descriptive statistics on selected variables, distinguishing between immigrants living in high- and low-segregated areas, respectively defined as blocks where our residential segregation (percentage of non-Italians) lies in the top and bottom 25% of the observed distribution.[17] Interestingly, we do not find strong evidence indicating that more educated immigrants sort into less segregated areas. Also differences in car ownership, that can be taken as a proxy for wealth, are minimal. Immigrants residing in highly segregated neighborhoods are slightly older, they arrived in Italy more recently, they seem to be more likely to be employed, to work on Sundays and to have obtained a job through friends, although the row mean differences are not always statistically significant. Also, there does not seem to be a significant difference in the outcome of the language test between those living in high- and low-segregated blocks, although the share of those refusing the test varies significantly.

[insert Table 6 here]

In Figure 2 we also report standard dissimilarity indexes computed for each of our 8 cities across census tracts using official data from the 2001 census (thus, excluding illegal migrants). The dissimilarity index measures the percentage of population that should be relocated in order to reach a perfectly uniform distribution of migrants and natives across census tracts.[18] Bologna and Milan seem to be the cities with the most homogeneous distributions of migrants and natives across census tracts and, even in these cities, the dissimilarity indexes are quite high and indicate that more than one third of the population would have to be reshuffled in order to obtain a perfectly uniform distribution. In the most segregated cities (Alessandria and Brescia) this number peaks around 50%.

[insert Figure 2 here]

3 Empirical model and estimation strategy

Our empirical analysis is primarily aimed at estimating the causal effect of residential segregation on the employment status of migrants. Our empirical model is based on the following

[17] According to the distribution of immigrants in the considered blocks, the threshold level for the high-segregated neighborhoods (top 25% of the distribution) is 25.5% of foreign households and that for low-segregated (bottom 25%) is 7.5% of foreign households.

[18] Let T_c be the total number of census tracts in city c, m_{tc} and n_{tc} the number of migrants and natives, respectively, residing in census tract t of city c, M_c and N_c the total number of migrants and natives, respectively, residing in city c, then the dissimilarity index for city c is computed as:

$$D_c = \frac{1}{2}\sum_{t=1}^{T_c}\left|\frac{m_{tc}}{M_c} - \frac{n_{tc}}{N_c}\right|$$

main equation:

$$y_{icdb} = \alpha_1 RS_{cdb} + \alpha_2 X_i + \alpha_3 B_{cdb} + \delta_d + \delta_c + \epsilon_{icdb} \tag{1}$$

where y_{icdb} is an indicator of employment for migrant i in city c residing in district d and block b; RS_{cdb} is a measure of residential segregation; X_i and B_{cdb} are sets of observable individual and block characteristics, respectively; δ_d is a district fixed effect (central, mid-central, peripheral); δ_c is a city fixed effect and ϵ_{icdb} is the error term. As mentioned above, we measure residential segregation with the percentage of all non-Italians residing in block b of district d and city c.

The parameter of main interest in equation 1 is α_1, whose identification is possibly impeded by the presence of unobservable factors that influence both the location decisions of migrants and their labor market outcomes. For example, one might be worried that residentially segregated migrants are negatively selected, as only the very high ability can afford to live in native-dominated neighborhoods and high ability workers also experience better labor market outcomes, regardless of where they live. Such a mechanism would bias downwards α_1 in standard OLS. Additionally, there might also be unobservable factors at the block level that affect both the migrant's probability of locating in the block as well as labor market success, such as the availability of some public services (employment services, public transport). Finally, our measure of residential segregation, being based on conversations with buildings' janitors and door-to-door conversations, is likely to be affected by measurement error. Although it is difficult to assess the exact extent of mis-measurement, the assumption of classical measurement error seems quite plausible in our setting, so that the resulting bias should draw the estimated parameter closer to zero.

Overall, it is hard to establish whether the total bias in simple OLS (or probit) estimates of equation 1 would be positive or negative.

More formally, one can think of the error term ϵ_{icdb} as being composed of three parts:

$$\epsilon_{icdb} = \eta_i + \Lambda_{cdb} + u_{icdb} \tag{2}$$

where η_i is an unobservable individual term, Λ_{cdb} is an unobservable block characteristic and u_{icdb} is a random term.[19]

The model is completed by an equation that describes residential segregation RS_{cdb}, which is the outcome of the joint process of residential location of both natives and migrants. We model RS_{cdb} in a reduced form framework as follows:

$$RS_{cdb} = \beta_1 B_{cdb} + \beta_2 C_{cdb} + \beta_3 \overline{X}_{cdb} + \Lambda_{cdb} + \overline{\eta}_{cdb} + v_{cdb} \tag{3}$$

[19]For simplicity we consider both η_i and Λ_{cdb} as scalars. The structure of our identification would be unchanged also in the presence of multiple unobservable components at either the individual or the block level.

where we distinguish two types of observable block characteristics: B_{cdb}, which are not excluded from equation 1, and C_{cdb}, which are excluded from equation 1. $\overline{X}_{cdb}$ is the vector of the average X_i among migrants in block cdb and, similarly, $\overline{\eta}_{cdb}$ is the average η_i among migrants in block cdb. v_{cdb} is a random term.

The presence of $\overline{\eta}_{cdb}$ and Λ_{cdb} on the right hand side of equation 3 generates endogeneity of RS_{cdb} in equation 1. In words, there might be unobservable characteristics of either the block (Λ_{cdb}) or the individuals (η_i) that affect both the location decisions and labor market outcomes. This is, in fact, the key identification issue in this literature. Several studies eliminate the problem of correlation in unobservables at the neighborhood level (sorting) by using metropolitan-area level variables and exploiting cross-metropolitan variations (Card and Rothstein, 2007; Cutler and Glaeser, 1997; Evans, Oates, and Shwab, 1992; Ross, 1998; Ross and Zenou, 2008; Weinberg, 2000, 2004). Others are based on special social experiments or quasi-experimental data (see Bayer et al. (2008) for an extensive survey and a balanced view of the existing literature).

One of the most convincing studies so far is Bayer et al. (2008), who use data from the US Census, disaggregated at the level of the city block and city blocks are grouped into small sets of adjacent areas. Hence, they condition on block-group fixed effects in their regression analysis to isolate block-level variation in neighbor attributes. Their identifying (untestable) assumption is the absence of correlation in unobservables across blocks within block groups. The particular sampling structure of our data does not allow us to adopt a similar strategy, as in most neighborhoods only one block is sampled.[20]

Our main identification strategy is different and it rests on the presence of excluded block characteristics C_{cdb}, i.e. variables that, conditional on the set of controls, affect residential segregation and have no impact on the migrants' labor market outcomes. Such variables can be used to instrument RS_{cdb} in equation 1 for identifying the parameter α_1. Contrary to Bayer et al. (2008), whose identification rests on neighborhood fixed-effects, our instrumental variable approach is robust to the presence of unobservable factors at any level of geographical disaggregation (including the block).

More specifically, we use the physical characteristics of the buildings in the block 10 years before the survey as instruments for current residential segregation. Using the actual addresses of the residential units of the individuals in our sample, we have linked our data to an ancillary database of the 2001 Italian population census. Such an ancillary database contains a large set of descriptive characteristics of each single city block in Italy, including the number of buildings by decade of construction, the total amount of square meters in the block (i.e. the sum of the square meters of each floor in each building), broken down by residential and commercial

[20]See footnote 6 for more details. We will use the information on the few adjacent blocks in Section 4.2 (Table 15) for a robustness check.

space. We use these data to construct two instruments: the average age of the buildings in the block and the ratio of residential square meters per residential building in the block. The first variable obviously measures whether the block is composed of relatively new or old houses, whereas the second takes high values in areas that are dominated by large residential buildings (lots of residential square meters for few buildings) and low values in areas with many smaller houses (e.g. residential villas, detached or semi-detached houses).

The validity of these instruments rests on the idea that the historical characteristics of the buildings are correlated with house prices and that migrants originally locate in areas with particularly convenient market conditions. Then, conditional on current housing prices, migrants have easier access to houses in areas where other migrants already reside. Importantly, our identification rests on a conditional exogeneity assumption, where the ability to observe and condition on current house prices as well as on other neighborhood characteristics is crucial.[21]

In Table 7 we produce evidence in support of our instruments. Column 1 shows the correlation coefficient between the two instruments in each city and suggests that there is quite bit of independent variation in the two variables, supporting the use of both of them simultaneously. Apart from Lucca, areas with the oldest buildings are also those with the lowest housing density, although the size of the correlation coefficient varies substantially from -0.9 in Rimini to -0.27 in Brescia (0.33 in Lucca).

Columns 2 and 3 document the relevance of the instruments and report the correlation between each of them and our measure of residential segregation. Interestingly, such correlations are very city-specific: when looking at building's age the coefficients are normally positive but in Alessandria, Bologna and Prato and range between -0.37 in Bologna to 0.765 in Brescia. Results for housing density are also heterogeneous across cities, as the correlation of this characteristic with residential segregation is negative in all cities but Alessandria, Bologna and Lucca and the range of variation remains very wide, from -0.755 in Rimini to 0.33 in Brescia.

Finally, in columns 4 and 5 we provide some suggestive evidence in support of the exogeneity assumptions. Although one important feature of our analysis is the ability to control for local house prices, one might be worried that house prices are not a good enough control for the potential set of current neighborhood attributes that might be correlated with the lagged physical characteristics of the buildings and, at the same time, also affect employment outcomes. In this perspective, it is reassuring that our instruments, which are measured about 10 years prior to the survey, are not particularly strongly correlated with current prices. Although,

[21]Our identification strategy is similar in spirit to the one that uses lagged values of the immigration-related variable in the different areas to instrument its current values (Altonji and Card, 1991). The use of historical area characteristics rather than the historical residential segregation rules out problems arising from the possible presence of area-level unobservable factors that are highly persistent over time and that are correlated with the settlement process and with the current labor market performance of migrants, e.g. see Pischke and Velling (1997). We will use the more traditional instrument for robustness checks in Section 4.2.

the correlation coefficients often reach conventional levels of statistical significance, they take different signs in different cities and are generally low in magnitude. Across the entire sample, the correlation coefficient between current house prices and lagged housing density is 0.003 and it goes up to 0.258 with the lagged age of the buildings.

We presume that this result is due to the relatively low time-series correlation of building's physical features. Current features affect current prices but, as new buildings are built and the use (residential vs. commercial) of existing buildings is modified, the housing market evolves in such a way that historical features are largely orthogonal to current prices. If current house prices are so mildly correlated with historical buildings' attributes, then it is plausible to assume that also other potential unobservable block characteristics that might affect employment are orthogonal to our instruments.

[insert Table 7 here]

The results in Table 7 justify the specification of our first stage regression (see Table A.1 in Section 4), where we interact both our instruments with city dummies to take into account the heterogeneity described in this table. The local patterns of spatial associations at the city-level are also depicted in Figure A.1 in the Appendix. We construct quintile maps that depict the geographical distribution of residential segregation and our indicators of urban structure for the cities under analysis. Darker areas correspond to higher values of the inspected variable. For example, in Alessandria there is an important settlement of migrants in the northern outskirts of the city, where housing density is low and buildings are old; in Bologna we observe instead a substantial presence of immigrants in areas where buildings are more recently constructed (especially in the South-East); in Brescia migrants are mainly located in the city center, where buildings are older and housing density is lower. Urban structure, thus, appears to be related to immigrant population density, with pronounced city-specific qualifications.

One other potential concern with our identification strategy is related to the possibility that the physical characteristics of the residential buildings might reflect some unobservable individual characteristics, such as ability to live in better dwellings, that could also be correlated with one's employment outcomes. Although we believe that this is very unlikely, given that we control for a vast set of individual and area-level characteristics, including proxies for income, house prices and commuting times, in Table 8 we investigate the correlation between the most important individual observable characteristics in our data and the housing types where people reside, within cities and districts.

Specifically, we define dummy indicators for blocks where the average age of the buildings is above the mean (within cities) and where the density of houses is above the mean (within cities) and we run simple OLS regressions of individual observables (age, education and car ownership, years since migration) on such indicators, controlling for city and, in some specifications (odd columns) also district fixed effects. We find that none of the observables that we consider is significantly correlated with buildings' characteristics. Of course, our identification rests on the lack of such correlation among unobservable individual traits but finding a zero correlation for several different observable characteristics is, at least, reassuring.

[insert Table 8 here]

3.1 Estimation and inference

Since the outcome that we consider in our empirical application is a simple dummy indicator for employment, we adopt a probit model.[22] Nevertheless, we prefer to frame the discussion of the identification structure in a linear setting, in order to clearly show that we do not exploit the non-linearity of the probit model for identification purposes. However, for comparison purposes we replicate main results (namely, those in Table 9) using a linear probability model in Table A.2 in the Appendix. The estimates are qualitatively unchanged.

As it is well known, identification in non-linear models in the presence of endogeneity poses additional complications compared to simpler linear models. We adopt the two-step procedure proposed by Rivers and Vuong (1988), which is computationally simpler that the more standard full-information maximum likelihood (FIML) approach.[23] Assuming joint normality of the error terms of equations 1 and 3, one can write the error term of the main model as:

$$\epsilon_{icdb} = \theta v_{cbd} + \xi_{icdb} \tag{4}$$

where $\theta = \frac{Cov(v_{cbd},\epsilon_{icdb})}{Var(v_{cbd})}$ and ξ_{icbd} is a purely random normal error. Then, one can estimate v_{cbd} with the OLS residuals of equation 3 and augment the main model of equation 1 with such estimated residuals. This procedure amounts to replacing ϵ_{icdb} with equation 4 in equation 1 and naturally leads to a probit model that consistently estimates all the parameters, including θ.

Beyond its computational simplicity, this approach also offers the advantage of producing a simple test of exogeneity. The coefficient of the first-stage residuals that is estimated in the second stage of the procedure is a consistent estimator of θ and, consequently, its conventional z-statistics is a perfectly valid test of the exogeneity of RS_{cdb} in equation 1. On the other hand, a major disadvantage of the Rivers&Vuong's procedure is that it produces rescaled coefficients

[22] Since the mean of our dependent variable is close to unity, the magnitude of the effects that would be estimated by a linear probability model would be highly misleading. This is the reason why we prefer to use a non-linear model, despite the estimation difficulties that we describe in this section.

[23] The full-information maximum likelihood approach very often fails to converge with multiple endogenous explanatory variables. In fact, we could not reach convergence of the FIML model when exploring non linearities (Table 10).

that are not directly comparable to their non-instrumented analogs. We will still be able, though, to compute comparable average partial effects by first calculating the marginal effect for every observation and then averaging over the entire sample.

Inference is also slightly complicated in our setting. Producing robust standard errors after the Rivers&Vuong's procedure is relatively easy and can be done either using the exact formula or by simple bootstrapping (which is what we do, with 200 replications). However, one might also want to allow non-zero correlation across the error terms of individuals located in the same city but the standard approach to clustering guarantees asymptotic consistency only as the number of cluster tends to infinity, thus it is subject to large small sample bias with few clusters, as in our case.

To address this issue, we adopt the *pair cluster bootstrap-t procedure* (with 500 replications) proposed by Cameron, Gelbach, and Miller (2008), which reduces the small sample bias. This methodology directly produces test statistics, whose distribution is generally unknown without further assumptions. This is the reason why in our tables we report p-values instead of standard errors, as it is somewhat more common. For completeness and comparison, we also compute p-values based on both a simple bootstrapped (200 replications) robust variance covariance-matrix of the estimators, which imposes lack of serial correlation both within and across cities, and the pair cluster bootstrap-t procedure with clusters defined at the city level.

4 Empirical results

Table 9 reports the probit estimation results of model 1, where the dependent variable is a dummy indicator of employment, using as measure of segregation the share of foreign households in the block. Columns 1 and 2 show our baseline specification where the set of controls includes a quadratic polynomial of age, a gender dummy, two education dummies (for secondary and tertiary levels, primary is the reference group), dummy indicators for ownership of (at least one) car(s), (at least one) mobile phones(s), internet at home, a full set of dummies for the years since migration in Italy, dummies for origin (New member countries and Western Balkans, other origins is the reference category). In column 2 our segregation measure is instrumented using the exogenous physical characteristics of the residential buildings interacted with city dummies. Column 1 of Table A.1 shows the first-stage results. Although the relationship between urban housing structure and residential segregation is different across cities (see Section 3), the instruments strongly predict immigrants residential patterns everywhere. The F-test of the excluded instruments range from around 5 to over 10, depending on the specification.

[insert Table 9 here]

Results show a negative and statistically significant impact of segregation on employment prospects, which is even more negative when using our instruments. The non-instrumented estimate is a 1.2 percentage point decrease in the probability of employment per 10 percentage point increase in segregation, but the coefficient does not reach conventional levels of statistical significance (column 1). The corresponding instrumented estimate is -0.054 and precision increases to the 5% level (column 2). This specification suggests that for the average person in the sample a 10 percentage points increase in residential segregation reduces the probability of being employed by 7 percentage points or about 8% over the average.[24]

Notice that the IV coefficient on our regressor of interest is, in absolute value, significantly larger that its non-IV counterpart, hence the endogeneity bias seems to be positive. As already discussed in Section 3, this is the combined outcome of the many potential sources of endogeneity in our model, such as individual sorting, unobserved neighborhood shocks or measurement error.

As we already discussed earlier on, the finding of columns 1 and 2 in Table 9 can be rationalized by several alternative explanations. Thanks to the richness of our data we can rule out some of them. In columns 3 to 6 of Table 9 we augment the set of controls with time-to-travel to the city center (columns 3 and 4) and with the scores of the language test (columns 5 and 6). We construct the time-to-travel measure by combining information on the exact address of each block in our sample and the center of the city, which given the strong historical heritage of all 8 cities (as most cities in Italy) is very easy to identify.[25] We then use the online websites of each local transportation authority to compute the time (in minutes) necessary to travel to the center by public transport.

The results in columns 3 and 4 of Table 9 indicate that the point estimates of the main effect of interest do not change significantly, suggesting that the spatial mismatch hypothesis is unlikely to provide an explanation for our findings. Similarly, segregation by language skills is also a potential explanation for the negative effect of residential segregation. Immigrants who cannot speak well the language of the destination country are at the same time more likely to live in segregated areas, where they can more frequently use their native language, and less likely to find jobs, given that knowledge of the local language is normally a requirement for most jobs. Unlike previous studies that considered only subjective language skill measure involving substantial measurement error (Dustmann and van Soest, 2001), we can test this hypothesis relying on the language tests carried out at the end of the interviews. We find that differences in language skills do not affect our main findings significantly, as shown in columns 5 and 6 of Table 9.

[24]We compute this average partial effect by first calculating the marginal effect for every observation and then averaging over the entire sample.

[25]The historical centers of the ancient roman or medieval cities still remain today the most important commercial areas in the majority of Italian cities and certainly in all the 8 that are covered in our survey.

Interestingly, notice that the estimated coefficients on the first stage residuals does not unequivocally support the endogeneity concerns. Statistical significance at conventional levels is achieved only when using the pair cluster bootstrap-t procedure in column 2.

Having ruled out some other explanations, we believe that discrimination by native employers is likely to be the most plausible reason why residential segregation leads to worse employment outcomes in our setting.

Employers may form prior beliefs about some unobservable qualities of perspective workers from information about their residential locations, under the assumption that only the best workers manage to leave those locations and pay the higher rents of less segregated neighborhoods. In equilibrium, if all employers share the same belief and migrants can only access segregated areas on arrival, nobody will ever be able to find better jobs and leave the segregated locations.

Although we are unable to provide definitive evidence in support of this explanation, we now present some additional results that are at least consistent with it and in contrast with others.

One interesting aspect of segregation that our data allow us to investigate is the role of non-linearities. The very concept of residential segregation is related to the idea that migrants concentrate in certain areas and it is only when such a concentration is particularly high that it may become relevant for a variety of outcomes, like employment.

Variation in the percentage of migrants across blocks is a necessary condition for identification in our model of equation 1, but it is interesting to explore more in details if there are explicit non-linearities in our data. In Table 10 we investigate the functional form of the relationship between residential segregation and employment by adding a quadratic term of residential segregation to both our specifications (instrumented and non-instrumented).

[insert Table 10 here]

Although, none of the coefficients on the linear and the squared residential segregation is estimated to be significant, the combined effect could be different from zero in some range of the observed distribution. Hence, Figure 3 plots the marginal effect derived from the estimates in column 1 of Table 10 over the distribution of our measure of segregation, together with 95% confidence intervals.[26] Since the first stage residuals do not appear to be significant in Table 10, we base the plot of Figure 3 on the non-instrumented estimates.

[26]The marginal effect is computed as:

$$\frac{\partial Pr(y_{icdb} = 1 \mid X_{icdb})}{\partial RS_{cdb}} = \varphi(X_{icdb})(\alpha_{1,1} + \alpha_{1,2} RS_{cdb})$$

where X_{icdb} is the full set of explanatory variables of equation 1 and $\alpha_{1,1}$ and $\alpha_{1,2}$ are the coefficients on the linear and the quadratic terms of RS_{cdb}, respectively.

The marginal effect is computed at the sample average of all the other explanatory variables in the model. Results show a very interesting pattern, with the incidence of migrants in one's block being essentially unimportant for employment until it reaches the threshold of 20%. After that threshold, which is located approximately around the 70% percentile of the distribution, the estimated marginal effect becomes negative and statistically significant at the 95% level and it remains rather constant for the remaining observable range of variation in residential segregation.

In unreported estimates, this finding is confirmed in a specification where residential segregation is redefined as living in blocks with at least a certain percentage of foreigners and we experiment with varying thresholds.

[insert Figure 3 here]

Overall, the results in Table 10 (and Figure 3) suggest that residential segregation generates a negative externality on the employment prospects of immigrants only when it reaches a threshold of approximately 15-20%. This is an important result that is consistent with statistical discrimination by native employers being the origin of such a penalty, as employers' beliefs about the correlation of residential location and workers' unobservable skills can only arise when a large enough mass of minority workers are concentrated in some specific areas. From the policy perspective, non-linearities open the door to relocation policies that may increase the average level of employment. However, one has to understand the mechanism behind these critical mass effects. Spatial mismatch cannot account for these non-linear effects. Sorting by skills could generate non-linear effects, but would bite even at relatively low levels of concentration without generating critical mass effects.

[insert Table 11 here]

In Table 11 we also document that residential segregation does not matter for the employment of natives, a result that is consistent with many papers (Angrist and Kugler, 2003; Bodvarsson, den Berg, and Lewer, 2008; Card, 1990, 2005; Friedberg and Hunt, 1995; Ottaviano and Peri, 2011) and that contradicts the spatial mismatch as well as the skill sorting hypotheses, at least in their simplest formulation.

Another popular argument related to residential segregation is based on informal hiring networks in ethnic communities and it is supported by a growing US literature (Elliott, 2001; Falcon and Melendez, 1996; Mouw, 2002; Munshi, 2003). Moreover, Conley and Topa (2002) show that these network effects are found to be highly localized, even at the level of the housing block, as in Bayer et al. (2008). In fact, as we report in Table A.3 in the Appendix, informal hiring networks seem to be present in our setting as those living in the most segregated areas are also more likely to find jobs through friends. However, the presence of local informal hiring networks should lead to a positive effect of segregation on employment probabilities, at least within some range.

To investigate further ethnic network effects, we perform in Table A.4 our analysis using as a proxy for segregation the share of households belonging to the same ethnic group. If ethnic groups find employment in particular jobs and industries in which own-ethnics are overrepresented, a positive and significant effect should be uncovered here. Table A.4 shows instead evidence in line with the results in Table 9. Ethnic networks may thus be important but they cannot account for the entire story behind our data.

4.1 Illegal immigrants

Illegal immigration is an important peculiarity of immigration into Europe, in particular for Southern European countries. Given that one of the most innovative features of our survey is the possibility of observing illegal immigrants, in Table 12 we investigate their role more thoroughly.

[insert Table 12 here]

In column 1 we replicate the model of Table 9 eliminating the indicator for illegal migrants from the control set (but still including such observations in the estimation). Results barely change, suggesting that the somewhat arbitrary assumptions made to identify legal and illegal migrants separately do not affect our main findings. Next, in columns 2, 3, 4 and 5 we eliminate from the sample the illegal immigrants as defined on the basis of our four alternative definitions, so as to replicate results that could be produced with more standard surveys that only cover the legally resident population. Once again, results change only very marginally.

One potential explanation for the findings of Table 12 would be that legal and illegal immigrants do not differ much, especially in terms of their unobservables. To investigate such an intuition, Table 13 compares a long list of observable individual characteristics between the samples of legal and illegal immigrants. Each cell reports the unconditional or conditional (on city and district dummies) difference between the means of the variable indicated in the first column of the table across the samples of legal and illegal immigrants. All estimates are produced by OLS.

[insert Table 13 here]

Compared to the legally resident, illegal migrants appear to be on average men, younger, slightly less educated, less proficient with the Italian language and more likely to rely on informal networks to look for employment. Especially when we restrict attention to the first definition, illegal immigrants also appear to be more recent migrants.

Overall, most of the differences detected in Table 13 are easily explained by a very simple integration process by which more recent and younger immigrants are more likely to be illegal and, thus, also less likely to own cars and speak well the language. However, having in mind 13, those differences do not seem relevant or pronounced enough to meaningfully affect the employment prospects of immigrants.

4.2 Robustness checks

In this section we perform two important checks of the robustness of our main results, namely those in Table 9.

[insert Table 14 here]

First, in Table 14 we replicate the estimates of Table 9 using alternative sets of instruments. In column 1 we only use housing density (interacted with the city dummies) and we exclude housing age from the set of instruments and we obtain an estimated effect that is still negative and significant, although a bit larger than our benchmark (Table 9, column 3).

In column 2, we do the opposite and use only housing age, excluding density. Now the estimated effect is still negative, smaller than the benchmark and it does not reach statistical significance at conventional levels.

In column 3, we substitute our measure of housing age in the block, which is computed on the basis of data collected during the 2001 census, with the same measure based on the previous census (1991). This is an important check given the rationale that we claim underlies the relevance of our instrument, namely the correlation between historical migration patterns and historical physical characteristics of the buildings. According to this argument, the farther back we can go in time to compute the instruments the better in terms of validity of the exogeneity assumption. Unfortunately, the 1991 census does not include measures of housing density, which appears to be the key excluded instrument in generating statistically significant effects, as suggested by the results in columns 1 and 2 in Table 14. For this reason we use the 2001 census for our benchmark estimates but the similarity of the results produced using the same set of instruments computed for different time periods, as in columns 2 and 3 of Table 14 supports the validity of our IV strategy.

Finally, in column 4 we adopt an approach that is rather common in migration studies (Altonji and Card, 1991), which consists in instrumenting current migration with its historical analog. In our case, such an approach amounts to using the percentage of immigrants in the block computed from the 2001 census, i.e. 9 years before our survey.[27] Results are in line with

[27]We also experimented with the same instrument computed from the 1991 census but at that time there were still very few immigrants in Italy and the instrument varies too little to generate statistically significant results.

our main findings, namely the estimated coefficient is still negative although it is estimated less precisely.

The next check that we report is meant to compare our identification strategy with that of Bayer et al. (2008), which rests on the comparison of blocks within narrowly defined groups using a fixed-effect model. As we discussed in Section 2.1, our survey includes a few blocks that are adjacent to one another, literally across the street to one another. Although these are selected blocks characterized by a particularly high incidence of migrants, they can be used to replicate our results using the approach of Bayer et al. (2008) on the subsample of adjacent blocks. Such a strategy consists in running models similar to ours in Table 9 including a set of fixed effects for narrowly defined groups of blocks. In our case such groups are pairs of adjacent blocks. The group fixed effects are meant to control for the local unobservables $\overline{\eta}_{cdb}$ and Λ_{cdb} in our empirical model of equations 1 and 3 and, thus, play exactly the same role of our instruments for identification purposes.

Unfortunately, since in our survey only a small set of blocks are adjacent, this approach comes at the cost of dramatically reducing the size of the sample. In fact, only slightly less than 30% of our individuals live in blocks with an adjacent sampled block.

[insert Table 15 here.]

In Table 15 we report the estimates of models similar to those of Table 9 (columns 1 and 2) where we adopt this alternative fixed-effect strategy. Given the smaller sample size we have to modify the control set and make the model more parsimonious otherwise the outcome can be perfectly predicted for too many individuals. In the footnote to the table we describe the new set of controls and, for brevity, in the table we only report the coefficients of interest. For brevity we only consider the indicator of residential segregation based on all immigrants. Panel A reports results from probit models while Panel B uses a logistic distribution, which is robust to the incidental parameter problem (Neyman and Scott, 1948). For comparison purposes, in column 1 we show the results obtained on the entire sample with the more parsimonious set of controls that is used also in columns 2 and 3, when the sample is restricted to individuals living in adjacent blocks. In column 2 we do not include block-pairs fixed effects for comparison with column 1 while in column 3 we do include those. In this sense, the results in column 3 of Table 15 should be compared with those in column 2 of Table 9.

We find that the estimated effects of residential segregation are still negative and of similar magnitude to those reported in Table 9.

5 Conclusions

The growing European debate around the effects of the recent immigrant inflows into the European labor markets suffers from inappropriate data availability and consequent scarcity of empirical evidence on immigrant-related issues. In this paper we take advantage of the information provided by new survey data that cover both legal and illegal migrants in 8 cities in the North of Italy to estimate the effect of residential segregation on employment.

Our analysis highlights a number of important empirical findings. First there appears to be a negative externality on employment prospect of immigrants related to their segregation in some areas. The effect is sizeable: according to our preferred specification, for the average person in the sample a 10 percentage points increase in residential segregation reduces the probability of being employed by 7 percentage points. Second, this negative effect appears to be non-linear or, more specifically, it varies discontinuously around a key threshold value of 15-20% of migrants over the total local population. Below the threshold there is no statistically detectable effect, while a negative and significant impact of residential segregation emerges above the threshold. Third, the effect does not involve natives while it equally affects legal and illegal migrants.

Our results cannot be rationalized in terms of the standard interpretations of the effects of residential segregation on employment. In particular, the spatial mismatch hypothesis is at odds with the non-linearities we observe in the relationship between segregation and employment and the fact that natives are unaffected. Moreover, the negative effect of segregation on employment survives when we include in our specification time-to-travel measures. Skill bias, another interpretation offered by the literature for the effects of residential segregation on employment, would also be inconsistent with the critical mass effects we observe in the data. Moreover, the effect survives when we include among the regressors the scores obtained in a language test that was administered at the end of the personal interviews.

The remaining suspect is statistical discrimination of native employers who may attach a negative signal for unobservable skills to immigrants located in segregated areas, at least when the level of segregation is larger than a given threshold.

From the policy perspective our results have important implications. On the one hand, the non-linearities in the effect of segregation on employment open the door to relocation policies aimed at improving overall employment rates, hence economic integration of migrants. On the other hand, the empirical evidence documented above leaves little room for policies lowering commuting costs by improving public transport or establishing a network of public employment services in segregated areas in order to improve immigrants' access to jobs in more distant locations from where they live.

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Tables

Table 1: Characteristics of the sampled cities

City	Size[a]	Income per capita[b]	Average age[c]	Unemployment rate[d]	Employment rate[d]
	[1]	[2]	[3]	[4]	[5]
Alessandria	93,676	13,648	46	0.065	0.45
Bologna	374,944	18,771	47	0.044	0.48
Brescia	190,844	15,812	45	0.048	0.48
Lucca	89,640	14,920	45	0.065	0.46
Milano	1,295,705	21,358	45	0.044	0.49
Prato	185,091	12,446	43[e]	0.057	0.51
Rimini	140,137	12,059	45[f]	0.070	0.46
Verona	265,368	15,220	44	0.049	0.48
Italy	60,045,068	12,953	43	0.112	0.43
Northern Italy[e]	27,390,496	15,529	44	0.049	0.49

[a] Number of residents. Source: ISTAT, 2009.
[b] Annual gross taxable income. Source: Tax declarations, 2007.
[c] Source: ISTAT, 2007.
[d] Source: ISTAT, 2001 Population Census.
[e] Source: City Population Register, 2005.
[f] Source: City Population Register, 2009.
[g] Norther Italy includes the following regions: Piemonte, Valle D'Aosta, Lombardia, Trentino Alto Adige, Veneto, Friuli Venezia Giulia, Liguria, Emilia Romagna.

Table 2: Shares of immigrants in the surveyed cities

	From population registers	From fRDB/EBRD survey		
		legal	illegal[a]	total
	[1]	[2]	[3]	[4]
Alessandria	0.110	0.136	0.032	0.168
Bologna	0.090	0.098	0.025	0.123
Brescia	0.156	0.200	0.092	0.292
Lucca	0.079	0.092	0.017	0.109
Milano	0.140	0.165	0.035	0.200
Prato	0.137	0.178	0.064	0.242
Rimini	0.092	0.114	0.044	0.158
Verona	0.130	0.147	0.031	0.178

[a] Illegal immigrants (definition 1): those without a permit of stay or not answering to the question, those declaring not to have access to Italian health system and not to have the documents to go back to their country more often.

Table 3: Sampling structure

	Sampled neighborhoods per district				
	Central	Mid-central	Peripheral	Total[a]	Average obs[b]
	[1]	[2]	[3]	[4]	[5]
Alessandria	2	3	1	6 (23)	3.8 (4073)
Bologna	2	5	7	14 (90)	6.2 (4135)
Brescia	2	3	0	5 (30)	5.4 (6482)
Lucca	2	2	6	10 (79)	4.6 (1093)
Milano	4	8	19	31 (87)	6.5 (14879)
Prato	0	2	4	6 (35)	2.8 (5334)
Rimini	2	3	1	6 (57)	6.2 (2455)
Verona	0	4	5	9 (23)	3.7 (11528)
Total	14	30	43	87 (424)	5.4 (6247)

[a] Total number of neighborhoods in the city in parentheses.
[b] Average number of resident persons per neighborhood (from city registers) in parentheses.

Table 4: Comparison with other data sources

Variable	Survey		
	fRDB-EBRD[a]	LFS[b]	ISMU[c]
	[1]	[2]	[3]
Share of migrants	0.75	0.07	1.00
Share of migrants from NMS[d]	0.25	0.17	0.13
Share of migrants from Western Balcans[e]	0.25	0.19	0.17
Share of migrants other origins	0.25	0.63	0.70
1=illegal migrant[f]	0.20	0.00	0.11
1=female migrants	0.47	0.51	0.51
1=no education	0.04	0.05	0.04
1=primary education	0.38	0.46	0.30
1=secondary education	0.48	0.39	0.45
1=tertiary education	0.10	0.10	0.21
1=employed	0.85	0.47	0.68

[a] These statistics refer to the whole sample (1,137 observations), not just to the sample used for the empirical results.

[b] The LFS data, being sampled from the population registers, only capture legal migrants. Moreover, it is not representative at the level of the single municipality and the reported data are restricted to the entire North of Italy.

[c] The ISMU data include both regular and irregular immigrants. It is based on 12,000 interviews conducted between October 2008 and February 2009 at popular social venues for migrants, such as language schools, assistance centers, et. The reported data are also restricted to the North of Italy.

[d] Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovak Republic and Slovenia.

[e] Albania, Bosnia, Croatia, Macedonia, Kosovo, Montenegro and Serbia

[f] Preferred definition of illegal immigrant (definition 1).

Table 5: Descriptive statistics

Variable	Mean [1]	Std. Dev. [2]	N [3]
Socio-demographic characteristics:			
Area of origin:			
New Member States (NMS)[a]	0.32	-	474
Western Balkans[b]	0.31	-	474
Other countries	0.36	-	474
Age	37.54	9.00	474
1=female	0.47	-	474
years living in Italy	8.98	5.27	474
Education:			
none	0.04	-	474
primary	0.39	-	474
secondary	0.46	-	474
tertiary	0.11	-	474
1=illegal immigrant	0.20	-	474
1=refused test	0.14	-	474
Language test score	482.08	88.7	474
1=owns (at least) one car	0.54	-	474
1=owns (at least) one mobile phone	0.99	-	474
1=internet at home	0.61	-	474
Labour market outcomes:			
1=employed	0.88	-	474
1=work on Sundays	0.32	-	400
1=found work through friends	0.58	-	402
Residential segregation (at the block level):			
% of non-Italians	16.60	10.37	474
% of immigrants from same origin	6.28	7.79	474

[a] Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovak Republic and Slovenia

[b] Albania, Bosnia, Croatia, Macedonia, Kosovo, Montenegro and Serbia

[c] The score of the test was normalized so that the average score is 500 with a standard deviation of 100.

Table 6: High and low segregated immigrants

Variable	High segregation[a] [1]	Low segregation[a] [2]	Diff. [3]
Age	39.607	36.638	2.968
	(0.887)	(0.808)	(1.199)
1=female	0.470	0.487	-0.017
	(0.046)	(0.046)	(0.065)
Years since migration	8.179	10.033	-1.854
	(0.428)	(0.516)	(0.672)
1=secondary education or more	0.632	0.580	0.053
	(0.044)	(0.045)	(0.063)
1=illegal migrant	0.188	0.252	-0.064
	(0.036)	(0.040)	(0.054)
Language test score[b]	470.831	485.754	-14.923
	(10.540)	(9.524)	(14.340)
1=refused test	0.068	0.210	-0.142
	(0.023)	(0.037)	(0.044)
1=owns (at least) one car	0.513	0.538	-0.025
	(0.046)	(0.046)	(0.065)
1=employed	0.905	0.807	0.099
	(0.027)	(0.036)	(0.099)
1=work on Sundays	0.371	0.220	0.152
	(0.047)	(0.043)	(0.064)
1=found work through friends	0.612	0.568	0.043
	(0.048)	(0.051)	(0.070)

The table reports t-test on the difference in means (standard errors in parentheses) of the indicated variable in the two samples.

[a] High- and low-segregated blocks are those where our measure of residential segregation lies in the top and bottom 25% of the observed distribution, respectively.

[b] The score of the test was normalized so that the average score is 500 with a standard deviation of 100.

Table 7: Correlations of block characteristics

City	Correlation coefficients				
	Age of buildings[a]	Residential segregation[b]		Housing prices	
	vs.	vs.	vs.	vs.	vs.
	Housing density[c]	Age of buildings[a]	Housing density[c]	Age of buildings[a]	Housing density[c]
	[1]	[2]	[3]	[4]	[5]
Alessandria	-0.719	-0.250	0.279	-0.080	0.164
	(0.000)	(0.250)	(0.197)	(0.718)	(0.453)
Bologna	-0.673	-0.370	0.326	0.169	-0.193
	(0.000)	(0.000)	(0.002)	(0.116)	(0.073)
Brescia	-0.270	0.765	-0.478	0.558	-0.501
	(0.172)	(0.000)	(0.012)	(0.002)	(0.008)
Lucca	0.363	0.330	0.214	0.645	0.326
	(0.012)	(0.024)	(0.241)	(0.000)	(0.025)
Milano	-0.554	0.281	-0.199	0.399	-0.108
	(0.000)	(0.000)	(0.004)	(0.000)	(0.127)
Prato	-0.700	-0.122	-0.477	-0.526	0.323
	(0.002)	(0.641)	(0.053)	(0.030)	(0.206)
Rimini	-0.909	0.695	-0.755	-0.816	0.882
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Verona	-0.308	0.241	-0.524	-0.383	0.376
	(0.081)	(0.177)	(0.002)	(0.028)	(0.031)

P-values in parentheses.
[a] Average age of the buildings in the block.
[b] Percentage of immigrants in the block.
[c] Ratio of residential square meters per residential building in the block

Table 8: Immigrant characteristics across types of blocks

Variables	Age		1=Secondary educ. or more		1=Owns (at least) one car		Years since migration	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
New buildings[a]	-0.084	-0.174	-0.042	-0.054	0.015	0.025	-0.641	-0.662
	(0.930)	(0.854)	(0.407)	(0.279)	(0.765)	(0.618)	(0.228)	(0.221)
Dense housing[b]	0.105	0.171	-0.039	-0.057	0.166	0.128	0.589	0.146
	(0.918)	(0.864)	(0.472)	(0.292)	(0.002)	(0.016)	(0.306)	(0.797)
City fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
District fixed effects	yes	no	yes	no	yes	no	yes	no
Observations	474	474	474	474	474	474	474	474

P-values from robust standard errors in parentheses.
[a] 1=buildings in the block are older than average (within the city).
[b] 1=housing density in the block is higher than average (within the city).

Table 9: Probit regressions for employment

Variables	Dependent variable: 1=employed					
	Probit	IV-Probit[a]	Probit	IV-Probit[a]	Probit	IV-Probit[a]
	[1]	[2]	[3]	[4]	[5]	[6]
% of non-Italians	-0.012	-0.054	-0.010	-0.049	-0.013	-0.046
	(0.244)	(0.039)	(0.357)	(0.207)	(0.259)	(0.007)
	(0.264)	(0.038)	(0.320)	(0.014)	(0.196)	(0.018)
1=illegal immigrant	-0.451	-0.486	-0.455	-0.499	-0.415	-0.452
	(0.036)	(0.002)	(0.033)	(0.086)	(0.053)	(0.115)
	(0.088)	(0.118)	(0.106)	(0.132)	(0.176)	(0.202)
Average housing price	0.001	0.001	0.001	0.001	0.001	0.001
	(0.005)	(0.078)	(0.031)	(0.043)	(0.025)	(0.005)
	(0.248)	(0.118)	(0.294)	(0.152)	(0.136)	(0.156)
Time to travel to city center[b]	-	-	-0.009	-0.001	-0.009	-0.002
			(0.410)	(0.963)	(0.420)	(0.893)
			(0.336)	(0.932)	(0.392)	(0.868)
1=refused test	-	-	-	-	-0.367	-0.466
					(0.128)	(0.144)
					(0.530)	(0.524)
Language test score[c]	-	-	-	-	0.002	0.002
					(0.091)	(0.235)
					(0.312)	(0.308)
First stage residuals[d]	-	0.051	-	0.049	-	0.042
		(0.232)		(0.177)		(0.106)
		(0.032)		(0.104)		(0.144)
City fixed effects	yes	yes	yes	yes	yes	yes
District fixed effects	yes	yes	yes	yes	yes	yes
Observations	474	474	474	474	474	474

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.
Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins).

[a] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies. The complete first stage results are reported in Table A.1. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

[b] Time to travel is measured in minutes by public transport and it is computed from the websites of the local transportation authorities.

[c] The test score is set to the mean score for individuals who refused to take it and the set of controls includes a dummy for not taking the test.

[d] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table 10: Segregation and employment with non-linearities

Variables	Dependent variable: 1=employed	
	Probit	IV-Probit[a]
	[1]	[2]
% of non-Italians	0.023	-0.014
	(0.199)	(0.859)
	(0.268)	(0.578)
% of non-Italians squared	-0.001	-0.001
	(0.061)	(0.584)
	(0.196)	(0.220)
First stage residuals[b]	-	0.040
		(0.308)
		(0.166)
City fixed effects	yes	yes
District fixed effects	yes	yes
Observations	472	472

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.

Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins), time to travel to the city center, language test score (including a dummy for refusing to take the test) and a dummy for illegal immigrants.

[a] Segregation (both linear and squared) is instrumented with the average age and average housing density of building in the block, both interacted with city dummies.

[b] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table 11: Employment and segregation for natives

Variables	Dependent variable: 1=employed	
	Probit	IV-Probit[a]
	[1]	[2]
% of non-Italians	-0.022	-0.031
	(0.164)	(0.610)
	(0.306)	(0.436)
Age	0.464	0.465
	(0.000)	(0.042)
	(0.198)	(0.208)
Age squared	-0.006	-0.006
	(0.000)	(0.036)
	(0.190)	(0.194)
1=female	-0.963	-0.935
	(0.001)	(0.182)
	(0.190)	(0.200)
1=primary education[b]	2.470	2.465
	(0.004)	(0.453)
	(0.312)	(0.286)
1=secondary education[b]	2.113	2.054
	(0.014)	(0.532)
	(0.404)	(0.464)
1=tertiary education[b]	2.790	2.771
	(0.002)	(0.394)
	(0.406)	(0.438)
Average housing price[c]	0.002	0.002
	(0.002)	(0.232)
	(0.436)	(0.428)
Time to travel to city center	0.017	0.021
	(0.275)	(0.623)
	(0.212)	(0.220)
First stage residuals[d]		0.010
		(0.873)
		(0.724)
City fixed effects	yes	yes
District fixed effects	yes	yes
Observations	190	181

Robust standard errors in parentheses clustered at the city level. Additional controls: car ownership, mobile ownership, internet at home.

[a] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies.

[b] Omitted category: no education

[c] Time to travel is measured in minutes by public transport.

[d] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table 12: Illegal immigrants

Variables	Dependent variable: 1=employed				
	full sample no illegal imm. control	only legal def. 1[a]	only legal def. 2[b]	only legal def. 3[c]	only legal def. 4[d]
	[1]	[2]	[3]	[4]	[5]
PANEL A: Probit models					
% of non-Italians	-0.011	-0.012	-0.009	-0.012	-0.011
	(0.294)	(0.356)	(0.466)	(0.323)	(0.337)
	(0.370)	(0.328)	(0.434)	(0.308)	(0.400)
Observations	474	350	391	368	415
PANEL B: IV Probit models[e]					
% of non-Italians	-0.054	-0.072	-0.074	-0.084	-0.093
	(0.127)	(0.342)	(0.216)	(0.122)	(0.069)
	(0.036)	(0.178)	(0.110)	(0.146)	(0.108)
First stage residuals[f]	0.052	0.069	0.074	0.082	0.093
	(0.240)	(0.385)	(0.067)	(0.134)	(0.076)
	(0.034)	(0.168)	(0.108)	(0.152)	(0.102)
Observations	474	350	391	368	415

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level. Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins), time to travel to the city center, language test score (including a dummy for refusing to take the test).

[a] Definition 1: illegal immigrants are those without a permit of stay or not answering to the question, those declaring not to have access to Italian health system and not to have the documents to go back to their country more often.

[b] Definition 2: illegal immigrants are NON EU citizens without a permit of stay or not answering to the question, those declaring not to have access to Italian health system and not to have the documents to go back to their country more often.

[c] Definition 3: illegal immigrants are those without a permit of stay or not answering to the question.

[d] Definition 4: illegal immigrants are NON EU citizens without a permit of stay or not answering to the question.

[e] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies.

[f] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table 13: Legal and illegal immigrants

Variable	Definition 1[a] unconditional [1]	Definition 1[a] conditional [2]	Definition 2[b] unconditional [3]	Definition 2[b] conditional [4]
1=female	-0.133	-0.112	-0.258	-0.233
	(0.018)	(0.051)	(0.000)	(0.000)
Age	-1.839	-1.888	-1.372	-1.270
	(0.078)	(0.066)	(0.281)	(0.315)
Years in Italy	-0.998	-0.745	-0.388	0.109
	(0.123)	(0.268)	(0.621)	(0.895)
1=(at least) secondary education	-0.097	-0.091	-0.194	-0.176
	(0.091)	(0.117)	(0.003)	(0.010)
Language test score	-48.580	-42.372	-58.985	-48.951
	(0.000)	(0.000)	(0.000)	(0.000)
1=from NMS	0.123	0.128	-0.188	-0.194
	(0.029)	(0.027)	(0.000)	(0.000)
1=from Western Balkans	0.006	-0.013	0.171	0.158
	(0.911)	(0.814)	(0.010)	(0.020)
1=owns (at least) a car	-0.185	-0.186	-0.165	-0.158
	(0.001)	(0.002)	(0.012)	(0.025)
1=owns (at least) one mobile phone	-0.016	-0.008	-0.026	-0.015
	(0.298)	(0.481)	(0.235)	(0.340)
1=internet at home	0.001	-0.005	0.047	0.048
	(0.984)	(0.934)	(0.463)	(0.460)
1=found work through friends	0.174	0.157	0.199	0.182
	(0.004)	(0.011)	(0.004)	(0.010)

Each cell reports the unconditional or conditional (on city and district dummies) difference between the means of the variable indicated int he first column across the samples of legal and illegal immigrants. All estimates are produced by OLS.

P-values based on a standard robust estimate of the variance of the estimated difference.

[a] Definition 1: illegal immigrants are those without a permit of stay or not answering to the question, those declaring not to have access to Italian health system and not to have the documents to go back to their country more often.

[b] Definition 2: illegal immigrants are non-EU citizens without a permit of stay or not answering to the question, those declaring not to have access to Italian health system and not to have the documents to go back to their country more often.

Table 14: Estimates with different IV sets

Variables	Dependent variable: 1=employed			
Instruments:	Only housing density (2001) [1]	Only housing age (2001) [2]	Only housing age (1991) [3]	Residential segregation in 2001[a] [4]
Share of non-Italians	-0.096	-0.010	-0.006	-0.064
	(0.029)	(0.799)	(0.859)	(0.194)
	(0.062)	(0.718)	(0.876)	(0.144)
First stage residuals[b]	0.092	-0.003	-0.007	0.059
	(0.044)	(0.943)	(0.829)	(0.251)
	(0.056)	(0.888)	(0.858)	(0.192)
Observations	474	474	466	474

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.

Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins).

[a] Percentage of non-Italians in the census tract computed from the 1991 census.

[b] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table 15: Estimates with neighborhood effects

Variables	Dependent variable: 1=employed		
	all sample	adjacent blocks[a]	
	[1]	[2]	[3]
PANEL A: Probit models			
% of non-Italians	-0.002	-0.020	-0.013
	(0.010)	(0.033)	(0.040)
	(0.724)	(0.996)	(0.998)
Block-pair fixed effects	no	no	yes
Observations	472	155	82
PANEL A: Logit models			
% of non-Italians	-0.015	-0.031	-0.010
	(0.024)	(0.064)	(0.072)
	(0.432)	(0.622)	(0.408)
Block-pair effects	random	random	fixed
Observations	472	157	82

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.

Additional controls: age, gender, a dummy for education above primary school, legal status, quintiles of language test scores, car ownership, time to travel to city center, year of arrival in Italy (linear), dummy for non taking the language test.

[a] The sample is limited to individual residing in neighborhoods where two blocks have been sampled.

Figures

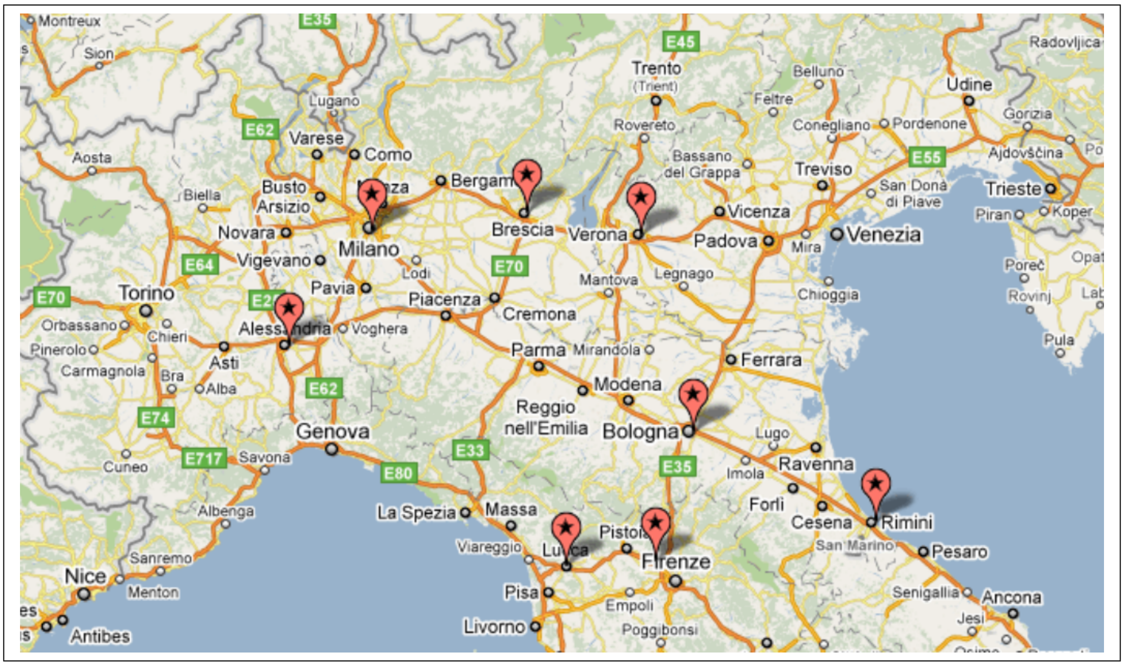

Figure 1: Surveyed cities

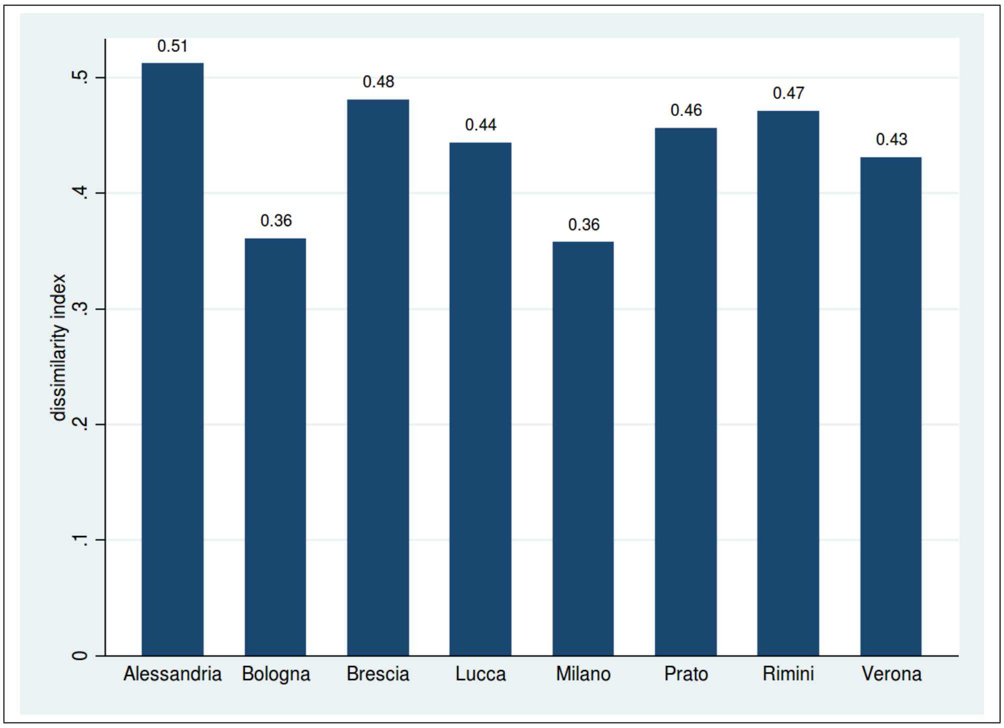

Figure 2: Dissimilarity indices

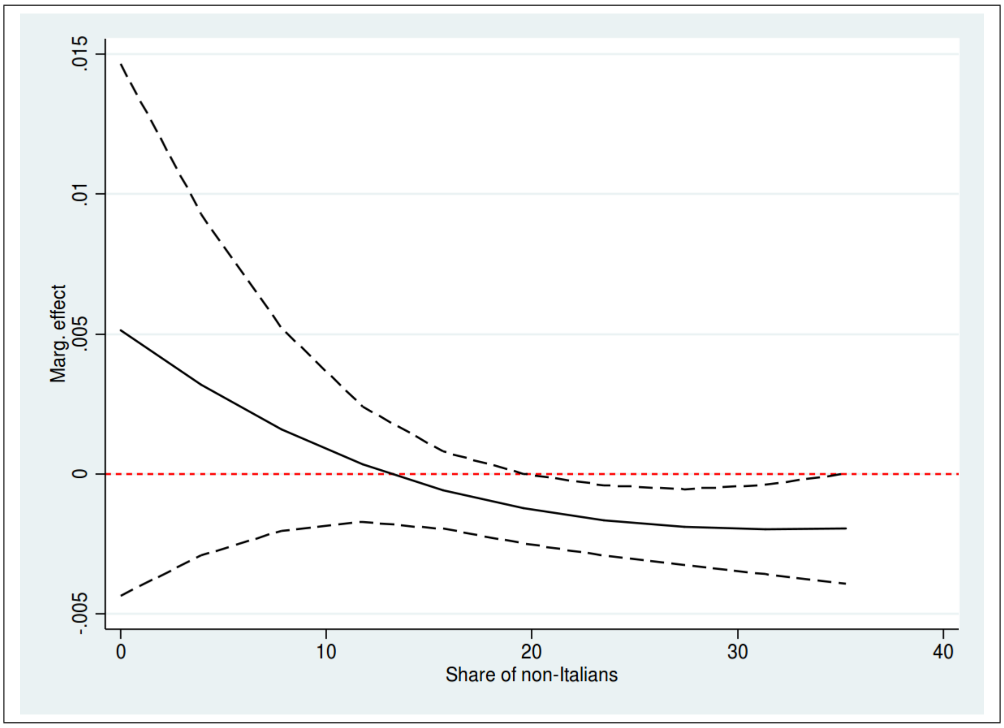

Figure 3: Quadratic effect of segregation on employment

Appendix

Table A.1: First stage regressions

Variables	Segregation in the block:	
	% of non-Italians	% of migrants from same origin
	[1]	[2]
Housing age[a]	0.102	0.149
	(0.645)	(0.038)
	(0.630)	(0.298)
Housing age x Bologna	-0.216	-0.264
	(0.356)	(0.004)
	(0.594)	(0.358)
Housing age x Brescia	0.098	-0.129
	(0.664)	(0.273)
	(0.770)	(0.768)
Housing age x Lucca	0.076	-0.067
	(0.748)	(0.394)
	(0.824)	(0.534)
Housing age x Milano	-0.026	-0.046
	(0.910)	(0.667)
	(0.872)	(0.604)
Housing age x Prato	-0.942	-0.769
	(0.002)	(0.001)
	(0.378)	(0.376)
Housing age x Rimini	0.245	-0.063
	(0.332)	(0.711)
	(0.428)	(0.946)
Housing age x Verona	-0.038	-0.140
	(0.885)	(0.112)
	(0.904)	(0.602)
...		

Table A.1: First stage regressions (continued)

Variables	Segregation in the block: % of non-Italians	% of migrants from same origin
	[1]	[2]
...		
Housing density[b]	0.005	0.000
	(0.130)	(0.822)
	(0.294)	(0.738)
Housing density x Bologna	-0.003	-0.001
	(0.369)	(0.609)
	(0.564)	(0.626)
Housing density x Brescia	-0.008	0.001
	(0.022)	(0.623)
	(0.428)	(0.550)
Housing density x Lucca	-0.006	0.003
	(0.794)	(0.751)
	(0.708)	(0.770)
Housing density x Milano	-0.005	-0.000
	(0.086)	(0.877)
	(0.478)	(0.852)
Housing density x Prato	-0.046	-0.027
	(0.000)	(0.002)
	(0.384)	(0.386)
Housing density x Rimini	-0.017	-0.001
	(0.192)	(0.950)
	(0.528)	(0.514)
Housing density x Verona	-0.015	-0.005
	(0.000)	(0.039)
	(0.432)	(0.432)
Average housing price	0.003	-0.002
	(0.012)	(0.145)
	(0.346)	(0.124)
N	474	474

Robust standard errors in parentheses clustered at the city level. Additional controls: age, age squared, gender, education, legal status, language test score, car ownership, mobile ownership, internet at home, time to travel to city center, years since migration in Italy, dummy for non taking the language test, dummies for origin (New member countries, Western Balkans, other origins). * significant at 10%; ** significant at 5%; *** significant at 1%.

[a] Average year of construction of the buildings in the block.

[b] Residential square meters over residential buildings in the block.

Table A.2: Linear probability models for employment

Variables	Dependent variable: 1=employed					
	Probit	IV-Probit[a]	Probit	IV-Probit[a]	Probit	IV-Probit[a]
	[1]	[2]	[3]	[4]	[5]	[6]
Share of non-Italians	-0.001	-0.007	-0.001	-0.006	-0.001	-0.006
	(0.542)	(0.024)	(0.637)	(0.031)	(0.551)	(0.047)
	(0.392)	(0.008)	(0.522)	(0.010)	(0.354)	(0.010)
1=illegal immigrant	-0.081	-0.087	-0.080	-0.086	-0.073	-0.079
	(0.111)	(0.078)	(0.118)	(0.082)	(0.151)	(0.110)
	(0.384)	(0.362)	(0.386)	(0.368)	(0.402)	(0.356)
Average housing price	0.000	0.000	0.000	0.000	0.000	0.000
	(0.012)	(0.001)	(0.060)	(0.007)	(0.078)	(0.012)
	(0.242)	(0.024)	(0.258)	(0.026)	(0.284)	(0.088)
Time to travel to city	-	-	-0.001	0.000	-0.001	-0.000
center[b]			(0.580)	(0.954)	(0.529)	(0.902)
			(0.354)	(0.928)	(0.302)	(0.834)
1=refused test	-	-	-	-	-0.045	-0.059
					(0.372)	(0.230)
					(0.614)	(0.506)
Language test score[c]	-	-	-	-	0.000	0.000
					(0.115)	(0.088)
					(0.136)	(0.128)
City fixed effects	yes	yes	yes	yes	yes	yes
District fixed effects	yes	yes	yes	yes	yes	yes
Observations	474	474	474	474	474	474

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.

Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins).

[a] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies. The complete first stage results are reported in Table A.1.

[b] Time to travel is measured in minutes by public transport and it is computed from the websites of the local transportation authorities.

[c] The test score is set to the mean score for individuals who refused to take it and the set of controls includes a dummy for not taking the test.

Table A.3: Probit for jobs found through friends

Variables	Dependent variable: 1=jobs found through friends			
	Probit	Probit	IV-Probit[a]	IV-Probit[a]
	[1]	[2]	[3]	[4]
% of non-Italians in the block	0.020		0.005	
	(0.013)		(0.828)	
R&V residuals[b]			0.019	0.031
			(0.462)	(0.008)
City fixed effects	yes	yes	yes	yes
District fixed effects	yes	yes	yes	yes
Observations	402	402	402	402

The sample is restricted to employed persons. Robust standard errors in parentheses clustered at the city level. Additional controls: age, age squared, gender, education, legal status, language test score, car ownership, mobile ownership, internet at home, time to travel to city center, dummies for years since migration in Italy, dummy for non taking the language test, dummies for origin (New member countries, Western Balkans, other origins).

[a] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies.

[b] Estimated residuals from the first stage regression. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

Table A.4: Probit regressions for employment

Variables	Dependent variable: 1=employed			
	Probit	Probit	IV-Probit[a]	IV-Probit[a]
	[1]	[2]	[3]	[4]
% of immigrants from same	0.008	-0.001	-0.020	-0.027
country of origin	(0.386)	(0.950)	(0.727)	(0.658)
	(0.412)	(0.808)	(0.634)	(0.572)
1=illegal immigrant	-0.450	-0.520	-0.389	-0.520
	(0.039)	(0.071)	(0.114)	(0.037)
	(0.066)	(0.052)	(0.268)	(0.070)
Average housing price	0.001	0.000	0.001	0.000
	(0.005)	(0.306)	(0.020)	(0.573)
	(0.248)	(0.276)	(0.332)	(0.228)
Time to travel to city center[b]		-0.012		-0.010
		(0.277)		(0.632)
		(0.246)		(0.328)
1=refused test		-0.314		-0.357
		(0.203)		(0.330)
		(0.556)		(0.542)
Language test score[c]		0.002		0.002
		(0.078)		(0.303)
		(0.266)		(0.296)
R&V residual			0.030	0.042
			(0.514)	(0.603)
			(0.498)	(0.544)
City fixed effects	yes	yes	yes	yes
District fixed effects	yes	yes	yes	yes
Observations	474	472	474	472

Two versions of the p-values of the estimated coefficients are reported in parentheses. The first version is based from a standard robust variance-covariance matrix. The second version is based on a *pair cluster bootstrap-t procedure* with 500 replications and clustering at the city level.

Additional controls: age, age squared, gender, education, car ownership, mobile ownership, internet at home, dummies for years since migration in Italy, dummies for origin (New member countries, Western Balkans, other origins).

[a] Segregation is instrumented with the average age and average housing density of building in the block, both interacted with city dummies. The complete first stage results are reported in Table A.1. The estimation is carried out following the procedure proposed in Rivers and Vuong (1988).

[b] Time to travel is measured in minutes by public transport and it is computed from the websites of the local transportation authorities.

[c] The test score is set to the mean score for individuals who refused to take it and the set of controls includes a dummy for not taking the test.

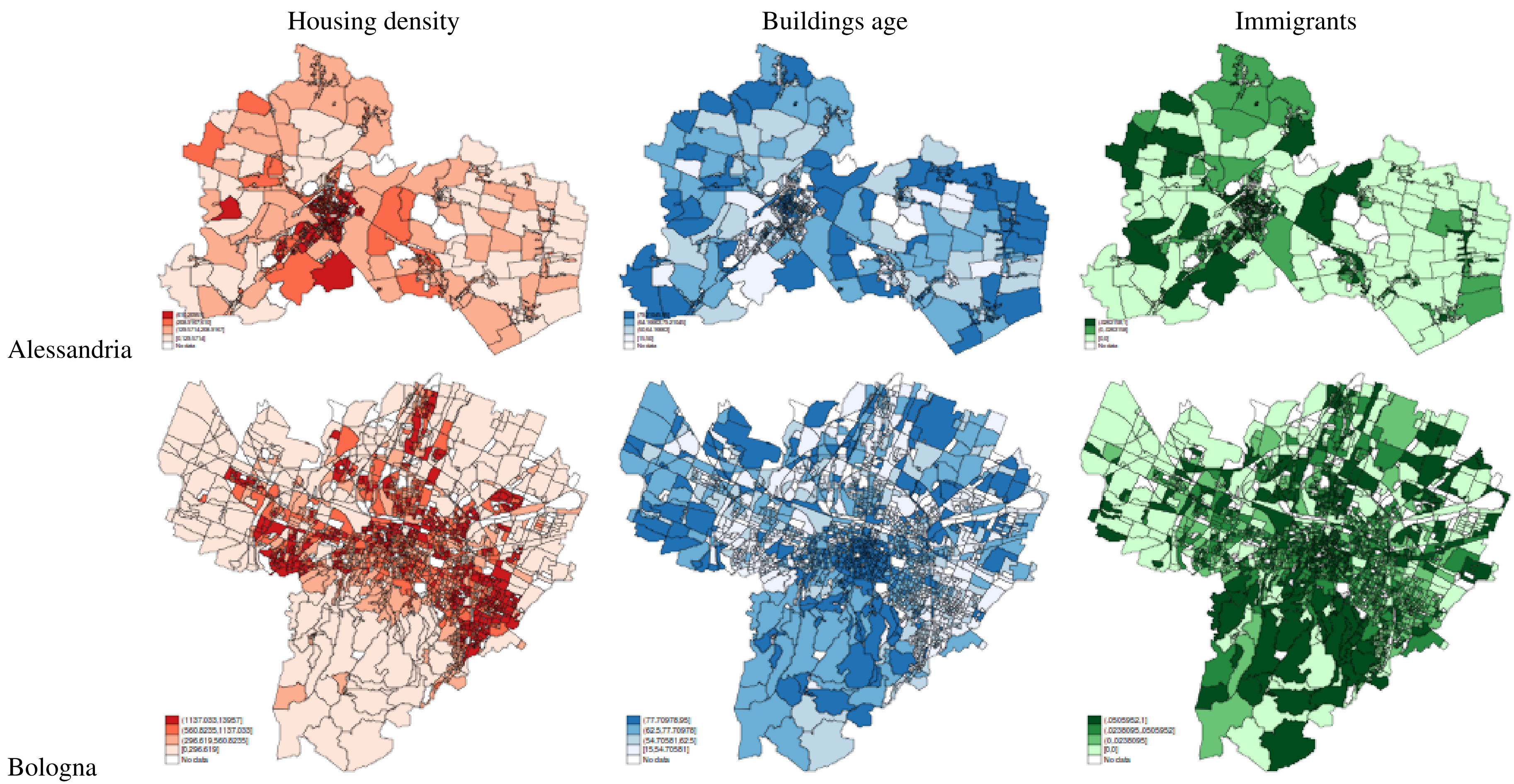

Figure A.1: Urban structure and immigrant population density

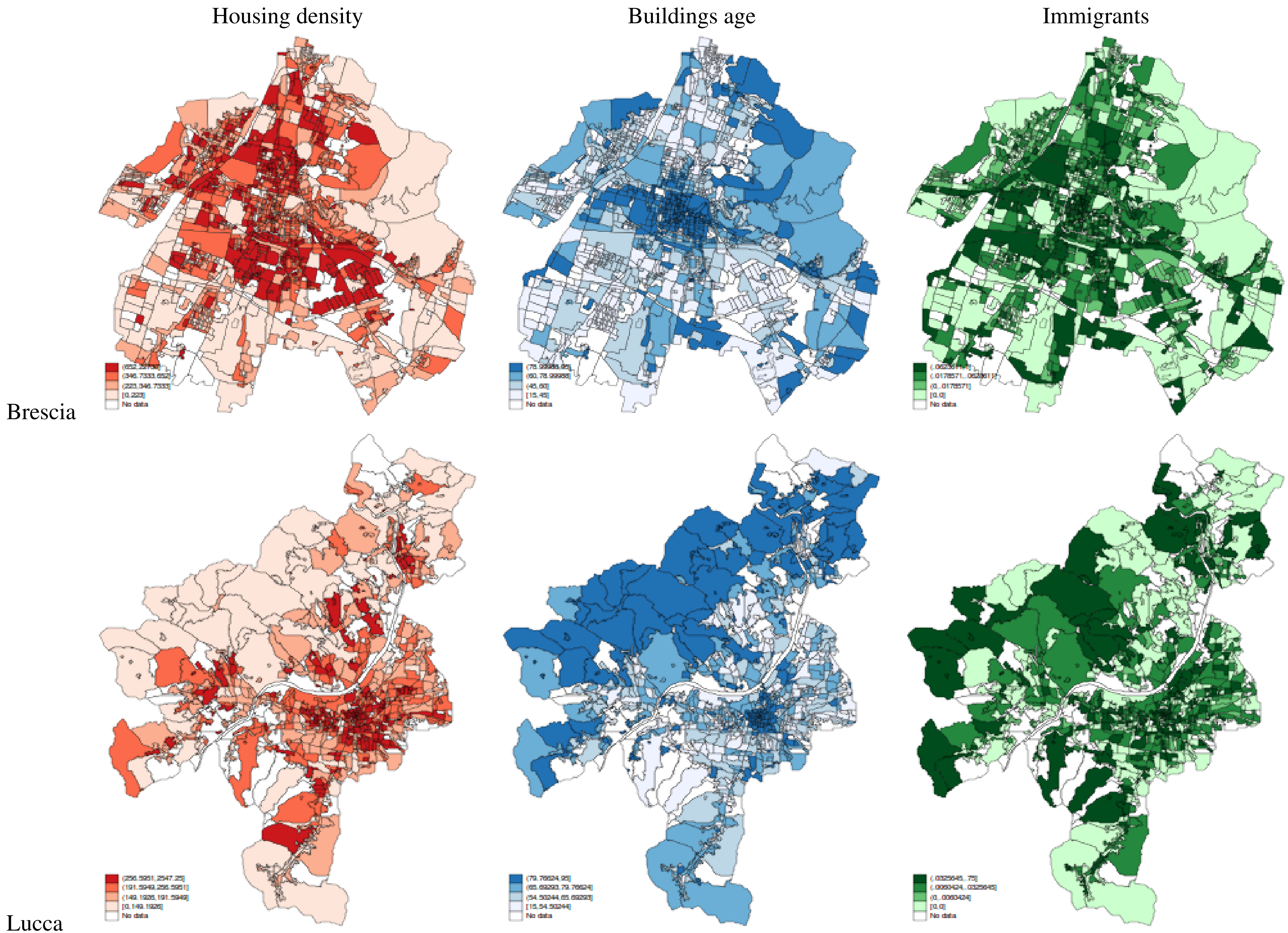

Figure A.1: Urban structure and immigrant population density (continued)

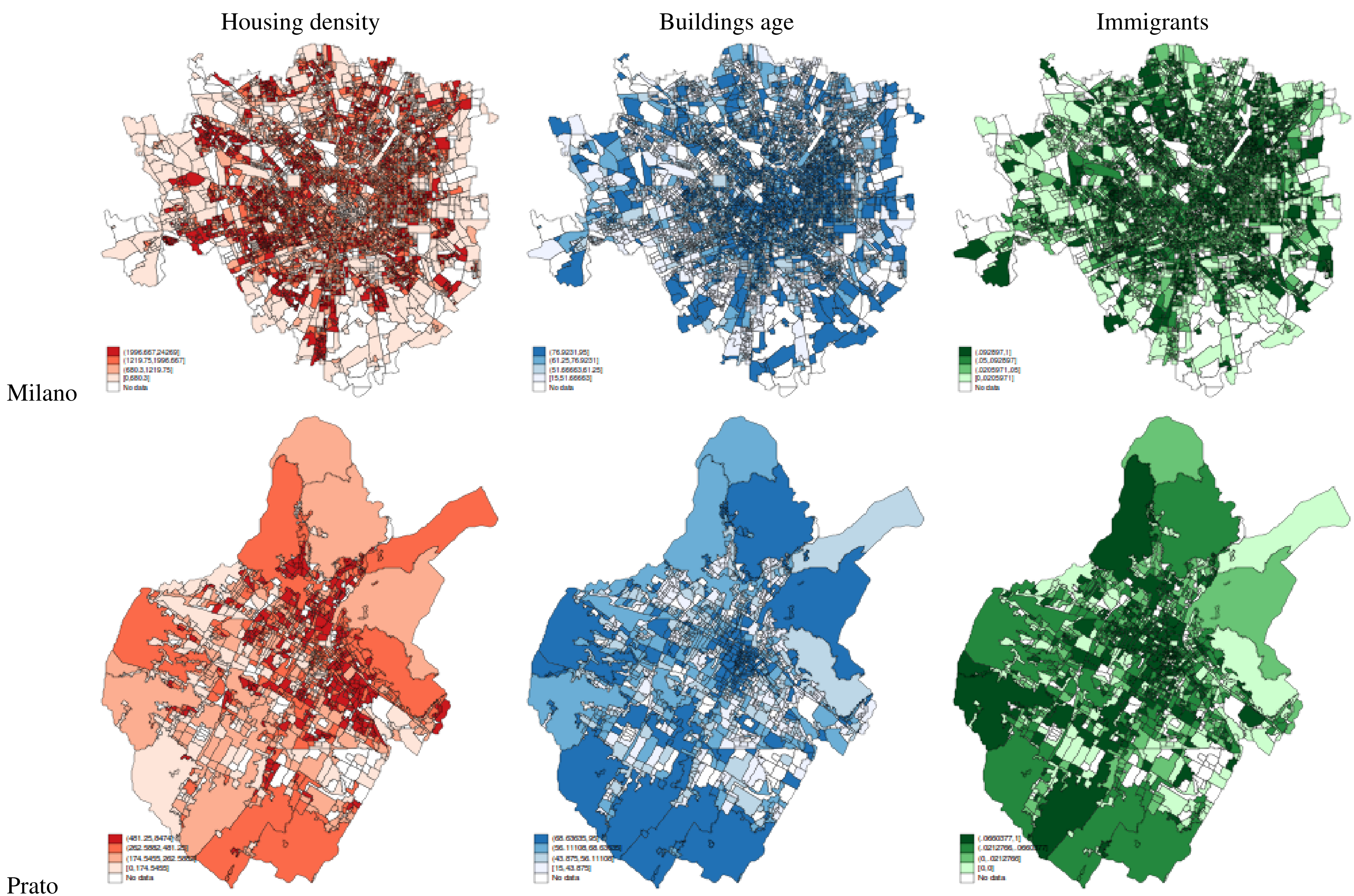

Figure A.1: Urban structure and immigrant population density (continued)

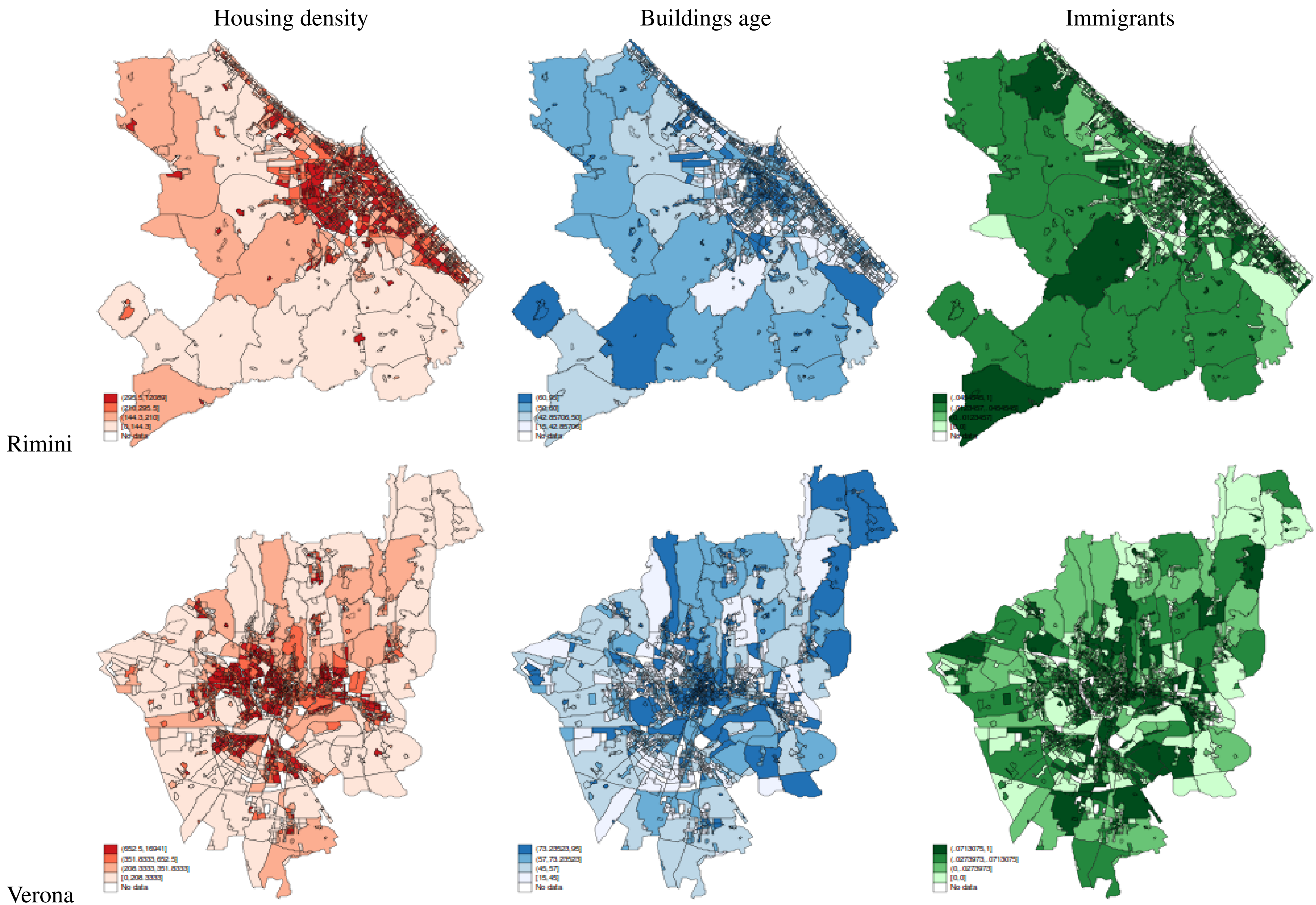

Figure A.1: Urban structure and immigrant population density (continued)