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Decomposing the impact of immigration on house prices

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ABSTRACT

How does an increase in immigrant inflows affect housing demand and prices for a given housing supply? In this paper, I show that we can formally decompose total demand changes into those from the immediate increase in population due to the new arrivals (the "partial effect") and additional changes from relocated natives (the "induced effect"). I propose and apply a method to estimate these effects separately, exploiting data for Spain between 2001 and 2012. Using an instrumental variables strategy, I find that a one percentage point increase in the immigration rate raises average house sale prices by 3.3%. Partial demand estimates are 24% lower than total estimates due to immigrants and natives locating in the same provinces. The results show that accounting for the impact of immigration on native mobility is central to understanding net demand adjustments, as partial and total effects can significantly differ depending on native population relocation.

1. Introduction

The study of the economic impact of immigration in receiving regions has been a highly researched area for the past 30 years and continues to attract much attention from academics and policymakers.[2] Recent large population displacements have renewed interest in analysing the effects of large immigration inflows across locations.[3] Within a vast empirical literature on the effects of immigration, a few papers have provided evidence on its impacts on (consumption) goods prices (Lach, 2007; Cortés, 2008; Balkan and Tumen, 2016), mainly finding negative effects. In the case of housing, which is an inelastic and non-tradable good, its price adjustment to an increase in local population might be different. For a given housing and local population stocks, an inflow of foreign-born population intensifies spatial competition on housing consumption, which may initially push prices up. In addition, population shocks might trigger internal migration across locations, affecting local demand further. The total (net) impact is the result of three adjustments: (1) increased demand from newly arrived immigrants, (2) additional demand changes from relocated population and (3) changes in housing conditions (density and construction). Ultimately, the sign and magnitude of the total demand effect on local average house prices is ambiguous (Saiz, 2007). Within this context, using Lewis and Peri (2015) terminology,[4] my paper aims to provide a framework to interpret the coefficients according to their partial or total effect on prices, enabling a better understanding of the total estimates.

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[2] Most of the theoretical and empirical contributions on this topic have originated from the analysis of their impact on labour markets, mostly on natives' employment and wages, with still controversial results. See Dustmann et al. (2016), Borjas and Monras (2017) and Clemens and Hunt (2019) for discussion on the current debate.

[3] For example, recent papers have looked at local impact of large refugee waves. See Tumen (2016) and Balkan and Tumen (2016) (from Syria to Turkey), Caruso et al. (2021) (from Venezuela to Colombia) and Trojanek and Gluszak (2022) (from Ukraine to Poland).

[4] Lewis and Peri (2015, pg 4–5): *"Traditionally the economic analysis has distinguished between short and long run effects of immigration. However, the so-called short-run effects are mostly a theoretical device to decompose a complex effect. When economists analyse the 'short-run effects' of immigrants they try to isolate the consequences of immigration when all other variables (including the stock of capital, the skill supply of natives and the technology and productive structure) are fixed. This should be called 'partial' effect. It is a way to understand and isolate a specific effect, not a way to forecast what happens, even in the short run"*. In fact, Saiz (2007) refers to long and short-run impacts when allowing for adjustments on native population and housing conditions or not.

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Most papers providing empirical evidence on the impact of immigration on house prices have estimated the net effect, paying little attention to the adjustments driving it. While many studies have found positive estimates of immigration on both house prices,[5] a handful of papers (Hatton and Tani, 2005; Saiz and Wachter, 2011; Accetturo et al., 2014; Sá, 2015) have found negative impacts of immigration on average house prices, in particular when focusing on smaller geographical areas. The displacement of natives from these areas ("native-fly") is the main argument used to explain these negative findings,[6] but, with the exception of Sá (2015), this channel is rarely explicitly estimated. In this paper, I provide novel evidence on the impact of immigration on house prices, proposing a method to tease apart the effect due to increased demand stemming from new arrivals ("partial effect") from additional demand changes from relocated natives ("induced effect"). The 2000s Spanish immigration wave is an ideal empirical setting to study this as the country experienced a large increase in foreign-born population shares between 2001 and 2012 (almost 10 percentage points, as shown in Fig. A.2(a)), coupled with an unprecedented housing boom. A few papers have studied the role of immigration on housing markets in Spain, using different levels of spatial aggregation with mixed findings (Sosvilla-Rivero, 2008; García-Montalvo, 2010). Notably, González and Ortega (2013) attribute one quarter of house price growth to (working-age) immigration inflows in Spain during the decade 2000–2010. These papers lack an analysis of how prices are adjusted in response to immigration inflows through different channels.

To formalise my framework, I start by showing that, under certain assumptions, the total impact of immigration on house prices is the net sum of two effects: (1) the impact from the direct increase in local population from the new arrivals ("partial") and (2) the additional changes from relocated population ("induced"), both affecting housing demand. The first component is theoretically positive and it resembles a reduced-form demand coefficient. The second component can be positive or negative, depending on the impact that immigrants have on native mobility. Both adjustments reflect demand changes as long as supply is accounted for in the estimation equations. The sum of both components determines the size and sign of the total "net demand" effect.

I use data for Spanish provinces between 2001 and 2012 to estimate the empirical counterparts of the decomposition, proceeding in three steps. In Step 1, I estimate a similar specification to Saiz (2007), regressing the annual local house price growth on the immigration rates. To be able to make causal claims about the estimates, I use a modified version of the standard immigration shift-share instrument and control for relevant local characteristics, time and province fixed effects, including housing supply conditions. The baseline estimated semi-elasticities for house prices is approximately 3.3%, for an increase in 1 percentage point (p.p.) of the immigration ratio.[7] These estimates correspond to the total effect.[8] In Step 2, I explicitly test the impact of immigrant inflows on native mobility and, in line with existing estimates for Spain (Fernández-Huertas et al., 2009), I find that immigrants attract natives to areas in which they locate (approximately 3 natives for each 10 immigrants).[9] To identify the impact of immigration on prices that is only due to increased immigrant housing demand ("partial" effect), in Step 3 I use population growth rate as the main regressor. I estimate the coefficient of this variable using solely the variation on population growth which is due to exogenous location of immigrants (predicted by the instrument). The estimated immigrant demand semi-elasticities for house prices using this methodology is 2.5%. The difference between the empirical estimates of steps 1 and 3 corresponds to the change in demand from natives locating in the region contemporaneously to the immigrant inflow, i.e. "induced" native demand.

The total and partial estimated coefficients correspond to demand effects when housing supply is accounted for in the regressions. In the estimation of the baseline results, which already include province fixed effects, I also include local attributes in flexible trends that relate to housing supply conditions. I further explore the impact that directly controlling for housing stock changes has on the estimates, using an instrument for changes in housing stock. I find that they have very little additional effect on the coefficients. Multiple tests on the validity of the instrumental variable strategy are provided in Section 5. The empirical results are robust across different specifications, to different constructions of the instrument and remain very similar when using long differences instead of year-to-year variations. In an Online Appendix (Section B.4), I propose a simple model to explain the mechanisms behind the results: an inflow of immigrants increases housing costs in the receiving region but, due to the specialisation of natives and immigrants on different production sectors, it also attracts natives to the region, increasing house costs even further. The model predicts that the total demand effect would be larger than the immigrant demand effect, which is what I find in the empirical exercise.

The results of the paper highlight the importance of taking into account local population mobility when interpreting the effect of immigration on house prices, or any other local outcome affected by population changes. The impact of population mobility on the identification of aggregate local effects gained renewed interest after the publication of Borjas (2003). This paper criticised studies on regional labour market impacts of foreign-born inflows, claiming that the United States worked as a single labour market and that the existence of mobility across areas could hinder the estimation of regional effects. The lack of local effects could be the result of the exit of native population after an inflow of immigrants, resulting in a net zero or very small change in local labour demand. As total housing demand changes are affected by direct and induced population inflows, if these have opposing signs, the net estimates might be close to zero but masking sizeable partial adjustments. Previous papers have relied on the existing US evidence to argue that native area displacement due to immigration is small or not large enough to cancel out increased demand stemming from increased area population so its impact on the estimates is irrelevant, and thus discussed total and partial effects as equivalent. However, my findings suggest that the impact of immigrants on native location can

[5] Saiz (2003, 2007), Ottaviano and Peri (2012) and Sharpe (2019) provide estimates for the USA. Other studies include Hyslop et al. (2019), who provide positive impact estimates for New Zealand; Degen and Fischer (2017), who find positive effects for Switzerland; Moallemi and Melser (2020) who find positive impacts in Australia and Akbari and Aydede (2012) who find positive but small impacts in Canada. Tumen (2016) studies the impact of the Syrian refugees' inflow on Turkish housing rents and finds a positive effect on high-quality units and no effect on low-quality units. Cochrane and Poot (2021) provide a recent review.

[6] An exception here is Monras (2020), who attributes the decrease in house prices in the long run to reductions in costs due to the large increase of supply of construction workers associated with Mexican immigration.

[7] The impact on average rents is smaller, around 1%, due to the nature of the data and the institutional framework. Price data is based on transactions (flow), while rent data is based on average price on rented properties (stock), hence most of the variation in the second measure stems from new tenancy contracts, a limited fraction of the stock. Moreover, during this time period the maximum annual rent increase was limited to the national price index (CPI).

[8] My estimates are directly comparable to Saiz (2007) results of around 0.9% for rents and 3.3% for prices, and similar to other existing estimates (Larkin et al., 2019).

[9] This sizeable co-location estimate is rare, but not unique in the literature. In fact, even in the US the evidence on significant native displacement is not as robust as some authors have claimed, as thoroughly discussed in Peri and Sparber (2011). Mocetti and Porello (2010) and Wozniak and Murray (2012) find similar size estimates of co-location of natives and immigrants. In Spain, Fernández-Huertas et al. (2019) find co-location of natives and immigrants in newly developed neighbourhoods, were most of the construction of new dwellings too place between 2001 and 2012.

be non-negligible, so we need to be more careful about making these claims.

My paper makes several contributions. First, from combining the estimating equations I show that the coefficient that captures total demand changes can be formally decomposed as the sum of direct (immigrant) demand changes plus additional demand shifts from relocated population (induced). It presents a framework to understand better the demand adjustments on local house prices following a large immigration inflow. This is the first paper to provide causal estimates of all the elements of this decomposition, and in particular, to identify the relationship between them. Second, to obtain causal counterparts of the decomposition elements I construct a shift-share instrument that combines historical immigrant location patterns with predicted national inflows by country of origin obtained from a push-factors gravity model. This is an improved version of the standard ethnic networks instrument widely used in the immigration literature and it is the first time it is applied to Spanish data. This modification of the traditional shift-share immigration predictor addresses many of the concerns raised in the recent shift-share instruments literature.[10] Finally, in order to be able to interpret the results in terms of changes of prices in equilibrium in my estimations I partial out housing supply conditions. The main results are estimated including province fixed effects and additional province-level supply-related attributes interacted with year dummies. By doing this, the coefficients correspond to demand impacts, which makes their interpretation more straightforward.

The rest of the paper is organised as follows. Section 2 provides the formal decomposition of the total impact. Section 3 explains the methodology: the empirical specifications (3.1), how the components of the decomposition can be estimated (3.2), the instrumental variables (IV) strategy (3.3) and the data and descriptive statistics (3.4). Section 4 describes the main regression results. In Section 5 I discuss and test the validity of the identification strategy. Section 6 provides additional results and robustness checks for the main findings. Finally, Section 7 presents the conclusions.

2. Decomposition of the effect of immigration on house prices

2.1. Total and partial effects

Let us assume a simple supply–demand framework where the observed local average house price is determined by the demand from local population and by the level of housing stock. When supply is fixed, changes in local average prices would occur when there are changes in the level of population in the province (by shifting housing demand). I focus on large local inflows of foreign-born population as the main driver of local population changes. In my context, I do this for two reasons. First, Spain experienced unprecedented population change during the first decade of the 2000s. Moreover, during the period of analysis (2001–2012) most of the population changes in Spain stemmed from foreign-born population inflows (around 76%), especially before 2009. While the national annual growth rate of foreign-born population was on average over 13% (with peaks around 30% between 2001 and 2003), annual growth rates of native population remained stable and very low throughout (around 0.25%). Putting this together, over the period the average population growth rate amounted to around 1% per year, driven by the immigration inflows. Second, for local changes in foreign-born population it is possible to construct a plausible source of exogenous variation, which is key in the empirical exercise and for the results in this section. One might want to assess the impact of changes of total local population (demand) on housing costs. However, as discussed by Sharpe (2019), finding a credible instrument to estimate the impact of *total population changes* on local prices is very difficult, and using two instruments for immigrants and natives separately is also problematic (Angrist and Pischke, 2009). What can plausibly be done is to capture changes in local population *driven by immigration*, isolating the impact of immigration on outcomes via their effect on total local inhabitants. This can be achieved using the variation on total population driven by the exogenous source of immigration inflows.

The total impact of changes in foreign-born population on house prices has been previously estimated using variations of the Saiz (2007) empirical specification:

$$\Delta \log \left(hpr_{i,t}\right) = \beta_1 \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \theta^1_{i,t} + \epsilon^1_{i,t} \tag{1}$$

In this equation, β_1 captures the total impact of immigration on prices: the total adjustments due to the immigration inflow plus additional changes in native location, where changes in housing supply conditions are to be accounted for by θ^1, while ϵ^1 is a well-behaved error term. $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$ is the normalised immigration inflow during $t-1$ (or immigration rate) and it is measured in percentage points. β_1 is interpreted as the percent change in house prices for an increase in the rate of one percentage point.

Saiz (2007) discusses three channels affecting this "long-run" (total) adjustment of local house prices. Initially, an increase in foreign-born population in a given location raises total population and then pushes demand and prices in the "short run". I refer to this impact as the partial or direct demand effect. The "long-run" (total) impact on prices also depends on further changes on (1) housing supply conditions (density and construction) and on (2) the mobility of natives or previous residents following the immigration inflow (relocation).[11] An estimate of β_1 captures the combination of all these changes. The use of instruments and controls would produce unbiased estimates of the total effect coefficient, but it does not help with interpreting the channels driving it. If we want to disentangle the partial effects, we can try to control for the different adjustments. After controlling for housing supply conditions, an unbiased estimate of β_1 would capture the changes in housing demand stemming from the newly arrived population (partial) plus any changes in demand related to natives relocating *due to* the immigration wave (induced). Saiz (2007) argued that the impact of immigrant demand could not be separated from other demand changes induced by native population adjustments, affecting the interpretation of the estimated semi-elasticities.[12] Hence, the main contribution of my paper is to show that it is indeed possible to separately estimate the partial and induced changes in housing demand, and to show formally how these estimates are related to each other.

When estimating local average impacts of immigration inflows, for example on local average wages, one needs to take into account that changes in population in a given area affect the whole regions-cities system equilibrium. The relocation of population across regions within a country could hinder the identification of any area-level effects, as

[10] Notably Jaeger et al. (2018), Adao et al. (2019), Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022).

[11] The theoretical discussion in Saiz (2007) uses short-run and long-run adjustments terminology. By short-run he refers to a situation where native mobility and housing supply cannot adjust and, by long-run when they do. In the empirical results of his paper he uses both annual first-differences models and decennial long-differences models, but the interpretation of the coefficient is always the same: the total (reduced-form) effect. In fact, as discussed in Lewis and Peri (2015), when economists refer to the "short-run" effects of immigrants, they try to isolate the consequences of immigration on one variable keeping the rest fixed. These authors refer to this as a "partial" effect. Following this, hereby I refer to "total" effect when I allow for adjustments in all channels and to "partial" effect when I focus on the direct increase in demand from recently arrived population.

[12] *"There is no way to separate the effect of increased housing demand (immigration) from the potential decreased demand associated with potential native out-migration. Part of the local response to the treatment (immigration) can occur through native out-migration. In this case, we need to be careful about the interpretation of the coefficient of immigration on rents"*. (Saiz, 2007, pg. 348).

the effects of a local immigration inflow would dissipate throughout the country if natives relocate (Monras, 2020). If large population outflows are triggered by immigration, the net area impact estimates would tend towards zero. In the analysis of the impact of immigration on local labour market outcomes, the existence of "native displacement" has been used as the main argument for the lack of robust estimates of the impact of immigration on wages across US labour markets (Borjas, 2006). When displacement exists, cross-region regressions would underestimate the effect of immigrants on local labour markets. However, native displacement might be quantitatively small, as discussed in Peri and Sparber (2011). The existence of spillovers or displacement is also an important issue in the estimation of place-based policies impacts (Neumark and Simpson, 2015).

In this paper I show that native mobility can also affect the estimation of the average effect of immigration on local house prices. In the immigration literature, the impact of mobility on the interpretation of the net area estimates has been generally inferred from the sign of the total impact estimate. If the reduced-form estimate is positive it is assumed that natives are little or not "sufficiently" displaced by immigrants (González and Ortega, 2009). Thus the sign of the total effect is interpreted as a test of "native displacement" (Saiz, 2007). However, the impact of immigrants on native mobility is in most cases not directly estimated in these studies. The specific relationship between total and partial effects is derived and discussed in the next section.

2.2. Formal decomposition of the total effect

The first step to derive the channels behind the total impact of immigration on prices is to define the relationship between price growth and population growth:

$$\Delta \log\left(hpr_{i,t}\right) = \beta_2 \left(\frac{\Delta POP_{i,t-1}}{POP_{i,t-2}}\right) + \theta^2_{i,t} + \epsilon^2_{i,t} \tag{2}$$

where $\left(\frac{\Delta POP_{i,t-1}}{POP_{i,t-2}}\right)$ is the local population growth rate. $\theta^2_{i,t}$ includes variables that relate to housing supply conditions, as well as time and area fixed effects. In the empirical exercise, I use plausibly exogenous variation in local foreign-born stocks to estimate β_2. Hence, this coefficient captures the effect of immigration in prices through its impact on local population. Importantly, it is not the effect of changes of *total population* on prices because, as explained above, it is very difficult to find a credible identification strategy to causally estimate this effect. By including supply controls in the specification, it captures the impact via changes in housing demand.

Changes in local population are by definition the sum of changes in foreign-born and natives:

$$\Delta POP_{i,t-1} \equiv \Delta FB_{i,t-1} + \Delta NAT_{i,t-1} \tag{3}$$

Replacing (3) into (2):

$$\Delta \log\left(hpr_{i,t}\right) = \beta_2 \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \beta_2 \left(\frac{\Delta NAT_{i,t-1}}{POP_{i,t-2}}\right) + \theta^2_{i,t} + \epsilon^2_{i,t} \tag{4}$$

This expression decomposes local population growth into the two components that can be affected by immigration: immigrant $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$ and native population $\left(\frac{\Delta NAT_{i,t-1}}{POP_{i,t-2}}\right)$ rates.

The following equation defines how native rates are affected by immigrants:

$$\left(\frac{\Delta NAT_{i,t-1}}{POP_{i,t-2}}\right) = \beta_3 \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \theta^3_{i,t} + \epsilon^3_{i,t} \tag{5}$$

where β_3 captures the reaction of native mobility to an immigration inflow and its sign and size would inform about the existence of displacement or co-location. Plugging (5) into (4) and rearranging:

$$\Delta \log\left(hpr_{i,t}\right) = \left(\beta_2 + \beta_2\beta_3\right) \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \beta_2\theta^3_{i,t} + \theta^2_{i,t} + \upsilon_{i,t} \tag{6}$$

This expression shows that, when we use the components of local population, we can decompose the impact of immigration on prices in terms which depend on parameters β_2 and β_3, and other terms such as $\theta^2_{i,t}$, $\theta^3_{i,t}$ and $\upsilon_{i,t}$, where $\upsilon_{i,t} = \left(\beta_2\epsilon^3_{i,t} + \epsilon^2_{i,t}\right)$. Coefficient β_2 is the effect of immigration in prices through its impact on local population (partial or *direct* demand impact) and β_3 is the impact of immigration rates on native population rates (*mobility*). The interaction of both parameters, $\beta_2\beta_3$, captures the changes in local demand due to natives relocating following the immigration wave (*induced* demand).

In order to obtain an expression that relates the total impact to its partial adjustment components, I turn to the empirical counterparts of Eqs. (1) and (6). I start by differentiating equation (1) with respect to changes in the immigration rate:

$$\frac{\partial \Delta \log\left(hpr_{i,t}\right)}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} = \beta_1 + \frac{\partial \theta^1_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} + \frac{\partial \epsilon^1_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} \tag{7}$$

Then, I also differentiate equation (6) with respect to changes in the immigration rates:

$$\frac{\partial \Delta \log\left(hpr_{i,t}\right)}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} = \beta_2 + \beta_2\beta_3 + \beta_2 \frac{\partial \theta^3_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} + \frac{\partial \theta^2_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} + \frac{\partial \upsilon_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)} \tag{8}$$

By definition, $\epsilon^1_{i,t}$, $\epsilon^2_{i,t}$ and $\epsilon^3_{i,t}$ (in $\upsilon_{i,t}$) are uncorrelated with $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$, so the last terms in Eqs. (7) and (8) would be zero. The partial derivatives of the θ terms with respect to immigration rates would capture any biases in the estimation of consistent β_1, β_2 and β_3. $\frac{\partial \theta^1_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)}$ captures the correlation between variables related to immigration rates and house prices in Eq. (1). The same applies to $\frac{\partial \theta^2_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)}$: it captures the impact of variables correlated with changes in house prices and changes in population (from immigration inflows) in Eq. (2). Finally, $\frac{\partial \theta^3_{i,t}}{\partial\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)}$ captures the correlation between variables related to native and immigration rates at the same time in Eq. (5). These variables can include observables (which can be accounted for) and un-observables. I deal with omitted variable bias and fixed unobservables by using a large set of controls and time and area fixed effects. To eliminate endogeneity bias from un-observables I use an IV strategy, discussed in detail in Section 3.3. Therefore, in the empirical counterparts of these terms, the θ terms would not directly depend on the immigration rate and would be zero.

When the coefficients are consistently estimated we can match (7) and (8) to decompose the total impact $\hat{\beta}_1$ as follows:

$$\hat{\beta}_1 = \hat{\beta}_2 + \hat{\beta}_2\hat{\beta}_3 = \hat{\beta}_2\left(1 + \hat{\beta}_3\right) \tag{9}$$

where $\hat{\beta}_2$ is the impact of immigration on prices via its impact on the size local population changes ("partial demand" impact) and $\hat{\beta}_3$ is the impact of immigration on local native population changes (native mobility or the so-called "native-displacement"). The term $\hat{\beta}_2\hat{\beta}_3$ captures the changes in prices that are due to additional changes in demand from relocated (native) population ("induced demand" impact). This term can be positive or negative depending on how native mobility is affected by immigrants. $\hat{\beta}_1$ is the total impact which captures the net sum of all these changes. These coefficients correspond to demand effects as long as supply conditions are partialled-out in the estimation.

Coefficient $\hat{\beta}_2$ can be interpreted as the price semi-elasticity with respect to changes in local population, which in our setting is estimated from an exogenous change in the foreign-born population (predicted by the instrument), and it is expected to be positive (for an inelastic normal good). It is the partial impact, when we do not consider additional adjustments in demand and we control for adjustments in supply. The sign and size of the total impact $\hat{\beta}_1$ depends on the term $\left(1 + \hat{\beta}_3\right)$, which captures the impact of native mobility on additional changes in local demand. This last term could be negative (if immigrants displace natives more than one-to-one), positive but smaller than one (if immigrants displace natives but not one-to-one), one (if immigrants have no impact on native mobility), or greater than one (if immigrants and native co-locate).

If we ignored the impact that native mobility has on changing the total demand in the region via its impact on natives mobility, we could assume that $\hat{\beta}_1$ (total) and $\hat{\beta}_2$ (partial) are the same and that $\hat{\beta}_1$ corresponds to the changes in housing demand from immigrants. This would only be the case if natives are unaffected by immigrant inflows, e.g. $\hat{\beta}_3 = 0$. In reality, only the partial effect coefficient corresponds direct demand changes, while the total (demand) impact includes changes in demand from the exogenous change in population (from the immigrant inflow) and from endogenous change in (native) population. The decomposition provided in this paper clarifies this issue and the results in this paper provide the first joint estimates of all three impacts in a relevant context. The next sections explain the methodology used to obtained consistent coefficients of the three effects and shows that the decomposition in Eq. (9) holds exactly in the data.

3. Methodology

3.1. Empirical specifications

To obtain the estimates of Eq. (9) we estimate the following models:

$$\Delta \log\left(hpr_{i,t}\right) = \beta_1 \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \lambda_t + \gamma_i + \phi'\left(Z_i * \lambda_t\right) + \delta' \Delta X_{i,t-2} + \varepsilon^1_{i,t} \tag{10}$$

$$\Delta \log\left(hpr_{i,t}\right) = \beta_2 \left(\frac{\Delta POP_{i,t-1}}{POP_{i,t-2}}\right) + \lambda_t + \gamma_i + \phi'\left(Z_i * \lambda_t\right) + \delta' \Delta X_{i,t-2} + \varepsilon^2_{i,t} \tag{11}$$

$$\left(\frac{\Delta NAT_{i,t-1}}{POP_{i,t-2}}\right) = \beta_3 \left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right) + \lambda_t + \gamma_i + \phi'\left(Z_i * \lambda_t\right) + \delta' \Delta X_{i,t-2} + \varepsilon^3_{i,t} \tag{12}$$

Eq. (10) estimates the total impact of immigrants on house prices, Eq. (11) the partial immigrant demand (direct) and (12) the impact of immigration on native mobility. The geographical units of observation are the 50 Spanish provinces i and t denotes time periods (years 2002 to 2012). I exclude the African territories due to their historical particularities and the lack of reliable data. The identification strategy aims to produce causal consistent estimates of β_1, β_2 and β_3.

$\Delta \log(hpr_{i,t})$ is the change of the natural logarithm of average house prices in province i during year t (approximately the growth rate), while $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$ is the immigration rate during $t-1$ (and similarly for natives (NAT) and total population (POP)). The inflow of immigrants during $t-1$ is calculated as the change in the foreign-born population between the end of $t-1$ and the end of $t-2$. The same applies when I use native or total population changes.[13] Similarly to other authors, in the main results I use the immigration inflows lagged one period with respect to changes in prices.[14] Hence, inflows in the sample correspond to years 2001 to 2011 ($t-1$), while price growth rates to years 2002 to 2012 (t). λ_t are time fixed effects, γ_i are province fixed effects, $\Delta X_{i,t-2}$ is a matrix of province time-varying controls and Z_i is a matrix of province time-invariant attributes. In the most demanding estimations, I include the province attributes interacted with time dummies ($Z_i * \lambda_t$). Finally, $\varepsilon_{i,t}$ is the random error term. In this set-up the coefficients of interest in Eqs. (10) and (11) are interpreted as a semi-elasticity: an increase in the rate of one percentage point has an effect on the change in prices of β percentage points. In Eq. (12), β_3 captures the number of natives that relocate for each additional immigrant (Peri and Sparber, 2011). Note that in this specification, differently from the other two, the effect of the immigration rate is contemporaneous.

I am estimating the relationship between immigration and house prices in growth rates and population changes. The first differences setting of Eqs. (10)–(12) already eliminates any unobservable province characteristics which might be correlated with the outcomes and the population rates *in levels*. However, there could still exist some confounders at the area level which are correlated province outcomes and treatments *in changes*. These could be of two types: unobservable and observable province characteristics. Vector Z_i contains time-invariant province attributes that control for the fact that provinces with different levels of these characteristics might have different growth trends. This includes variables related to the nature of housing supply (share of residential secondary homes, share of residential empty homes, share of households which own a secondary home) and housing consumption (share of renters and log average home square metres per person; for foreign-born and for natives separately) in 2001. I also include information about the economic structure in 2001 (share of employed in construction sector and share of employed in services sector) and some other attributes potentially correlated with location decisions and price growth (log road distance to Madrid, length of coastline, log of rain precipitation in January). Finally the share of developable land and average ruggedness index in the province aim to capture the factors related to the potential growth of housing supply (construction). When I use province fixed effects the province attributes included in Z_i drop. In the most demanding specification, I include province fixed effects and the above province attributes interacted with time dummies, which control for differential growth trends by level of attribute ($Z_i * \lambda_t$).

Vector $\Delta X_{i,t-2}$ contains time-varying province characteristics (in changes). I control for changes in output per capita, conditions in the financial sector, unemployment rate, education levels and infrastructure endowments. As contemporaneous changes of these factors could well be the result of population changes ("bad controls"), I use an additional lag with respect to population rates variable, e.g. changes in the variables during $t-2/t-3$ (one period before the inflows in ($t-1$) and two periods before the change in prices ($t/t-1$)). The results change very little if the time-varying controls are contemporaneous to the population rates or if these are excluded.

Other factors potentially inducing bias in the estimates could be unobservables. Time fixed effects λ_t control for common unobserved shocks affecting all Spanish provinces in a given year. Province fixed effects γ_i control for time-invariant province heterogeneity. When including both sets of fixed effects, the specification corresponds to a first differences fixed effects estimation (e.g. growth regression with fixed effects). In this model, the coefficients are estimated off the within-province time variation in outcome and treatment changes, conditional on the controls and time fixed effects. To deal with time-varying unobservables I use an IV strategy which is described in Section 3.3.

[13] Using normalised inflows instead of (log) net inflows as the measure of "immigration" eliminates any unobservables that might equally affect both the numerator (immigration inflow) and the denominator (original province population). Standardising immigration inflows by initial population stock also deals with the fact that regions of different sizes have different population and house price dynamics (Card, 2001; Peri and Sparber, 2011; Wozniak and Murray, 2012). Scale effects can induce spurious correlation between higher inflows and higher price changes. This correlation could arise due to the fact that the average and standard deviation of both variables are likely to be proportional to the total population in the province. In addition to the standarisation, I control for the effect of initial population trends by either including it directly in the specifications or by using province fixed effects. Additionally, this format allows a more straightforward comparison to existing estimates in papers that have used similar specifications, and the interpretation of the coefficients as semi-elasticities.

[14] I also investigate other lag structure as a robustness test in Table 10.

Table 1
Residential density in Spain 2001–2012.

	All provinces			Low immigration			High immigration		
Year	Population	Housing stock	Housing stock/pop	Population	Housing stock	Housing stock/pop	Population	Housing stock	Housing stock/pop
2001	41,692,558	20,988,378	0.545	15,940,732	7,728,691	0.520	25,751,826	13,259,687	0.571
2002	42,573,670	21,440,413	0.549	16,044,778	7,877,993	0.527	26,528,892	13,562,420	0.570
2003	43,055,014	21,878,187	0.554	16,105,694	8,018,749	0.535	26,949,320	13,859,438	0.573
2004	43,967,766	22,368,785	0.555	16,241,244	8,177,697	0.541	27,726,522	14,191,088	0.570
2005	44,566,232	22,877,640	0.560	16,346,618	8,336,402	0.548	28,219,614	14,541,238	0.573
2006	45,054,694	23,443,569	0.568	16,419,649	8,519,092	0.558	28,635,045	14,924,477	0.579
2007	46,008,985	23,983,886	0.571	16,581,611	8,709,850	0.565	29,427,374	15,274,036	0.578
2008	46,593,673	24,518,341	0.580	16,688,538	8,898,996	0.574	29,905,135	15,619,345	0.585
2009	46,864,418	24,856,498	0.587	16,739,433	9,024,952	0.582	30,124,985	15,831,546	0.591
2010	47,029,641	25,054,029	0.591	16,771,796	9,113,294	0.587	30,257,845	15,940,735	0.594
2011	47,100,501	25,196,069	0.595	16,756,995	9,182,064	0.593	30,343,506	16,014,005	0.598
2012	46,961,924	25,328,848	0.603	16,678,085	9,234,733	0.601	30,283,839	16,094,115	0.605

Notes: Spanish Department of Housing and Annual Population Registers. High/low immigration status is defined by being above or below the median in the foreign-born inflow (2001–2011) over population in 2001 (0.0783). The low immigration provinces are (sorted from lower to higher): Caceres, Jaen, Cordoba, Cadiz, Orense, Badajoz, Zamora, Lugo, Palencia, La Coruña, Salamanca, Pontevedra, Leon, Asturias, Sevilla, Valladolid, Vizcaya, Albacete, Guipuzcoa, Granada, Cantabria, Avila, Ciudad Real, Burgos and Huelva. The high immigration provinces are (sorted from lower to higher): Alava, Soria, Navarra, Valencia, Teruel, Segovia, Huesca, Cuenca, La Rioja, Las Palmas, Zaragoza, Santa Cruz de Tenerife, Murcia, Barcelona, Toledo, Madrid, Castellon, Malaga, Baleares, Lleida, Tarragona, Girona, Alicante, Almeria, Guadalajara.

3.2. Estimation of the components of the decomposition

In this section I describe the empirical issues related to the estimation of the components of the decomposition and their interpretation as total or partial demand estimates. This relates to the consistent estimation of the coefficients, by using plausible exogenous variation, and controlling for supply-related factors in the estimation. First, for Eq. (9) to hold, the three β coefficients must be consistently estimated. The IV estimation of the β_1 coefficient in (10) yields a consistent estimate of the total impact of immigration on prices. To obtain the right-hand-side components on (9) I also need to estimate coefficients β_2 (partial demand impact) and β_3 (native mobility).

As suggested by Peri and Sparber (2011), to test the impact of immigration on native mobility I use a normalised change in native population in the left-hand-side and estimate Eq. (12). The sign and size of $\hat{\beta}_3$ captures the relationship between immigration inflows and native location and measures how many natives relocate in response to one immigrant arrival. If a sizeable causal relationship exists, we need to be more cautious about the interpretation of $\hat{\beta}_1$. The results of the estimation of the impact of immigration on native mobility are discussed in Section 4.2.

In Section 2 I refer to the interpretation of $\hat{\beta}_1$ and $\hat{\beta}_2$ as total and partial *demand* coefficients. This interpretation relies on conditioning out housing supply conditions (density and construction) when estimating these parameters. In order to do this, I first explore how housing density evolved during this period. I define provinces with *high* and *low* immigration status during the 2001–2011 period, where we classify the province over and below the median immigration rate between 2001 and 2011. As observed in panel (a) of Fig. A.1, although both groups experienced growth, there were significant differences in the increase of the foreign-born population share over time.

Table 1 presents data on the total population and total housing stock, and also the per-person housing stock from 2001 to 2012, total and by high-low immigration status. Over the period, both aggregate variables experienced substantial growth, but their ratio, housing stock per person, remained relatively stable, increasing only slightly. This suggests that the significant influx of immigrants was accompanied by intensive construction, which helped to maintain a relatively constant rate of housing density. Although the table indicates that in 2001 there were significant differences in the average housing stock per person between provinces with high and low immigration (5 p.p), panel (b) in Fig. A.1 shows that these differences were not statistically different in any year during the period and had converged by 2012. Therefore, I consider the province fixed effects are sufficient to control for differences in housing density across provinces over time. Furthermore, I control for differential housing consumption patterns by immigrants and natives by including average square metres per person in 2001, either in levels or interacted with year dummies.

This table also shows that a large number of housing units were built between 2001 and 2012 (over 5.2 million, more than 400,000 per year). House construction could be directly correlated with immigration inflows if immigrants locate in areas where it is higher. I account for the effect of housing supply on house prices in two ways. Firstly, in the estimation of (10)–(12) I include a set of time-invariant province attributes and trends related to housing supply. However, if I want to truly condition-out the impact of changes on housing supply on price growth, I need to include time-varying changes in housing supply as an additional control variable. This variable would remove potential bias arising from the fact that immigrants might be locating in areas where construction is growing faster (to work in this sector or due to higher availability of homes) and that house construction also affects housing costs via increasing supply of housing units. Including time-varying supply changes in the estimation as an additional control is very problematic, because even if lagged, housing construction is a "bad control" given that construction is directly affected by immigration.[15] In Section B.3, I discuss the impact that directly including a measure of changes in housing stock as an additional control (also instrumented) has on the estimates. Given the demanding empirical specification and the set of fixed effects and controls, the impact of adding this additional variable on the main estimates is negligible. As including this additional control barely affect the main estimates, I refrain from doing so and rely on the specifications (10)–(12) as specified above.

Once I have considered the impact of supply conditions on the total estimates, I need to apply a method to pin down the partial demand coefficient β_2. To recover unbiased estimates of the total and partial effects I implement a shift-share-type IV strategy explained in Section 3.3, while including controls, fixed effects and trends in the specifications. To isolate the variation in total population that arises only from immigrant inflows, I replace immigration rate in (10) by a normalised population inflow variable (or population growth rate) and estimate the effect of changes in population using solely the variation which is due to exogenous location of foreign-born (predicted by the instrument). In practice, this corresponds to the estimation of model (11) using an instrument for $\left(\frac{\Delta POP_{i,t-1}}{POP_{i,t-2}}\right)$. When I do this, $\hat{\beta}_2$ is estimated from exogenous variation in local population which is only related to the arrival of immigrants, and thus I am able to separate the

[15] González and Ortega (2013) find a sizeable impact of immigrants on home construction in Spain during a similar time period.

partial demand changes from other adjustments in demand due to other population relocation.

3.3. Construction of the immigration rate instrument

Even after including fixed effects and control variables, a consistent estimation of the β coefficients requires the regressors of interest to be uncorrelated with the time-varying part of the error term. Unobserved factors could still induce omitted variables or endogeneity biases. There is no prior on the direction of the bias. For example, in the case of the total impact (10), the estimated β_1 could be upward biased if immigrants are going to provinces with positive shocks or unobserved better economic prospects, while it would be downward biased if, for some reason, immigrants locate in province in which prices are growing slower (conditional on all the controls). To estimate consistent causal parameters I use an instrumental variables (IV) strategy.

The instrument I construct is an imputation (or prediction) of the immigration rate: $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$, which is defined as the immigration inflow during $t-1$ divided by total population at the end of $t-2$. The immigration rate in province i that I want to instrument for is:

$$IMM\_rate_{i,t-1} = \frac{\Delta FB_{i,t-1}}{POP_{i,t-2}} = \frac{FB\_inflow_{i,t-1}}{FB_{i,t-2} + NAT_{i,t-2}} = \frac{FB_{i,t-1} - FB_{i,t-2}}{FB_{i,t-2} + NAT_{i,t-2}} \tag{13}$$

Each component of this rate has to be imputed. I construct the instrument adapting the "shift-share" methodology, which has extensively been used before, for instance by Card (2001) or Ottaviano and Peri (2006). Intuitively, a province-year immigrant stocks ($FB_{i,t}$) imputation is constructed by distributing year-to-year variation on the national stocks of immigrants by country of birth (the "shift" or "shock") across different areas, using some location pattern (the "share") to allocate this magnitude. The most commonly used shift-share instrument builds up on the fact that, to take advantage of social and economic established networks, immigrants tend to disproportionately locate in areas where immigrants from the same nationality or ethnicity have located before (ethnic networks instrument). I use past (1991) location patterns by country of birth ("share") to predict current location patterns. For the national yearly immigration inflow ("shift") by country of birth, I use country-of-origin-specific predicted inflows based on a gravity push factor model. The product of the shift-share produces annual imputations of the stock or inflow of foreign-born for each nationality in each province in each year. To calculate the predicted province $FB_{i,t}$ stocks in (13), I sum these imputations over nationalities.

To compute the annual immigrant population by province and by country of origin, I first calculate the share of immigrants that were located in that province in the base year.[16] This share corresponds the proportion of immigrants located in a particular province i over the total immigrants from the same nationality n in Spain in 1991 (sum r provinces over a total of $R = 50$):

$$share^n_{i,1991} = \frac{FB^n_{i,1991}}{\sum_r^R FB^n_{r,1991}} = \frac{FB^n_{i,1991}}{FB^n_{Spain,1991}} \tag{14}$$

The imputed foreign-born stock of a specific nationality n in province i at time t, $imp\_FB^n_{i,t}$ is calculated allocating yearly total national stocks ($FB^n_{Spain\_i,t}$) by nationality weighted by their historical location share calculated as (14). I then sum this across nationalities to calculate the total imputed foreign-born stock in province i at time t, $imp\_FB_{i,t}$:

$$imp\_FB_{i,t} = \sum_n^N \left(share^n_{i,1991} * FB^n_{Spain\_i,t}\right) = \sum_n^N \left(imp\_FB^n_{i,t}\right) \tag{15}$$

[16] The nationalities used are listed in Table B.6 of the Online Appendix.

For the shift-share instruments to be valid and yield consistent estimates two conditions must be met. They need to be relevant (sufficiently correlated with the variable they predict) and conditionally exogenous (uncorrelated with unobserved shocks). The relevance of the instrument can be assessed by the value of the F-statistic of the instrument in the first stage of the 2-stage-least-squares (2SLS) regressions, and additionally by using weak identification tests. Despite concerns about the low level of immigrant stocks in specific nationality-province pairs in 1991 (Jaeger et al., 2018), the results tables' F-statistics demonstrate that the instrument is sufficiently strong, even after controlling for a substantial number of variables and province fixed effects.

For the exogeneity condition to be met, both elements involved in the construction of province yearly immigrant predictions must be orthogonal to local shocks related to the outcome variable, conditional on controls. Regarding the local share of immigrants by nationality in the base year, the exclusion restriction requires that the only channel through which foreign-born geographical distribution in 1991 affects current changes in house prices/native locations is through its influence on shaping the current immigrants location patterns, conditional on controls and province and time fixed effects. In other words, the unobserved factors determining the location of immigrants in one province with respect to another in the base year (1991) have to be uncorrelated with the relative economic prospects of the two provinces during the period of analysis (2001–2012). I consider 1991 to be separated enough from 2001 for this condition to be reduced, and the province fixed effects to capture across-provinces heterogeneity correlated to past location patterns to a great extent. Nevertheless, in Section 5, I perform a number of tests to assess the validity of the choice of 1991 as base year.

We also require that the national total stock of immigrants by country of birth in a given year, $FB^n_{Spain\_i,t}$, is exogenous to specific province unobservable shocks. This magnitude is the sum of all Spanish province stocks of a given country of birth in a given year. It is unlikely that shocks that have driven immigrants of different nationalities to specific locations in a given year (the magnitude that we are instrumenting for) are uncorrelated to shocks in neighbouring provinces. To solve this issue, a similar strategy to Saiz (2007) and Ortega and Peri (2016) is adopted. I predict yearly national stock and inflow of immigrants by country of origin from the results of estimating gravity migration model which depends only on push factors in origin. Details of this procedure are given in the appendix (Section B.2.1). I use these models to obtain predictions of foreign-born stocks and inflows in years 2001 to 2012 for each nationality. I multiply this by the shares and sum across nationalities to obtain province-year imputations of foreign-born stocks and inflows.

To construct the instrument for the immigration rate (13), in the numerator I use an imputed prediction of foreign-born inflows ($imp\_pred\_FB\_inflow_{i,t-1}$). In the denominator I need to compute a (lagged) prediction of population stocks. This is composed of foreign-born stocks and native stocks ($FB_{i,t-2} + NAT_{i,t-2}$ in Eq. (13)). For the first component, I can use the lagged imputed prediction of foreign-born stocks ($imp\_pred\_FB_{i,t-2}$). However, as discussed in Section 3.2, the number of total natives residing in a given province might depend on the number of foreign-born in the same location or on unobservables correlated with house price growth. For this reason, I use a similar shift-share strategy to compute a prediction for the location of natives $imp\_NAT_{i,t-2}$, based on past location patterns. Details on this procedure are also given in the appendix (Section B.2.2).

With all this, I compute the instrument as the ratio of the imputed FB inflow and the imputed population (imputed FB stock plus imputed native population stock). Formally this is:

$$IV\_IMM\_rate_{i,t-1} == \frac{imp\_pred\_FB\_inflow_{i,t-1}}{imp\_pred\_FB_{i,t-2} + imp\_NAT_{i,t-2}} \tag{16}$$

Prediction (16) is used to instrument $\left(\frac{\Delta FB_{i,t-1}}{POP_{i,t-2}}\right)$ in Eqs. (10) and (12) and to instrument $\left(\frac{\Delta POP_{i,t-1}}{POP_{i,t-2}}\right)$ in Eq. (11). I use $IV\_IMM\_rate_{i,t-1}$ in

Table 2
Housing prices and population rates summary statistics.

Variables	Time period	Mean	Std. Dev.	Min	Max
Δ Log Sale Prices (t)	2002/01–2012/11	0.047	0.094	−0.157	0.276
Δ Log Rent Prices (t)	2002/01–2012/11	0.028	0.017	−0.014	0.083
Population rate ($t-1$)	2001–2011	0.010	0.012	−0.012	0.061
Immigration rate ($t-1$)	2001–2011	0.009	0.008	−0.005	0.046
Natives rate ($t-1$)	2001–2011	0.002	0.006	−0.017	0.031

the main IV estimation results and different versions of it in robustness checks. In Section 5 I discuss and test the validity of this IV approach and in Section 6.2 I check if the results are robust to using different definitions of the shift and share in the construction of the instrument.

3.4. Data and descriptive statistics

To investigate the impact of immigration on housing costs, I exploit a panel of Spanish provinces for the period 2001–2012. This setup is suitable to study this question because during this period large immigration inflows were coupled with a period of housing sector boom (2001–2007) followed by a bust (2008–2012), which provides large variation in the data. Average annual rent and house price growth during the period was around 3 and 5%, while the average population growth rate was around 1%, mostly due to foreign-born population growth. Fig. A.2 shows the evolution of immigrant stocks and inflows and of housing costs during these years. Foreign-born stock increased from around 2.5 million people in 2001 to 6.7 million in 2012, an increase of almost 160%. In these years, the share of foreign-born over total population rose almost 10 percentage points (from 4.8% to 14%). This increase was particularly remarkable for high-immigration provinces (as shown in panel (a) of Fig. A.1). Between 2001 and 2008, the annual inflows of foreign-born were over 400,000 persons per year, and even after the start of the recession they remained between 70 and 100,000. With respect to the nationality of the immigrants, panel (b) in Fig. A.2 shows that the largest immigrants inflows by origin stemmed from Latin American and Eastern Europeans, which had moderate presence in 2001, followed by EU-15 and North Africans. EU-15 immigrants were the most important nationality group in the instrument base year 1991, by large. The change in the most important sending nationality groups could reduce the strength of the instrument, but it helps with concerns raised by Jaeger et al. (2018) and Goldsmith-Pinkham et al. (2020).

Until 2008, average local housing costs also increased considerably, in particular house sale prices. As the middle panels of the figure show, during the housing boom years average house prices increased around 108%, and even with the fall that followed, on average they increased almost 65% during the 2001–2012 period. For rents, the increase was of around 37% between 2001 and 2012, around 30% until 2008. The annual increase slowed down after 2008 but nevertheless remained slightly above 1%.[17]

Regarding the spatial distribution of the variables of interest, A.3 shows the distribution by quintile of the share of foreign-born population in 2001 and 2012. In this map, we observe that in 2001, in the top 2 quintiles of the FB share distribution (darkest colours) we find core economic areas (Madrid, Catalonia, Valencia, Basque Country), tourism-oriented locations like the Islands, Malaga, Murcia and Alicante (where wealthy European foreigners locate) but also poorer areas (Galicia, Extremadura), with large proportions of 50–60s out-migrant returnees. By 2012, within the areas with larger FB shares, we have several provinces in Castilla and Aragon. The change in the spatial pattern in the location of foreign-born shows that we have much variation to exploit in the empirical exercise, and this can adopt a very demanding empirical strategy.

To carry out the empirical analysis I used data from multiple sources. Immigrant and population data comes from the Municipal Population Registers (*Padrón Municipal*), which keeps an annual record of all registered individuals in a municipality over time regardless of their legal immigration status. This is the most reliable source to study the impact of the size of immigration on area economic outcomes. House price data was obtained from Uriel-Jiménez et al. (2009), who provide an improved version of the Housing Department Average Province House Price Index, which is based on property appraisal data. Data on rents was obtained combining data from the Housing Department and the National Institute of Statistics. Finally, data on the controls comes from several sources including the National Institute of Statistics, the Public Works (Housing) Department, the European Environmental Agency and the 2001 Census. Full details on the data sources are provided in Section B.1 of the Online Appendix. Summary statistics for the main variables in the analysis are provided below in Table 2. The full list of controls is provided in the descriptive statistics table (Table B.5) and in the results tables notes.

4. Main results

4.1. The total effect of immigration on house prices

In this section I present the results of the estimation of the total impact, e.g. β_1. In the main results I discuss the impact of immigration on house sale prices, as I believe they capture better the impact that immigration might have on housing demand. The growth of rent prices is limited by the data and institutional setting. Results related to rent prices are discussed separately in the robustness section. Table 3 presents results of the estimation of Eq. (10) by ordinary-least-squares (OLS). Each column presents a specification that includes different sets of controls and fixed-effects. In all specifications the standard errors are clustered at the province level and robust to heteroskedasticity, and I include year fixed effects to control for national shocks. Specifications range from more to less demanding in terms of data variation: OLS results (column 1) to first differences province fixed effects with attribute trends model (column 5). The list of control variables is specified in the notes of Table 3, and it is the same in all result tables unless specified. Coefficient β represents a semi-elasticity and it can be interpreted as the growth of housing costs in percentage points for a 1 percentage point (0.01 units) increase in the rate. As explained above, in this case β corresponds the total demand estimate and captures the combined impact of changes in demand from immigrants and natives.

The first column of Table 3 shows the results obtained when I only include year dummies. It reports a simple correlation of 0.6 for house prices. In columns 2 and 3 I add province attributes (time-invariant characteristics) and the province fixed effects (which are collinear with the attributes). The estimates increase substantially with respect to column 1. In column 4 I add the province attributes interacted with year dummies, which control flexibly by trends on the levels of the attributes correlated with immigration rates and housing costs. This is

[17] The range of variation of rent price growth is much smaller than that of sale prices, as can be seen in the standard deviations in Table 2. This is because average rent prices calculations are based on prices in properties currently rented (whose rents grows slowly or tied to national CPI indices) and on newly rented properties (with higher price increases when tenancy agreements change). Also, rent prices capitalise consumption of housing as a service, while sale prices growth also has an speculative component when housing is acquired as an investment asset.

Table 3
Total demand effect estimates: OLS and FE results.

Δ Log Sale Prices (t)	(1)	(2)	(3)	(4)	(5)
Immigration rate ($t-1$)	0.604**	1.069***	1.468***	1.947***	2.024***
	(0.291)	(0.331)	(0.374)	(0.596)	(0.584)
Adjusted R^2	0.85	0.85	0.86	0.86	0.86
Province attributes		Yes			
Province FE			Yes	Yes	Yes
Province attributes * Year FE				Yes	Yes
Time-var controls ($t-2$)					Yes

Notes: Significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Each column presents results from a different specification. All regressions include year dummies and use 550 observations (50 provinces over 11 periods). $t = 2002\backslash 2012$. Clustered (province) standard errors in parenthesis. *Time-varying controls* (lagged two periods e.g. $t-2\backslash t-3$) include change of log current GDP per capita, change of log of number of credit establishments, change of unemployment rate, change average years of education employed, change share working-age-population without any degree, change of log transport infrastructure and change of log urban infrastructure. *Province attributes* (time invariant) include share of residential secondary dwellings, share of residential empty dwellings, share of households which own a secondary home, share of employed in construction sector, share of employed in services sector, share of foreign-born renters (residents in family homes), share of natives renters (residents in family homes), log average sqm dwelling per person foreign-born, log average sqm dwelling per person natives; all these in 2001. They also include log road distance to Madrid, length of coastline in 100s of kms, log of rain precipitation (January), share of developable land (Corine 2000) and average ruggedness index. *Province attributes * Year FE* interact the time-invariant characteristics with year dummies.

particularly relevant for characteristics which are quite different across provinces, like house tenancy and consumption patterns by natives and foreigners. The coefficients increase again and remain very similar when I further add time-varying controls in column 5. The model in this last column, where I include time and province fixed effects, province attribute flexible trends and time-varying controls is the most demanding one, and the baseline specification in the rest of the paper. Here, the estimated semi-elasticities are around 2 for sale prices. Even if this is informative, these coefficients roughly correspond to partial correlations.

In order to be able to make causal claims about the estimates, I implement the IV strategy explained in Section 3.3. Table 4 presents the results using the instrument as defined in Eq. (16), using 1991 as base year and the gravity-predicted immigration inflows to construct the shift-share instrument. I depict the coefficients for the baseline specification in column 5 of Table 3. I estimate the models using 2 stages least squares (Correia, 2018). Column 1 shows the semi-elasticity for house sale prices, which correspond to the total demand semi-elasticities. The first-stage estimate is shown in column 3. I report the weak identification test (F-stat Kleibergen–Paap), which informs about the relevance of the instrument and the mean values of the outcomes and the rates. The table shows that the instrument is very strong. As expected, in all specifications the standard errors increase when using IV.

Compared to the OLS fixed effect estimates, the IV coefficients are much larger. This suggests that immigrants are moving, conditional on the controls and the area fixed effects, to provinces which are experiencing negative shocks in the growth of prices, and therefore the estimates of Table 3 are downward biased. Given that I am controlling for a wide set of time-varying economic factors and time-invariant attributes, it is quite reasonable that, conditional on all these controls, immigrants locate in places where housing is more affordable.[18] In addition, as discussed above, the downward bias of the OLS estimates could be due to measurement error, either due to poorly measured raw population register number or due to the fact that the total foreign-born number masks substantial nationality-mix heterogeneity across provinces and the IV better captures the average treatment effect. I find a semi-elasticity of around 3.3% for sale house prices, for an increase in the immigration rate of 1 percentage point, which is very similar to previous findings.[19]

4.2. The effect of immigration on native location

In this section I discuss the estimate of the impact of immigration on native mobility, in order to assess the difference between the total and partial demand effects formalised in 2. It involves the IV estimation of coefficient β_3 in Eq. (12). The sign and size of this coefficient inform about the existence of native displacement or co-location (attraction). The estimated coefficient, shown in column 2 of Table 4, indicates that for each 10 immigrants that settle in a province, around 3 natives relocate there due to the immigration inflow. The time period of analysis is 2001–2011. The first-stage is the same than for column 1. The effect is estimated contemporaneously in order to match Eq. (9), but in the following sections I also explore the timing of these adjustments. This non-negligible attraction estimate suggests that the difference between partial and total impacts would be sizeable.

This finding suggests that natives and immigrants are contemporaneously co-locating in the same provinces. The attraction or co-location estimate, although counter-intuitive, has also been found in other papers (for example Mocetti and Porello, 2010; Wozniak and Murray, 2012). While many authors have argued the existence of "natives' fly", the empirical findings on native displacement are inconclusive (Amior, 2021). Peri and Sparber (2011) argued that displacement could not be as quantitatively relevant as previously thought, at least in the case of the US. The expected direction of the relocation effect might also depend on the geographical size of the unit of analysis and the underlaying characteristics of the locations (Larkin et al., 2019).[20] One potential explanation for the attraction result is that immigrants might be complementary to natives, due to different tastes or skill levels, and thus positively affect their location decisions. Recently, the immigration impacts literature has focused in the research of these complementarities (Ottaviano, 2014). Besides enhancing productivity through improved task specialisation (Peri, 2012), immigrants might have desirable attributes for natives. For example immigrants could be specialising in producing goods and services which are desirable for natives (Ottaviano et al., 2013), increasing their consumption opportunities (Mazzolari and Neumark, 2012) or allowing female workers to increase their labour supply (Barone and Mocetti, 2011). In order to provide some intuitions on the co-location finding, in Section B.4 of the Online Appendix, I lay out a simple theoretical framework where natives and immigrants specialise in different sectors (high-skill natives in the tradable sector and low-skill immigrants in the non-tradable local services sector). In the model, an inflow of immigrants reduces the price

[18] González and Ortega (2013) and Farré et al. (2011) find the same direction for the OLS-IV bias.

[19] Saiz (2007) finds 3.2 for prices, Degen and Fischer (2017) find 2.7 for Swiss prices, González and Ortega (2009) find 3.2 for house prices and Ottaviano and Peri (2012) find up to 2 for prices.

[20] There is some previous evidence that also points towards native-immigrant co-location in Spain. Fernández-Huertas et al. (2009) find a comparable result to mine for a long-differences estimation from population growth regressed on the immigration rate for the period 2001–2008. Their prediction is of 1 native for each 10 immigrants. They argue that this number is sufficiently small not to have an impact on compensation or reinforcement of the impact of immigration inflows on the housing or the labour markets. The difference in the results could be due to the fact that these authors do not use IVs in their estimation and they use long differences between 2001 and 2008, so they only have 52 observations. In fact, when they perform the estimation at the municipality level, using over 8,000 observations, they find very similar estimates to mine. Fernández-Huertas et al. (2019), using finer spatial data, also find co-location of natives and immigrants in newly created suburban communities, and only find mild native displacement in neighbourhoods where housing supply is constrained.

Table 4
Total and partial demand effect estimates: instrumental variables 2SLS results.

	(1)	(2)	(3)	(4)	(5)
	Δ Log Sale Prices (t)	Native Rate ($t-1$)	Immigration Rate ($t-1$)	Δ Log Sale Prices (t)	Population Rate ($t-1$)
Immigration rate ($t-1$)	3.278** (1.236)	0.308*** (0.088)			
Population rate ($t-1$)				2.506*** (0.881)	
Immigration rate SSIV ($t-1$)			0.684*** (0.143)		0.894*** (0.180)
Test weak identification (KP)			22.94		24.78
Mean Value of Outcome (Y)	0.047	0.002	0.009	0.047	0.010
Mean Value of Rate (X)	0.009	0.009	0.007	0.010	0.007
All province FE and controls	Yes	Yes	Yes	Yes	Yes

Notes: Significance levels: $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. Prices are in log changes. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3. Clustered (province) standard errors in parenthesis. Obs = 550.

of local services making locations more attractive to natives, who co-locate with the immigrants. While data availability prevents a full test of the model, I provide some correlations that indicate this mechanism could be credible, especially in provinces that receive a large number of natives and immigrants.

4.3. The partial effect of immigration on house prices

In this section, in order to estimate the partial demand effect, I apply the methodology described in Section 3.2. I use population growth rate as the main regressor in Eq. (11) and instrument it with expression (16). This instrument predicts exogenous foreign-born location. Conditional on controls and fixed effects, the predicted-by-the-instrument population growth second-stage estimate only captures changes in population due to immigrant inflows. This coefficient captures the impact on house prices stemming from changes in foreign-born demand, abstracting from the induced demand due to other population changes. By doing this, the estimated coefficient corresponds to a direct immigrant demand elasticity (partial impact), independent from demand changes from relocated natives.

The results of this exercise are shown in columns 4 and 5 of Table 4. The instrument is very strong in all specifications, as shown in the weak identification test, and predicts almost 90% of population growth. The estimated semi-elasticity for house sale prices is 2.5%, for an increase in population growth (due to immigration) of one percentage point. If I combine the coefficients of columns 4 with the co-location estimate of column 2, the decomposition equation (9) holds exactly in the data. The partial demand semi-elasticities are almost 24% smaller than the total demand effect, due to the increase in demand induced by the native relocation process. The total demand impact of an increase of immigration in one percentage point is 3.3%, of which 2.5% is due to direct immigrant demand and 0.78% to additional demand for relocated natives. This insight is new in the literature, and highlights the importance of the framework laid out in Section 2.

5. IV strategy discussion

In this section I discuss the validity of the IV strategy implemented. Two conditions must apply for the shift-share prediction to be an appropriate instrument. For the exclusion restriction to be valid, conditional on all controls, the only channel through which the predicted immigrant stocks affect the housing costs growth must be via its effect on current immigrant stocks. This implies that historical settlement pattern of immigrants by nationality/country of origin in the base year (share component in Eq. (15)) has to be sufficiently lagged that, conditional on controls, it is orthogonal to unobservables correlated with current housing costs growth (exogeneity condition). At the same time, the instrument has to be sufficiently strong in its prediction of the current immigrant location patterns (relevance condition). I provide two pieces of evidence to test these conditions. The first one relates to the exogeneity of the instrument and the second one, to its relevance. Finally, I also correlate the instrument with changes in house sale prices before the period of analysis, particularly during 1994–1998 where there was mild housing boom. This exercise aims to test for the existence of pre-trends.

The base year for the construction of the instrument, 1991, is 10 years before my observation period starts, which is substantially longer than in other applications.[21] However, one could still think of unobservable shocks correlated with housing costs and location decision of immigrants that existed in 1991 that still affect both aggregates today (even conditional on all the trends and province attributes/fixed effects). To test this, similarly to Farré et al. (2011), I regress the share of foreign-born population in 1991 on 1990–91 economic factors and then the change in this share during my observation period (2001–2012) on the same variables. The aim of this exercise is to show that the determinants of the geographical distribution of the mass of immigrants in 1991 does not perfectly predict their location during my period of study. The results are shown in Table 5.

The explanatory variables include the log of disposable income, the log of average wage (region wage bill over workers), the share of different sectors (construction, services and industry) in the regional value-added (the excluded category is the agriculture sector), the unemployment rate for natives and foreign-born workers, the log housing density (number of residential housing units per square km) and the share of built-up land over total land (to control for urbanisation) and additional controls related to geography (area, coast dummy and length of coastline and distance to Madrid). In column 1, the model has high predictive power (R^2 is around 0.82) and most of the regressors are significant.[22] When I regress this same set of variables on the change of share of foreign-born population over the 2001–2012 period none of the coefficients is significantly different from zero and the explanatory power of the model is much lower. This test is supportive of the appropriateness of using 1991 as base year. In case I consider 1991 still too close to the start of the period, in the robustness checks provided in the next section, I also use 1981 as base years to construct the instrument (which remains strong) and the results remain fairly similar.

The second instrument validity exercise relates to the relevance of the instrument. I construct alternative instruments with placebo shifts or placebo shares combining them with the actual share or shifts

[21] For example Saiz (2007) uses data from 1985–1998 and the base year is 1983 and Sá (2015) uses data from 2003 and the base year is 2001.

[22] These estimates correspond to partial correlations, hence the coefficients of individual variables have to be interpreted conditional on everything else.

Table 5
IV validity checks: base-year validity regressions.

	(1) Share FB in 1991	(2) Δ Share FB 2001\2012
Log disposable income	−0.026**	−0.032
	(0.010)	(0.052)
Log of average wage	−0.013	−0.006
	(0.020)	(0.095)
Share of GVA construction	0.200**	−0.580
	(0.091)	(0.533)
Share of GVA services	0.113***	0.011
	(0.036)	(0.333)
Share of GVA manufacturing	0.085**	−0.027
	(0.036)	(0.288)
Natives' unemployment rate	−0.030	−0.165
	(0.028)	(0.179)
Immigrants' unemployment rate	−0.076**	−0.175
	(0.037)	(0.185)
Log no. of homes per sqkm	0.038***	0.042
	(0.012)	(0.066)
Share of built-up land over total	−0.283*	0.025
	(0.146)	(0.460)
Log province area	0.022**	0.043
	(0.010)	(0.054)
Province located on the coast	−0.009	0.015
	(0.006)	(0.032)
Length of coastline (in 100s of kms)	0.001***	0.001
	(0.000)	(0.004)
Log road distance to Madrid (in kms)	0.006	−0.006
	(0.004)	(0.011)
Constant	0.014	0.183
	(0.143)	(0.746)
Observations	50	50
Adjusted R2	0.82	0.43

Notes: Significance levels: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Data sources: Census 1991, Spanish Regional Accounts, Corine Landcover 1990. GVA stands for Gross Value Added. Economic values in 1991, share of build-up land in 1990. The omitted category is share agricultural GVA.

(those used in the construction of the instrument (16)). Specifically, I use shares and shifts which I expect to have little strength predicting current immigrant location patterns. I use these placebo instruments to check if the results hold. The purpose of this exercise is to prove that it is the exact combination of the (nationality-specific) 1991 location patterns and the gravity predicted national inflows that produces a reliable strong instrument. I show the results of this exercise in Table 6, for native mobility (panel A) and the partial demand estimates results (panel B). The first column shows the baseline estimates, which uses the instrument where the share is based on the 1991 provincial foreign-born stocks by nationality and the shift is based on the gravity estimates of Table B.1.

In columns 2 to 4, I change the province share and interact it with the gravity national prediction (baseline shift). In column 2, I distribute each immigrants nationality randomly across provinces and I multiply this allocation by the gravity-model predicted annual inflow by nationality. The results are significant for native mobility and not-significant for house prices. In column 3, I use nationality-province immigrant stock information from the 1940 census. This shows that such old past location patterns do not have any predictive power for the current ones. The instruments used in columns 2 and 3 are very weak, with KP tests below 4.

In the last three columns of the table, I use the 1991 share (baseline share) and interact it with a placebo annual shift by nationality. In columns 4 to 5, I use the immigrants by nationality (inflow and stock) going to other rich countries which are very far and have different production structures than Spain. I use inflows to the USA (column 4) and to Australia and New Zealand (column 5), and I find the instrument becomes very weak and coefficients are very small and insignificant. Finally, in column 6, I use the annual growth in province population predicted by the natural movement of population (births–deaths), and distribute it based on the total share of foreign-born population in 1991 in each province. The coefficients of interest are also highly insignificant and the instrument is weak. These results show that is the combination of a relevant share and a relevant shift that gives raise to a strong instrument that predicts the annual location of immigrations by nationality in each of the provinces.

In the last validity exercise I test if the instrument correlates with pre-trends in the outcome variable, house sale prices, during the years 1990 to 2000. In this decade there was an economic crisis (1990–1994) followed by high (1994–1998) and moderate (1998–2000) house price recovery. I use information from the Valuation Society (*Sociedad de Tasación*) during the period. In Table A.1, I correlate my instrument with growth rates in these sub-periods, either using the whole panel and year-by-year data (columns 1–3) or a 10 year long-difference version of the instrument and one cross section. The results of the table show there is no correlation between price dynamics in the 1990–2000 decade and our instrument, which strengthens the validity of my instrumental variables strategy.

6. Additional results

6.1. Effects on rent prices

In this section, I discuss the results related to the impact of immigration on rent prices. The yearly province rent prices measure is based on the stock of rented properties, which has smaller variation than transaction sale house prices (as shown in Table 2). Moreover, the institutional framework limited annual price growth for already rented properties to the national CPI index, which furthermore limits the variation of rents, and results in a worse indicator to capture changes in housing demand. Hence, compared to the main results, I consider these results of secondary importance.

Columns 1 to 3 in Table 7 show the OLS and IV results for rent prices, which use the same set of controls as for sale prices (the first stage is the same as in columns 3 and 5 of Table 4). The total effect is almost 1% and the partial effect 0.75%, and the decomposition of total demand also holds for these results. The coefficients for the total impact is in line with previous estimates (for example Saiz, 2007 and Ottaviano and Peri, 2007). The coefficients are 4 times smaller than those for sale prices, which is consistent with the limited scope of the rent price growth. In columns 4 to 7, I present additional robustness rent prices results which are discussed in Section 6.3.

6.2. Robustness checks

Here I discuss additional results in order to check the robustness of the findings. I focus on the native mobility and partial estimates coefficients, but the results are also robust for the total demand estimates. The robustness results for rents are displayed in Table 8. In the first column I show the baseline estimate for comparison purposes.

Column 2 uses foreigners instead of foreign-born population. A fraction of the Latin American immigrants that settled in Spain held Spanish passports and were able to settle as nationals, so the number of foreigners is smaller than the number of foreign-born immigrants. The coefficient using this measure is very similar for all three outcomes, with the instrument (constructed with 1991 foreigner settlement patterns) being stronger than in the baseline results. In columns 3 and 4 I change the base year for the computation of the instrument, with location patterns based in 1981 (older) and 2001 (more recent) ethnic networks. The instrument is weaker using 1981 information and stronger using when using that of 2001, as we would expect, and in both cases with a KP-test over 10. With 1981 shares, the result is very similar to the baseline, while with 2001 shares the coefficients are larger than in the baseline. However, it is unlikely that the exclusion restriction holds with 2001 settlement patterns. Given this, the preferred

Table 6
IV validity checks: shift-share placebos.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Change Share		Change Shift		
	c91&grav	random	c1940	USA	AUS&NZ	NAT_GR
PANEL A: Native population rate ($t-1$)						
Immigration rate ($t-1$)	0.308***	0.391**	0.151	−0.349	0.284	0.123
	(0.088)	(0.188)	(0.274)	(0.952)	(0.259)	(0.252)
Weak identification test (KP)	22.94	3.24	3.80	0.76	3.14	4.87
PANEL B: Δ Log Sale Prices (t)						
Population rate ($t-1$)	2.506***	3.128	−2.358	−17.823	6.615	5.368
	(0.881)	(1.974)	(4.692)	(49.532)	(5.419)	(4.917)
Weak identification test (KP)	24.78	3.23	2.30	0.18	2.34	2.80
All province FE and controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$. Prices are in log changes. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3. Clustered (province) standard errors in parenthesis. Obs = 550.

Table 7
Additional results: effect on rent prices.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Log Rent Prices (t)	OLS	Total	Partial	LongDiff	LongDiff	Lags	Lags
Immigration rate ($t-1$)	0.675***	0.986**					
	(0.203)	(0.465)					
Population rate ($t-1$)			0.754**				0.482
			(0.353)				(0.405)
Population rate (t)						0.989**	0.823*
						(0.443)	(0.443)
Immigration rate (LD5)				1.027**			
				(0.405)			
Population rate (LD5)					0.718***		
					(0.261)		
Weak identification test (KP)		22.94	24.78	20.54	14.07	13.26	8.19
Observations	550	550	550	100	100	550	550
Area FE and controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$. Prices are in log changes. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3. Clustered (province) standard errors in parenthesis. Obs = 550.

Table 8
Additional results: robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Foreigners	c1981	c2001	49COB	Gateways
PANEL A: Native population rate ($t-1$)						
Immigration rate ($t-1$)	0.308***	0.299***	0.429**	0.396***	0.291***	0.368***
	(0.088)	(0.084)	(0.178)	(0.110)	(0.089)	(0.100)
Weak identification test (KP)	22.94	28.25	8.95	42.01	19.49	29.03
PANEL B: Δ Log Sale Prices (t)						
Population rate ($t-1$)	2.506***	2.519***	2.278**	3.191***	2.540**	3.409***
	(0.881)	(0.925)	(1.054)	(0.805)	(0.957)	(1.041)
Weak identification test (KP)	24.78	30.92	11.38	51.57	19.98	42.01
All province FE and controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Significance levels: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$. Prices are in log changes. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3. Clustered (province) standard errors in parenthesis. Obs = 550.

base year remains 1991. In column 5, I use a different grouping of countries to construct my instrument, 49 countries instead of the 104 nationalities depicted in Table B.6. The coefficients are very similar to column 1 and the instrument remains very strong.

In column 6, I use an alternative instrument based on the gateways/ports of entry. The intuition is that different nationality immigrants will locate disproportionably in regions which are more accessible to them.[23] Using the model with province fixed effects, province attribute flexible trends and all the controls the instrument is very

[23] This IV is inspired in the measured proposed by González and Ortega (2013). I first locate 50 ports of entry using 6 different travel modes (listed in Table B.7 in the Online Appendix). For 113 nationalities, I calculate the share of immigrants in 2000 that used those different modes of transportation using data from the National Immigrant Survey 2007. This produces nationality-specific variation, which is needed in order to avoid perfect collinearity with the province attributes and fixed effects included in the estimations. Then, for each province in Spain I calculate a weighted-by-road-distance and port size (using data on air and boat passengers in 2001) nationality-specific accessibility index. I calculate a weighted measure of how accessible a province is for each nationality from all the ports of entry, where the numerator is the port-size and the denominator is share of migrants that use that particular mode. I normalise this province accessibility measure and use it to distribute

Table 9
Additional results: timing of the adjustments — long-time differences.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ Log Sale Prices (LD3)	Native Rate (LD3)	Δ Log Sale Prices (LD3)	Δ Log Sale Prices (LD5)	Native Rate (LD5)	Δ Log Sale Prices (LD5)
Immigration rate (LD)	2.143*	0.208		2.132**	0.430**	
	(1.157)	(0.208)		(0.986)	(0.172)	
Population rate (LD)			1.774*			1.491**
			(0.884)			(0.656)
Weak identification test (KP)	16.38	16.38	11.17	20.54	20.54	14.07
Observations	150	150	150	100	100	100

Notes: Significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. The sample period is 2011/2001. Both the outcomes and the population/immigration rates are calculated using 3 year (2002/2005, 2005/2008 and 2008/2011, Obs = 150) or 5 year (2001/2006 and 2006/2011, Obs = 100) time differences. Clustered (province) standard errors in parenthesis. Prices are in log changes. The estimations include region and time dummies, time-varying LD controls and province attributes, as described in Table 3.

Table 10
Additional results: timing of the adjustments — lags.

	(1)	(2)	(3)	(4)	(5)
PANEL A: Native population rate (t)					
Immigration rate ($t-2$)			−0.120		−0.180
			(0.114)		(0.116)
Immigration rate ($t-1$)		0.074		−0.048	
		(0.102)		(0.116)	
Immigration rate (t)	0.308***			0.340***	0.307*
	(0.088)			(0.127)	(0.156)
Observations	550	500	450	500	450
Weak identification test (KP)	22.94	20.25	16.61	6.79	4.85
PANEL B: Δ Log Sale Prices (t)					
Population rate ($t-2$)			3.116**		2.890*
			(1.498)		(1.602)
Population rate ($t-1$)		2.506***		2.336***	0.694
		(0.881)		(0.827)	(0.998)
Population rate (t)	2.055			0.512	
	(1.902)			(2.232)	
Observations	550	550	500	550	500
Weak identification test (KP)	13.26	24.78	24.09	8.19	9.54

Notes: Significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Prices are in log changes. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3, adjusted accordingly depending on the lag used. The sample period is 2002/2012 for house prices and 2001/2011 for native mobility. Clustered (province) standard errors in parenthesis.

strong and the results are very similar to the baseline. It is very reassuring to find very similar results using a completely different location share based on accessibility.

A second robustness check is to test if immigration and population rates time leads affect past outcomes. The results for this are provided in Table A.2. Column 1 shows the baseline estimates for native displacement (contemporaneous) and population rate (lagged one period). In columns 1 and 2, I use one lead with respect to the outcome variable. i.e. ($t+1$). I find no impact on mobility and weak impacts for prices. When I use two leads ($t+2$) I find no effect in either outcome. Columns 4 and 5 use the leads in conjunction with the baseline lag for the outcomes and I find that the chosen time structure seem to be the one which remains significant even after controlling for leads. All in all, these falsifications checks provide additional robustness to the findings.[24]

6.3. The timing of the adjustments

While the empirical strategy has so far focused on providing total and partial demand estimates, in this section, I explore the timing of the adjustments of the results. First I explore if using longer lags of data, instead of one-year differences as in the main results, changes my main findings. The results for house sale prices and native mobility are shown in Table 9. I present the estimates using either three (columns 1–3) or five-year (columns 4–6) long-differences (LD) to construct the immigration and population rates and the instrument. The outcome variables also correspond to three and five-year growth rates. The panels use 150 and 100 observations respectively. For rents, I present only the results using five-year long-differences. For rent prices, the results are shown in Table 7.

There are two minor changes in the estimated specification. First, because the LD setting allows for the adjustments in prices from the inflows to take place at any time during the 3 or 5 year time period, I do not impose any lag structure in the specification. Therefore, in these results the long-difference changes in the outcomes and main regressors are contemporaneous. Secondly, given that I have fewer observations, I use a slightly less demanding specification than in Table 4, and use 17 regions (NUTS2) fixed effects. However, I still include a large set of controls which are listed in the notes of the table.

The table reports the coefficients for the total (columns 1 and 4), native mobility (columns 2 and 5) and partial (columns 3 and 6) demand effects. The main results generally hold when allowing for longer periods for the adjustments to take place. The instrument is still strong and the weak identification test statistics are well above the 10 rule-of-thumb threshold. Compared to the yearly panel variation, the coefficients are slightly smaller for sale prices. The coefficient for native mobility is not significant with three-year differences but larger

the nationality-specific gravity-model inflow/stock in every year from 2001 to 2011.

[24] Given that we are estimating a growth panel data model and exploiting within province variation, the yearly values of the variables are likely to be correlated, so this exercise is not foolproof.

with five. For rent prices, when using five-year differences, I find a very similar coefficient. If I use the coefficients of Table 9, the decomposition expression (9) is still valid. Overall, the results are similar to those in Table 4, and confirm my findings even when I allow for adjustments in prices over longer periods of time.

A second exercise to study the timing of the adjustments is to change the lag structure of the immigration and population rates. The results are again presented in Table 10 (for house sale prices and native mobility) and in Table 7 (for rent prices, in columns 6 and 7). Here, I test the robustness of the results to using a contemporaneous, one lagged or two-lagged inflow of immigrants, using them one at a time or combined. The baseline estimates are shown in bold for comparison purposes.[25]

The table shows that for native mobility the lag that seems to matter most is the contemporaneous one: immigration rate lagged one or two periods with respect to the native rate is insignificant, and when I add both at the same time (contemporaneous and lagged), it is the contemporaneous one that matters. For sale house prices, there are only the lagged rates that affect price growth, which would be explained by the fact that it takes more time for immigrants to be able to purchase homes. For rent prices, however, both the contemporaneous (column 6) and lagged (column 3) rates also affect rents growth, as we would expect, as immigrants consume housing from the moment they settle. Overall, the results are consistent and in line with the main findings.

7. Conclusions

This paper draws attention to a highly overlooked issue in the estimation of average area effects of immigration: the role of local population relocation on the adjustment of local demand and prices. The total impact of increases of foreign-born population on housing markets results from a combination of their immediate effect on housing demand, and their impact on native mobility and housing supply. The estimation of well-identified reduced-form total effects is with no doubt of interest for policy makers.

However, as well as the total impact, we might be interested on understanding the mechanisms driving its adjustment. Previous research has shown the importance of taking into account the role of displacement when estimating the impact of local policy interventions, particularly place-based initiatives (Briant et al., 2015; Mayer et al., 2017; Einiö and Overman, 2020). Null impacts of a local policy might be the results of positive and negative impacts cancelling each other out over space. Hence, it is important to account for the impact that mobility has on the estimation of aggregate local estimates. As I show in this paper, this issue is also important when assessing the impact of immigration on house prices, as immigration waves alter the spatial distribution of native population. Mine is the first paper that provides joint estimates of the total price impact of immigration and of all its components and, in doing so, it sheds light in the adjustment channels of local house prices to population shocks.

A better understanding of the impact of immigrant demand on housing costs is of great interest to urban economics. Incorporating better knowledge into the adjustment of housing markets is crucial when analysing the impact of immigration on urban labour markets and how demographic changes affect house prices (Gong and Yao, 2022). For example, Howard (2020) shows that immigration amplifies labour market shocks via its impact on housing demand. In addition, large immigration inflows impact local population generating shocks that dissipate across locations, with general equilibrium implications. Monras (2020) provides seminal work embedding the impact of immigration on housing markets into a quantitative spatial equilibrium framework. He provides new insights on the role of low-skilled immigrants, who largely work in the construction sector and, in the long run, reduce construction costs and house prices. Alternatively, my paper highlights the importance of accounting for native population relocation even in the short-run, and leaves the door open for further work incorporating this channel of adjustments into formal models. Finally, housing capital was a very important driver of wealth accumulation in Spain during the 2000s (Blanco et al., 2021). Correctly understanding the role of the large immigration inflows on housing stock capitalisation is also crucial when aiming to study wealth accumulation over time and locations, in order to better disentangle the price changes related to housing consumption demand from those related to real estate investment or changes in supply.

Declaration of competing interest

The author declares that she has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

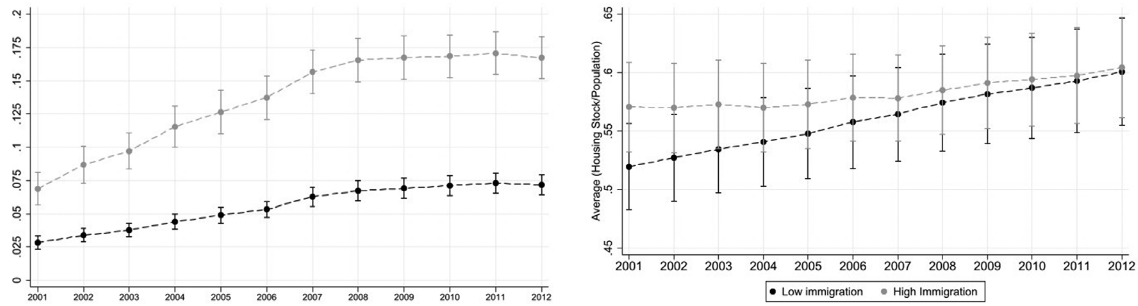
Data availability

Data will be made available on request

[25] It is worth mentioning that in panel estimations such as those of Table 10 it is difficult to exactly pin-down the timing of the effects, as I am exploiting within-province variation (conditional on many controls). This exercise then is similar to a horse-race regression, where I investigate which of the lags has a stronger explanatory power. It should not be read as an infallible test of the time-structure to be used in the specification.

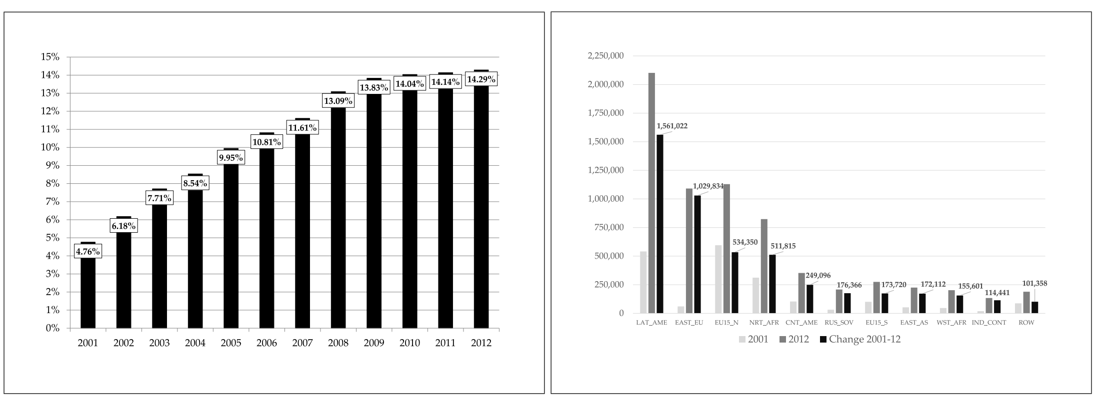
Appendix A.1. Appendix tables and figures

See Figs. A.1–A.3, Tables A.1 and A.2

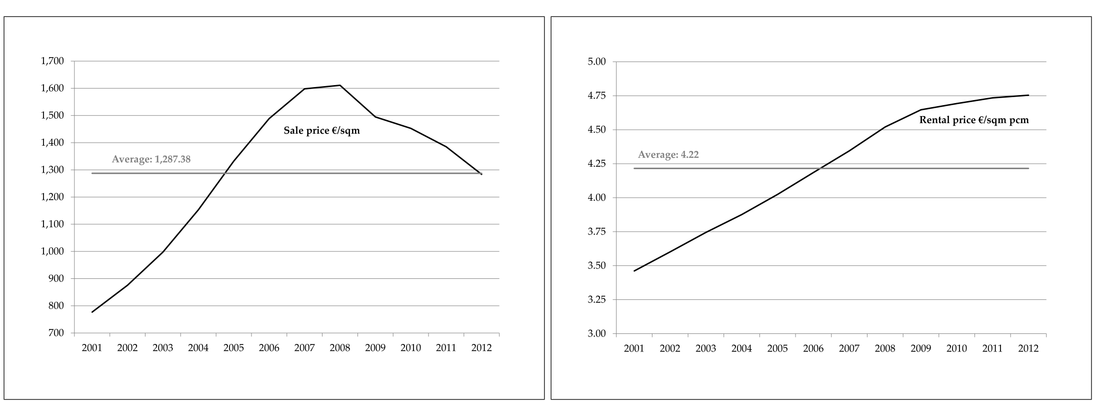


(a) Average FB/population (b) Average hous stock/population

Fig. A.1. Share of foreign-born population and housing stock per person — 2001–2012. *Source:* Spanish Department of Housing and Annual Population Registers. High/low immigration status is defined by being above or below the median in the foreign-born inflow over population during the 2001–2012 period (0.0783). Points depict province averages in the variable, while bars show the 95% confidence intervals.

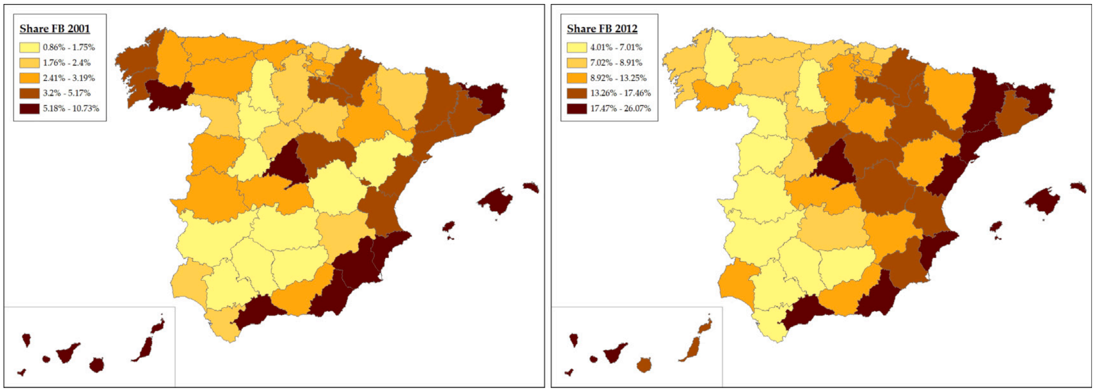


(a) Share of foreign-born population (b) Foreign-born stocks and inflows by nationality



(c) House sale price growth (d) House rent price growth

Fig. A.2. Foreign-born shares and house price growth over time. *Source:* Spanish Department of Housing, IVIE and Annual Population Registers.



(a) Share of FB in 2001 (b) Share of FB in 2012

Fig. A.3. Spatial distribution of shares of foreign-born 2001 and 2011.
Source: Annual Population Registers.

Table A.1
IV validity checks: correlations between immigration rate SSIV and pre-period house prices.

	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log Sale Prices between	1990\94	1994\98	1998\00	1990\94	1994\98	1998\00
Immigration rate IV ($t-1$)	−5.848	−0.354	2.153			
	(3.582)	(1.712)	(1.358)			
Immigration rate IV (LD10)				0.070	−0.103	0.070
				(0.355)	(0.648)	(0.355)
Observations	550	550	550	50	50	50
Model	Panel	Panel	Panel	LD10	LD10	LD10
Sample	2001–11	2001–11	2001–11	2011	2011	2011
R^2	0.10	0.04	0.07	0.62	0.45	0.62

Notes: Significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Each column shows the coefficients of running a different model. The main regressor is the immigration instrument, either year predictions (1–33) or long-difference (4–6). The outcomes of the models are the change in log (growth rates) of province average house prices for periods 1990–94, 1994–98 and 1998–2000, using data from the Valuation Society (*Sociedad de Tasación*). Columns 1 to 3 use the 2001–2011 panel and control for province attributes, (province attributes * year) and time-varying controls as in Table 3, but not for province FE (as the outcomes are time invariant). Clustered (province) standard errors in parenthesis. Columns 4–6 regress the 10 year long-difference of the instrument on the growth of house prices. These models include province attributes and 10 year LD time-varying controls. Robust standard errors in parenthesis.

Table A.2
Additional results: timing of effects — leads.

	(1)	(2)	(3)	(4)	(5)
PANEL A: Native population rate (t)					
Immigration rate (t)	0.308***			0.386***	0.360***
	(0.088)			(0.125)	(0.091)
Immigration rate ($t+1$)		−0.032		−0.301	
		(0.171)		(0.202)	
Immigration rate ($t+2$)			0.196		−0.087
			(0.194)		(0.180)
Observations	550	550	500	550	500
Weak identification test (KP)	24.94	18.84	11.90	6.94	4.20
PANEL B: *Δ* Log Sale Prices (t)					
Population rate ($t-1$)	2.506***			2.189***	1.741
	(0.881)			(0.766)	(2.139)
Population rate ($t+1$)		4.297*		2.217	
		(2.392)		(2.000)	
Population rate ($t+2$)			2.913		0.677
			(1.898)		(4.650)
Observations	550	500	450	500	450
Weak identification test (KP)	24.78	5.21	5.33	4.36	0.95

Notes: Significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. All specifications include province FE, province attributes*YearFE and time-varying controls, as described in Table 3. The sample period is 2002/2012 for house prices and 2001/2011 for native mobility.

Appendix B. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2023.103893.

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