Annual Overseas Migration and Housing Markets: Evidence from Australian Neighborhoods

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Abstract

This paper studies the impact of net overseas migration (NOM) on residential property markets using annual, neighborhood-level data for Australia. We apply an instrumental variable strategy based on historical immigrant settlement patterns to isolate exogenous variation in migration flows. A one percentage point increase in NOM as a share of the local population raises house rents by 2.8% and unit rents by 4.3%, and increases unit prices by 3.2%. The baseline effect on house prices is not statistically significant, though cumulative estimates over one- and two-year horizons become negative and significant. The effects of immigration on prices and rents are lower in older neighborhoods, consistent with an amenity interpretation where attitudes toward immigrants are less favorable among older residents, counteracting positive demand pressures. Finally, our estimated impacts on the out-migration of incumbent residents are limited and, if anything, smaller than those reported in the previous literature.

JEL Codes: R31, F22, R23

Keywords: Migration, Real Estate Prices, Rents, Housing, Shift-Share Instrument

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1 Introduction

Immigration is widely recognized as beneficial to receiving economies. Immigrants contribute to labor supply and long-term fiscal sustainability (Hernandez, 2024), foster innovation and local economic growth (Terry et al., 2026), and the movement of people across borders promotes the cross-pollination of ideas that underlies long-run prosperity (Mokyr, 2016). Yet immigration also generates distributional costs, or "winners and losers". Chief among these is pressure on housing affordability. Across the developed world, and particularly in the Anglosphere, house prices and rents have recently surged, and immigration has emerged as a leading explanation in both public debate and policy circles. The Bank of Canada, for instance, stated that record immigration levels were driving up housing costs (Bank of Canada, 2024); in Australia, a previous opposition leader argued that high immigration levels had created the housing crisis (SBS News, 2024).[1] Providing policymakers with credible, causal evidence on this trade-off is therefore urgent, and is the goal of this paper.

The recent Australian experience offers a particularly compelling setting to study this question. First, Australia has among the highest immigration intensities in the developed world: in 2019, it had the highest share of foreign-born residents in the OECD after Luxembourg, at 30 percent of the population.[2] That is more than twice the OECD average and well above Canada (21%), the UK (14%), or the US (14%) (International Monetary Fund, 2024). In addition, unlike many existing studies, which mainly focus on European settings where immigration flows are largely determined by EU free movement (Sá, 2015; Sanchis-Guarner, 2023; Accetturo et al., 2014; Unal et al., 2024), Australia has a highly managed immigration system. Quantifying the housing costs of immigration is therefore directly relevant to policy. Second, Australia's housing market is widely considered to be supply-constrained, with persistent planning restrictions (Kendall and Tulip, 2018), geographic concentration in a handful of coastal cities, and sluggish construction productivity (Productivity Commission, 2025). Under these conditions, demand shocks are likely to translate into price and rent effects rather than being absorbed by new supply. Third, analyzing housing markets at the neighborhood level, rather than at the city or region level, is better suited to detecting mechanisms that aggregate studies may obscure, such as out-migration of incumbent residents, amenity effects concentrated among specific populations, and heterogeneity in supply conditions. Our sample covers approximately 2,300 Statistical Areas Level 2 (SA2), with a median population of roughly 10,000 residents, providing an unusually fine-grained view of local housing market dynamics.

This paper studies the impact of immigration on residential real estate prices and rents in Australia. In our setup, the main threat to identification arises from the fact that immigrants likely choose where to settle based on local housing market conditions. If, for example, they disproportionately move to neighborhoods experiencing declining prices or rents, OLS estimates

[1] In the United States, President Donald Trump declared that "immigration is driving housing costs through the roof" and subsequently credited mass deportations with reducing them (Trump, 2024).

[2] Figure A.1 in Appendix A documents the recent evolution of immigration flows at the national and state level.

of the effect of immigration on housing costs will tend to be downward biased. To address this concern, we adopt the methodology pioneered by Altonji and Card (1991), commonly referred to as the *Immigrant Enclave* shift-share instrumental variable approach, which exploits the tendency of immigrants to settle in areas with a higher pre-existing concentration of co-nationals. Following Borusyak et al. (2022), identification relies on shift exogeneity: aggregate immigration inflows by country of origin are assumed to be orthogonal to unobserved neighborhood-level determinants of housing outcomes, an assumption we support with a range of empirical tests.

We find that immigration has a strong impact on rents, both for houses and units (apartments), as well as on unit prices. A one percentage point increase in net overseas migration (NOM) as a share of the SA2 population raises house rents by 2.8% and unit rents by 4.3%. This is consistent with the fact that the vast majority of immigrants rent upon arrival, as we document in Section 2. Unit prices also rise significantly, by 3.2%.

In our baseline specification, the impact on house prices is not statistically significant. However, the cumulative estimates over two and three year horizons turn negative and significant. The negative effect is consistent with an amenity interpretation: even a zero contemporaneous estimate may conflate a positive demand effect and a negative amenity effect. Consistent with this, we find that the effect is concentrated in older neighborhoods, in line with evidence that attitudes toward immigration tend to be less favorable among older Australians (Lowy Institute, 2019, 2024; Roy Morgan, 2025).[3]

Our results are robust to a range of sensitivity checks, including altering the base year for the instrument, excluding the largest Rotemberg-weight countries from the instrument construction (and, notably, India and China), the exclusion of Covid years, and considering flexible time trends to deal with dynamic effects above and beyond those addressed by unit fixed effects (Jaeger et al., 2019).

Cumulative effects over time remain broadly stable across all horizons, similarly to Saiz (2007). We also examine how the impact of immigration varies across several neighborhood characteristics. The effects on rents are positive and significant only within Australia's three largest cities and in urban areas, consistent with more severe supply constraints in these markets. The negative effect on house prices seems concentrated outside the largest cities, also consistent with a more elastic housing supply reducing demand pressures. Across the within-SA2 price distribution, effects on prices increase monotonically with the percentile, consistent with immigrants competing for higher-quality dwellings. Finally, we provide suggestive evidence that immigration is associated with a net outflow of incumbent residents, though our IV estimates are imprecise under exposure-robust inference and generally smaller than those obtained in the previous literature.

Our paper connects to the literature that studies the impact of immigration on residential real estate prices and rents. Early influential work in this area is that of Saiz (2007), who employed

[3]In the Appendix, we formalize this amenity interpretation in a spatial equilibrium model. Differential amenity sensitivity between owners and renters, amplified in the house market, jointly generates positive rent effects, attenuated or negative house price effects, and uniformly positive effects for units.

a Bartik-like instrument similar to the one used in this work. Examples of recent work, which we discuss in detail below, are Moallemi and Melser (2020), Sanchis-Guarner (2023), Helfer et al. (2023), Unal et al. (2024), among others. We contribute to this literature in three ways. First, to our knowledge, ours is the first study to combine annual frequency, neighborhood-level analysis, and both residential prices and rents. A crucial advantage of annual data, relative to the census-based studies that dominate the literature, is that they capture micro-level dynamics that longer differences obscure, including the possibility that immigrants resettle between census waves in response to evolving relative housing costs (Saiz, 2007). At the same time, the fine-grained spatial unit lets us consider mechanisms that operate locally and would be averaged out at more aggregated levels, such as amenity effects and the out-migration of incumbents. The simple spatial equilibrium model we develop in Appendix F clarifies these temporal and spatial considerations, and also rationalizes the central patterns in our estimates.

Second, we study these dynamics in a novel and recent setting: Australia offers a distinctive combination of exceptionally high immigration intensity, a highly managed system, and inflows dominated by temporary visas that frequently provide a path to permanent residency. These features are largely absent from the existing literature, which focuses on the European and US settings (Saiz, 2007; Sanchis-Guarner, 2023; Accetturo et al., 2014; Helfer et al., 2023; Unal et al., 2024). Finally, we conduct inference using exposure-robust standard errors (Adão et al., 2019; Borusyak et al., 2022), which account for correlation in residuals across neighborhoods exposed to the same origin-country shocks. This refinement can be consequential: for instance, the effect on house prices is statistically significant under conventional clustering, but indistinguishable from zero under exposure-robust inference.

Concerning results, many studies point to positive effects on prices and rents at the city (urban conglomerate, administrative unit) level. Using US data Saiz (2007) finds that an immigration inflow equal to 1% of a city's population raises average rents by about 1%, with a similar effect over one- and ten-year periods; housing value estimates are larger and more sensitive to the time horizon, ranging from around 1% using intercensal data to 3% using annual data. Helfer et al. (2023) find strong effects of immigration on rents and prices in Switzerland; Unal et al. (2024) find positive effects on unit prices and rents; at an even more aggregated level (Spanish provinces), Sanchis-Guarner (2023) finds that effect to be 3.3% on house prices and 1% in rents; and Akbari and Aydede (2012) find rather small, yet positive, effects in Canadian house prices. [4]

In contrast, Unal et al. (2024) find no effect of immigration on house prices in Germany, while Sá (2015), using data from the UK, reports a negative effect of around 1.7%. The latter study argues that immigration displaces high-earning incumbents, leading to income-driven declines in housing demand and, consequently, prices. We also examine the impact of immigration on incumbent out-migration, and find some support for this mechanism, although estimates are insignificant. Reflecting this interpretation, Sá (2015) finds no significant impacts at more aggregated geographic

[4] Balkan et al. (2018) find that rents increased in response to Syrian refugee inflows in Turkey. Their treatment is a binary regional indicator rather than a continuous measure of immigrant intensity, precluding a direct comparison of magnitudes.

levels, where such displacement effects may be less visible. Accetturo et al. (2014), using Italian data, and Stillman and Maré (2008), using data from New Zealand, also find a negative impact of immigration on house prices. The latter study also finds a negative impact on house rents, which, to the best of our knowledge, is a unique finding in the literature. Also in the UK, Braakmann (2019) complements the study by Sá (2015) showing that immigration leads to a decline in average dwelling space per resident, which could contribute to the decline in prices.

Another closely related paper is Saiz and Wachter (2011), who use a geographical diffusion instrument to study immigration and house prices in US census tracts. They find that immigration reduces prices, a result they attribute to preferences for residential segregation. Apart from methodological differences, our paper departs from theirs by using annual transaction-level data and by disaggregating across dwelling types, allowing us to capture short-run dynamics that long-differenced specifications may obscure. While we find similar qualitative effects on house prices, consistent with the mechanism they emphasize, the effect on unit prices and rents is positive, underscoring the importance of demand forces.

In the context of Australia, using postal-code level census data, Moallemi and Melser (2020) find that an immigrant inflow corresponding to 1% of the population increases house and apartment prices by 1.1% and 0.7%, respectively. Our study differs from theirs in several dimensions. First, we use yearly data ranging from the 2016-17 financial year to 2022-23, while they employ intercensal data from 2006, 2011, and 2016. The yearly setup allows us to employ neighborhood-level fixed effects, controlling for time-invariant unobserved characteristics and dynamic adjustments. Second, we also investigate the impact on rents, finding that it differs substantially from that on prices. Third, our estimates point to a qualitatively different effect on house prices: while Moallemi and Melser (2020) find positive effects, our point estimates are negative, imprecisely estimated contemporaneously but statistically significant at longer horizons.

Finally, we provide causal estimates of the effect of immigration on the outflows of incumbent residents. A positive association between immigration and native out-migration has been documented using US micro-data, though without instrumenting for immigration flows (Hall and Crowder, 2014; Stonawski et al., 2022). Fernández-Huertas Moraga et al. (2019), who instrument using a similar approach to Saiz and Wachter (2011), find that roughly one native leaves for every three immigrants arriving in Spanish neighborhoods. Our point estimates are considerably smaller than theirs though their results fall within our confidence intervals. Andersson et al. (2021), using annual Swedish register data with neighborhood fixed effects, find no significant effect on incumbent out-migration in the full population, though significant outflows among homeowners.[5] Studies finding no displacement, such as Card (2001) and Peri and Zaiour (2022), operate at the city level, where within-city residential reallocation is averaged out. Finally, as dis-

[5] Andersson et al. (2021) estimate outflow and inflow equations separately. In the full population (their Table 5, column 1), both coefficients are small and insignificant, implying essentially zero net impact on net flows. Among homeowners (column 2), the outflow coefficient is 0.346 and significant, but the inflow coefficient is 0.170 and insignificant. The implied net displacement of approximately 0.176 is close to our point estimate, though we cannot perform formal inference on this difference as the covariance between their outflow and inflow estimates is not reported. Our estimates are not conditional on tenure and are therefore most comparable to their full-population specification.

cussed above, Sá (2015) finds nearly one-for-one out-migration of natives at the local authority level in the UK, an estimate substantially larger than ours and than those among the aforementioned papers.

The paper proceeds as follows: Section 2 provides an overview of the immigration system and recent trends in Australia. Section 3 discusses our empirical methodology, while Section 4 discusses our data. Section 5 presents evidence in favor of our instrument validity. Section 6 presents the main results, while Section 7 presents heterogeneous effects by neighborhood characteristics. Section 8 concludes.

2 Overview - Immigration in Australia

This section provides an overview of the recent immigration trends in Australia, focusing on the characteristics of newly arrived migrants.

2.1 Overview of the Australian Migration System

Australia's immigration system is structured around two broad categories: permanent and temporary migration. The permanent migration program comprises three streams. The Skill stream targets professionals whose qualifications and experience meet assessed labor market needs, and includes both points-tested independent visas and state- or employer-nominated pathways. The Family stream admits partners, children, and parents of Australian citizens and permanent residents. The Humanitarian stream provides protection to refugees and others in humanitarian need. Each year, the Australian Government sets a Migration Program Planning Level, consisting of an annual cap on permanent visa grants, which is allocated across these three streams. Over our sample period, the planning level was set at approximately 190,000 places per year, with roughly two-thirds allocated to the Skill stream.

Alongside the permanent program, Australia admits a large and growing number of temporary migrants. The principal temporary visa categories include Student visas, which allow full-time study for the duration of the enrolled course; Temporary Skill Shortage visas, which allow employers to sponsor skilled workers for up to four years; and Working Holiday visas, which allow younger nationals of eligible countries to work and travel for one to three years. Temporary migration flows are substantially larger than permanent migration in volume, and account for the majority of net overseas migration over our sample period, as documented in Figure 2 below.

A defining feature of Australia's system is the existence of explicit pathways from temporary to permanent residency. Temporary graduate visa holders may remain in Australia for two to four years after completing their studies, during which time many transition to skilled or employer-sponsored permanent visas. Temporary Skill Shortage visa holders sponsored by an employer for at least three years may apply directly for permanent employer nomination. More broadly, temporary residence in Australia frequently serves as a stepping stone to permanent settlement: a substantial share of permanent visa grants each year are made to individuals already residing in Australia on a temporary basis, meaning that permanent migration does not in practice correspond to an equal volume of new arrivals.

2.2 Source Countries

Figure 1 summarizes the composition of net overseas migration across source countries. We compute annual NOM inflows for each origin country and average these flows over the financial years 2016-17 to 2023-24. Migration inflows are relatively dispersed across source countries, as reflected in a relatively low Herfindahl-Hirschman Index (HHI) of 0.08 obtained from the computed flows.[6]

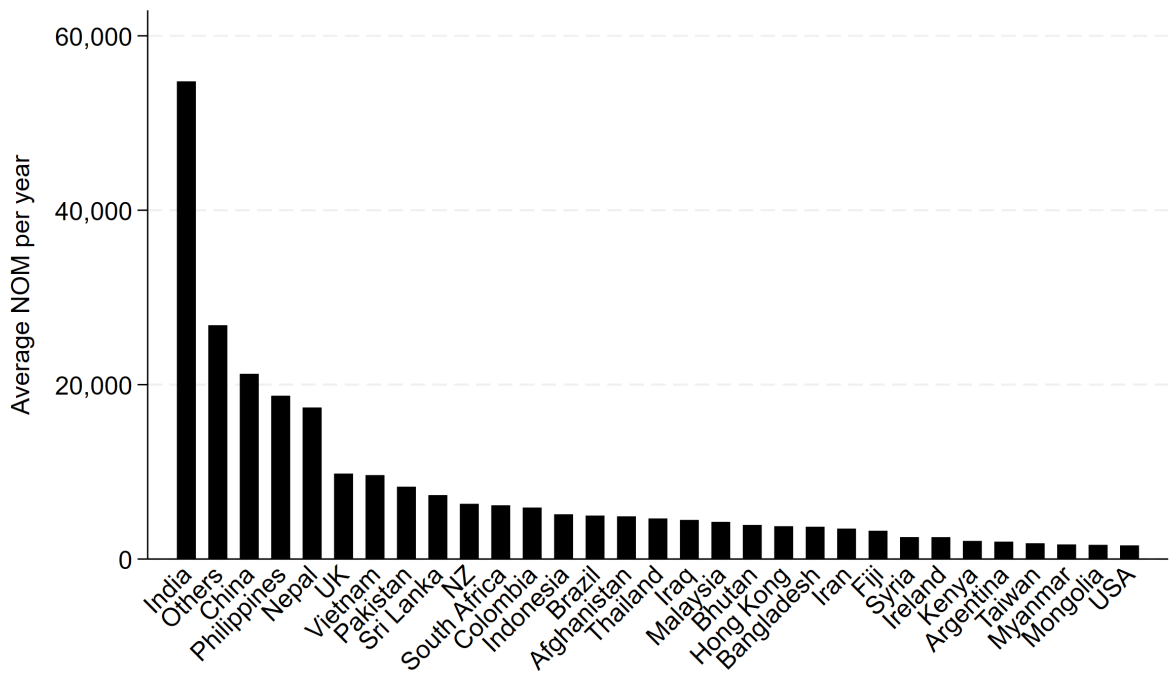


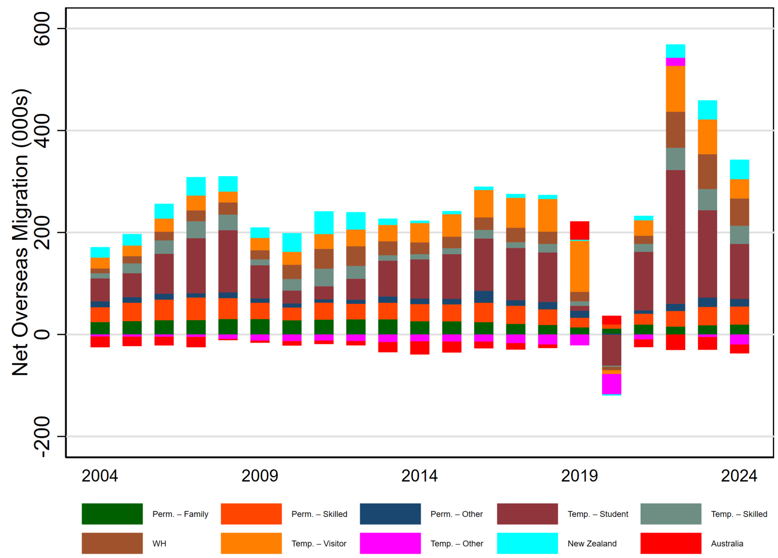
Figure 1: Average net overseas migration by source country

Notes: The figure reports average annual net overseas migration (NOM) to Australia by source country over the period 2016-17 to 2023-24. The category *Others* pools all remaining origin countries outside the top 30 contributors.

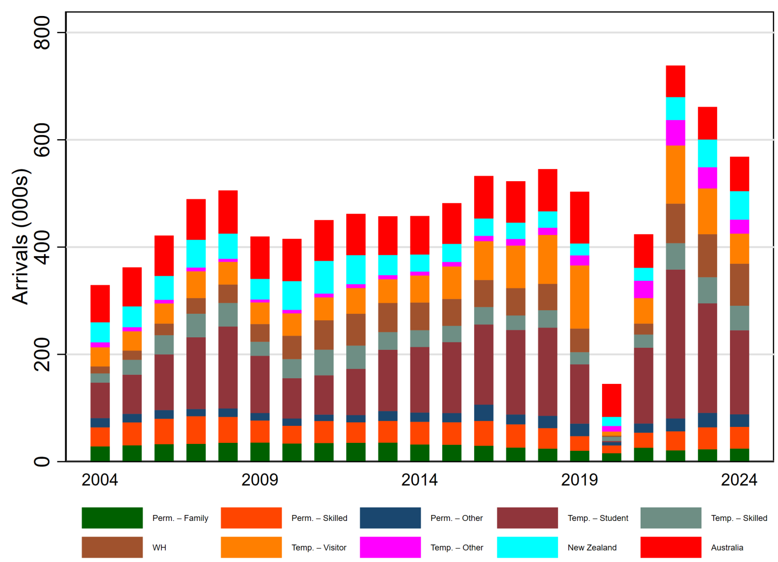
2.3 Visa Types

Figure 2 decomposes Australia's migration flows by visa category over time, separately for net flows and gross arrivals. Among new arrivals, migration to Australia is overwhelmingly driven by temporary visas, rather than permanent entry. Student visas, temporary skilled visas, and visitor or working-holiday visas together account for approximately 64.3% of arrivals and 78.6% of net migration over most of the sample period.

[6]In Appendix Sections D and E, we further examine the role of migrants from different source countries by studying heterogeneous responses and by analyzing Rotemberg weights to identify which origin-country shocks are the primary drivers of our estimates.



Panel A - Net Overseas Migration by Visa Type



Panel B - Arrivals by Visa Type

Figure 2: Composition of Australian migration flows by visa type.

Notes: Panel A reports net overseas migration, while Panel B shows gross arrivals, decomposing flows into permanent and temporary visa categories. The red bar represents returning Australian citizens.

Official Australian migration statistics apply a residence-based definition rather than a visa-based classification: a person is counted as an overseas migrant if they reside in Australia for 12 months or more within a 16-month period. Our empirical analysis adopts this same definition, as we detail in Section 4. While permanent visas grant immediate settlement rights, many temporary visas confer long durations of stay, often with renewal options. As a result, individuals on temporary visas frequently satisfy the statistical residency threshold used to identify migrants in the data. For example, student visas typically allow individuals to remain in Australia for several years: three years for a standard undergraduate degree, often followed by a two-year post-study visa. Similarly, working-holiday visa holders may stay for two or three years after completing specified regional or agricultural work, while temporary skilled visas are generally granted for two to four years with renewal options. Consequently, migration flows measured under the residence-based definition encompass a substantial share of temporary visa holders whose duration of stay exceeds the 12-month threshold.

2.4 Housing Tenure

Temporary Visa Holders. During the 2016-17 to 2023-24 period, temporary visa holders could purchase both established (existing) and new residential properties, but were required to navigate significant regulatory requirements and additional costs. All temporary residents needed to obtain approval from the Foreign Investment Review Board (FIRB) before purchasing any residential real estate. The FIRB application fees were tiered based on property value and increased substantially over this period. For properties valued at $1 million or less, the fee was initially $5,600, while for properties over $1 million, fees increased for every additional $1 million of consideration.[7] The approval process typically took one to two months.

Beyond FIRB requirements, temporary visa holders faced substantially higher financial costs compared to Australian citizens and permanent residents. Both New South Wales and Victoria, the two largest property markets, imposed significant foreign buyer surcharges on stamp duty. These surcharges ranged from 4 to 8 percentage points depending on the state and year of purchase. Foreign buyers also typically needed to provide larger mortgage deposits (20-30% versus 10% for citizens) and were subject to annual land tax surcharges in some states.[8]

In all, temporary visa holder activity in property purchases represents a very small fraction of Australia's overall real estate market. In the 2022-23 financial year, the Register of Foreign Ownership recorded 5,360 residential property purchases by foreign persons (which includes temporary visa holders along with other foreign investors) valued at $4.9 billion (Australian Taxation Office, 2024). To put this in perspective, Australia hosted approximately 556,000 temporary migrants during 2022-2023, meaning that even if every one of these 5,360 property purchases were made by temporary visa holders, fewer than 1% of temporary migrants purchased property. These figures strongly suggest that the overwhelming majority of temporary visa holders in Australia are renters rather than property owners.

[7] All dollar figures are in Australian dollars. The AUD/USD exchange rate ranged between approximately 0.66 and 0.77 USD per AUD over the sample period.

[8] The only exception to FIRB approval applied when purchasing jointly with an Australian citizen or permanent resident spouse.

Permanent Visa Holders. Descriptive evidence on housing tenure among newly arrived *permanent* migrants is provided by the Continuous Survey of Australia's Migrants (CSAM), administered by the Australian Department of Home Affairs. The CSAM tracks settlement outcomes of permanent migrants in the Skill and Family streams at multiple stages after arrival, including at approximately 18 months and 2.5 years of settlement.

At the 18-month stage of settlement, the majority of permanent migrants reside in rental accommodation, with only 34% owning their home either outright or with a mortgage. Thus, even among permanent residents, rental housing dominates well beyond the first year after arrival. By the 2.5-year settlement stage, rental rates decline but remain substantial, with ownership rising to 41%.

2.5 Comparison with Other Countries

Australia stands among the leading OECD destinations for migrant inflows, with its position strengthening considerably once temporary inflows are taken into account. In 2017, Australia admitted 218,000 permanent migrants, ranking 7th among OECD countries behind the United States (1.1 million), Germany (860,000), the United Kingdom (342,000), Spain (324,000), Canada (287,000), and France (259,000). Yet this measure, which captures individuals granted permanent residence on arrival, those on effectively indefinitely renewable permits, and those whose status changed from temporary to permanent, does not fully reflect Australia's immigration intensity. Temporary inflows are substantial: the same year, Australia issued 163,000 residence permits to international tertiary-level students (3rd in the OECD, ahead of Canada, France, and Germany) and admitted 211,000 working holidaymakers, far more than any other OECD country and roughly double the United States' intake (OECD, 2019). Relative to its population of 24.6 million in 2017, these figures are without parallel among the countries considered.

Immigrants in Australia are, on average, substantially more educated than immigrants in most other OECD countries. Almost 60 per cent of the foreign-born population aged 25-64 in Australia holds a tertiary qualification, compared with around 40 per cent among immigrants across the OECD as a whole (OECD, 2023). Australia thus ranks among the countries with the most highly educated migrant populations, alongside Canada, and well above major migrant-receiving economies such as the United States and most continental European countries. Moreover, the education gap between immigrants and the native-born is particularly large in Australia: the share of tertiary-educated migrants exceeds that of the native-born by roughly 20 percentage points (pp), a difference that is larger than in any other major OECD destination – roughly twice the UK's 10pp gap and about four times Canada's 5pp gap – and stands in contrast to continental Europe and the United States, where migrants are on average less educated than the native-born.

Together, the features described throughout this section define a migration setting with few direct analogs: unusually high immigration intensity, a tightly managed visa system, inflows dominated by temporary visas with established pathways to permanent residency, and a migrant population that is substantially more educated than both natives and migrants elsewhere. The remainder of the paper studies how these inflows shape Australian neighborhood housing markets.

3 Empirical Methodology

Consider the following OLS specification, which is standard in the literature, regressing the log change in outcome y_{gst} (e.g., house prices), where g denotes the neighborhood in state s and year t:

$$\Delta \log y_{gst} = \alpha + \beta \left(\frac{immigrants_{gst}}{population_{gs,t-1}} \right) + \delta \mathbf{X}_{gs,t-1} + \gamma_{st} + \lambda_g + \varepsilon_{gst} \tag{1}$$

The primary coefficient of interest is β, which captures the effect of immigrant inflows during period t, expressed relative to the previous year's population, on the outcome. In other words, β captures the effect of population growth through immigration only. This specification includes neighborhood-level fixed effects, controlling for time-invariant neighborhood characteristics, as well as time-varying controls $X_{gs,t-1}$.[9] In addition, state-time fixed effects are included to account for state-level aggregate fluctuations in the outcome. Thus, one possible interpretation of β is as the effect of a 1% immigration inflow on the percent change in the outcome (e.g., house price), relative to the average change experienced in a given SA2, conditional on state-level shocks and holding constant time-varying local characteristics.[10]

3.1 Threats to Identification

A key challenge in estimating the causal effect of immigration is the endogeneity of settlement patterns: immigrants are not randomly distributed across neighborhoods. Instead, they sort into areas based on, for example, pre-existing local conditions, social networks, and housing market characteristics. To the extent that these factors are correlated with local housing market trajectories, OLS estimates of β in equation (1) are biased. For instance, if immigrants systematically settle in areas where rents are expected to decline, the bias is downward. Conversely, if they are drawn to areas with improving labor market conditions, which independently drive housing demand, OLS estimates will instead tend to be upward biased. Addressing these concerns requires a source of variation in immigration inflows that is orthogonal to unobserved local housing market conditions that could also drive outcomes.

An idealized setup to address these concerns is one in which immigration is random at the origin-country level. In this case inflows would constitute exogenous "shifts" in immigrant supply. Because different neighborhoods historically hosted immigrants from different countries of

[9] The term $\beta \left(\frac{immigrants_{gs,t}}{population_{gs,t-1}} \right)$ is usually introduced with a lag. In the next section, we explain why the timing we assume implies a 6-month lag in the price growth observation, relative to the immigration period. We also show dynamic responses, which evaluate the coefficient β for different time horizons.

[10] In additional robustness tests, we also include lags of both the dependent and independent variables.

origin, these national-level shocks would translate into differential neighborhood-level exposure. Crucially, in this example, the magnitude of the national surge from country c is unrelated to the unobserved characteristics of neighborhoods that historically hosted immigrants from c, and thus the resulting variation in local immigration inflows is as good as randomly assigned.

Our shift-share instrument is motivated by this logic, following the immigrant enclave approach pioneered by Altonji and Card (1991). We interact time-varying national immigration flows from country c – the "shifts" – with the predetermined share of immigrants from country c residing in each SA2 – the exposure "shares". The key identifying assumption is that of *shift exogeneity*, as proposed by Borusyak et al. (2022): conditional on SA2 fixed effects, state-year fixed effects, and controls, aggregate immigration inflows by country of origin are orthogonal to unobserved neighborhood-level determinants of housing outcomes.[11]

In specification (1), state-year fixed effects λ_{st} absorb aggregate fluctuations in housing markets that are common within a state in a given year, such as state-level policy changes or broad demographic trends specific to a state.[12] Identification thus comes from variation across neighborhoods within the same state and year. The vector of time-varying controls $\mathbf{X}_{gs,t-1}$ further absorbs neighborhood characteristics that evolve over time and could be correlated with both immigration inflows and housing market outcomes, such as population density, the share of apartment dwellings, and the share of college-educated residents.

More importantly, SA2 fixed effects λ_g absorb all time-invariant neighborhood characteristics that could simultaneously attract immigrants and drive housing market outcomes, such as proximity to employment centers, access to amenities, or the historical composition of the local population. Identification therefore asks: in years when a neighborhood received more immigrants than its own historical norm, did rents also grow faster than its own norm? This is a considerably cleaner comparison, as it differences out any time-invariant reason why some neighborhoods grow faster than others on average. Finally, the inclusion of SA2-level fixed effects also helps address the dynamic-adjustment concerns raised by Jaeger et al. (2019), as detailed in the next section.

Formally, the instrument $\mathcal{Z}_{gst}$ for immigration inflows into SA2 g in state s at time t is con-

[11] An alternative approach relies on the exogeneity of the *shares* rather than the shifts, as proposed by Goldsmith-Pinkham et al. (2020). Under this approach, the predetermined settlement shares are themselves treated as valid instruments, with identification resting on a parallel trends condition: neighborhoods with higher exposure to immigrants from country c would have experienced similar housing market trajectories in the absence of immigration shocks. We nevertheless prefer to ground identification in the exogeneity of the shifts, for two reasons. First, although settlement shares exhibit a very weak correlation with changes in most observable urban characteristics, such as population density and median age, they do exhibit a mild correlation with changes in the percentage of college educated, raising concerns about the parallel trends assumption. Second, we believe the shift exogeneity approach has the advantage of grounding identification in a cleaner economic narrative: country-level immigration flows are driven by conditions in origin countries and are plausibly unrelated to housing market dynamics in destination neighborhoods. As we show below, country-level immigration shifts are indeed uncorrelated with the observable characteristics of the neighborhoods historically associated with each origin country.

[12] Fluctuations that are common across all states, such as national interest rate cycles, are absorbed by the year component of the state-year fixed effects.

structed as:

$$\mathcal{Z}_{gst} = \sum_{c \notin \text{Aus}} z_{gstc}, \quad \text{where} \quad z_{gstc} = s_{cgs} \times nom_{ct} \tag{2}$$

where nom_{ct} represents total net overseas migration from country c to Australia in year t.[13] Following the previous literature, we define the time-invariant exposure shares as:

$$s_{cgs} \equiv \frac{pop_{cgs0}}{\sum_g pop_{cgs0}},$$

where pop_{cgs0} is the number of immigrants from country c living in SA2 g, state s in a base year.

Because exposure shares do not sum to one across countries within each SA2, the instrument is a share-weighted *sum* rather than a weighted average. In principle, SA2s with larger exposure could receive mechanically higher instrument values independently of the shifts – the incomplete shares concern discussed in Borusyak et al. (2022). In our setting, this is addressed by SA2 fixed effects: because settlement shares are time-invariant, the sum $S_{gs} \equiv \sum_c s_{cgs}$ is absorbed as a time-invariant SA2-level characteristic.

Inference. Because the identifying variation operates at the level of country-year shifts rather than at the SA2 level, valid inference requires standard errors that reflect the correlation in residuals across neighborhoods exposed to the same origin-country shocks. Following Adão et al. (2019), we report exposure-robust standard errors, computed via the equivalent country-level IV regression as in Borusyak et al. (2022). Conventional clustering at the SA2 level, which is the standard in the existing literature, ignores this source of correlation.

Before presenting further evidence in favor of the validity of the instrument in Section 5, we now turn to describing the data.

4 Data

4.1 Geographical Unit of Analysis.

Our entire analysis is conducted at the Statistical Area 2 level (SA2) as determined by the Australian Statistical Geography Standard (ASGS) in 2016, defined by the Australian Bureau of Statistics (ABS). These geographical regions are the smallest spatial units used by the ABS for the release of non-Census Population and Housing data.

According to the Australian Bureau of Statistics, SA2s are designed, where possible, around "whole gazetted suburbs or rural localities" in order to make the regions "as meaningful as possible to users unfamiliar with the statistical geography". In regional and remote areas, gazetted

[13] We exclude Australians from the computation, as the logic of immigrant enclaves clearly does not apply to the native-born population. A similar concern could apply to immigrants from New Zealand and other English-speaking countries. In Appendix C we show that the results are unchanged if we exclude New Zealand and the United Kingdom.

localities are often too small to constitute an SA2 on their own and are therefore combined based on whether they form part of what the ABS describes as a "functional area".

In major cities, SA2s often represent single suburbs and are usually named after them. However, because suburb size varies within and across cities, they do not always provide a convenient unit to be used directly as an SA2. In such cases, five general criteria are used to cluster smaller suburbs together or divide very large suburbs: "a shared road network," "shared community facilities," "Local Government Area boundaries", "shared historical or social links," and "socio-economic similarity." These design features suggest that immigration inflows measured at the SA2 level are likely to translate into meaningful social exposure through shared amenities such as schooling environments, public spaces, recreational activities, and local service use.

Our final sample features 2265 SA2s, with median population and area of approximately 10,000 residents and 13.4 square kilometers. Figure 3 displays the SA2 boundaries in New South Wales and Victoria, the two most populous states, with shading intensity reflecting their population levels in 2016.

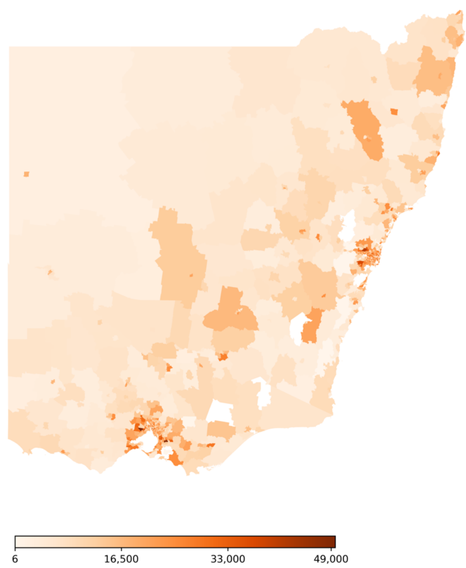


Figure 3: Map of Level 2 Statistical Areas - New South Wales and Victoria

Notes: Statistical Area level 2 (2016 version) boundaries. Color intensity is proportional to population in 2016.

4.2 Prices and Rents

Our residential real estate prices and rents information is provided by Corelogic via the Australian Urban Research Infrastructure Network (AURIN). It includes details on the monthly average, median, and distributional statistics of sales prices and rents of houses and units. Note that in Australian property terminology, "units" refers to apartments, flats, and similar dwellings in multi-unit developments, including some townhouses and semi-detached dwellings. In contrast, "houses" refers to detached single-family dwellings.

We aggregate monthly statistics to the yearly level using weights proportional to the number of transactions in each month. Our preferred statistic refers to median prices and rents at the SA2-level, and we report analogous results for averages in Appendix C.

Our final sample covers 2017-2023, to match the yearly available information on net overseas migration, as we explain in the next subsection. Over the period considered, median SA2-level house prices rose by 5.9%. Rents also experienced strong growth over the period, but not as pronounced.

4.3 Migration

The source for SA2 level migration data is the ABS "regional population components" publication (Australian Bureau of Statistics, 2023), which provides yearly data on SA2-level overseas arrivals and departures, and net overseas migration. The latter consists of our explanatory variable. Importantly, these data are constructed using the residence-based 12/16 rule: individuals are classified as net overseas migrants if they remain in Australia for 12 months or more within a 16-month period, and are removed if absent for the same duration. The classification is therefore independent of visa category and applies equally to permanent and temporary migrants who satisfy the residency criterion. Overseas migration to and from Australia's sub-state areas cannot be directly measured and is thus estimated from Census responses indicating residence overseas a year prior, with adjustments for changes in migration patterns using visa and student data.[14] To the extent that this introduces classical measurement error in our explanatory variable, our instrumental variable approach helps mitigate the resulting attenuation bias.

The "shift" component of our instrument (expression (2), term nom_{ct}) is obtained using data on yearly net overseas migration flows, also sourced from the ABS. These aggregate flows are based on border crossing records and are defined using the same 12/16 residence rule. Thus, both the SA2-level migration variable and the country-level shift component rely on the same definition of net overseas migration and include all individuals who meet the statistical residency threshold, regardless of visa type.

Both the aggregate and SA2-level migration data are supplied at the financial year level, which in Australia begin on the first of July. We convert this timing into yearly by, for instance, assigning the 2016-17 observation to the year 2017. This means that our primary estimates from equation (1) take into account a 6-month gap between the aggregation period of prices and rents and that of immigration flows.

Finally, to construct the initial migrant stock pop_{cgs0}, we use data from the 2006 Australian Census, drawing on respondents' reported year of arrival to recover the number of residents who had arrived by 2001 and were residing in a given SA2 in 2006. Baseline shares are therefore anchored to place of residence as of 2006, but we restrict attention to individuals who had arrived by 2001 for two reasons. First, using a distant baseline ensures that local origin shares are measured

[14] Regional Overseas Migration Estimate departures are modeled based on past overseas arrivals, weighted by education levels and the proportion of overseas-born residents.

well before the estimation period, as suggested by Borusyak et al. (2025). Second, the publicly available 2001 Census data (via the ABS Microdata Builder) do not provide sufficient spatial granularity to be consistently mapped into 2016 SA2 boundaries, which we use throughout the analysis. Note that the resulting baseline shares therefore reflect the composition of long-term residents as observed in 2006, traced back to their arrival cohort.

As a robustness check, we alternatively construct the instrument using contemporaneous 2006 country-of-origin shares, without tracing arrival cohorts back to 2001, and re-estimate our main specification. The effects are quantitatively very similar, suggesting that our results are not sensitive to the choice of baseline year or to the survivor-based reconstruction of the 2001 stock. These results are reported in Appendix C.

4.4 Controls and Other Variables

Controls. We control for the proportion of college-educated individuals, apartments as a share of the total dwelling stock, median age, (the log of) population, and population density. Except for the latter two, these variables are only available in census years, and thus we interpolate them for non-census years. Reassuringly, the inclusion of controls has limited impact on our estimates, reinforcing the validity of our instrument.

Heterogeneity Analysis and Mechanisms. We also incorporate data on building approvals at the SA2 level, sourced from the ABS. This variable measures the total number of new residential dwellings approved in a given SA2 each year and is an important indicator of changes in housing supply. This is illustrated by a strong correlation between building approvals and commencements, with a coefficient of 0.96 for houses and 0.93 for units throughout 2006-14 (Ong et al. (2017)). Finally, we also incorporate net *internal* migration from the ABS regional population components dataset.

4.5 Summary Statistics

Summary statistics of our variables are presented in Table 1. The average annual immigrant inflow at the SA2 level is 0.7%, with a standard deviation of 1.1%, reflecting the large variation across neighborhoods.

The last two rows report descriptive statistics for the shift-share instrument and the underlying country-year shifts. The instrument exhibits substantial variation across neighbourhoods and years, with a coefficient of variation of roughly 2. The effective sample size of 207 origin countries, computed following Borusyak et al. (2025), indicates that the instrument is well-diversified, and not driven by a single country or group of countries. We provide further evidence on this in Appendix D, where we show that the distribution of Rotemberg weights is quite dispersed and confirm that the results are not driven by any particular origin country or group of countries. Appendix Table B.1 further shows that residualizing the shifts on year fixed effects leaves

Table 1: Summary Statistics

Variable	Average	Median	Standard Deviation	p5	p25	p75	p95	Observations
Net Overseas Migration (count)	102.6	37.0	237.4	-17.0	10.0	116.0	445.0	15381
Net Overseas Migration	0.7	0.4	1.1	-0.2	0.2	0.9	2.6	15402
Area (square kilometers)	3240.6	13.4	24774.9	2.0	5.6	118.6	6527.2	15414
Population	11582.5	9958.0	7548.8	2732.0	5694.0	16189.0	25178.0	15414
Dwelling Approvals	91.2	39.0	190.2	1.0	13.0	95.0	338.0	15414
Approvals per 1000 inhabitants	8.7	3.9	105.0	0.2	1.8	7.6	22.1	15381
Dwelling Approvals (per square kilometer)	10.1	2.0	36.2	0.0	0.2	8.7	37.0	15414
Total Residential Dwellings	4416.0	3956.0	2590.8	1069.0	2383.0	6245.0	9171.0	2065
Apartment Ratio (%)	10.8	2.8	18.3	0.0	0.6	11.5	56.8	2063
University Educated (%)	19.5	16.1	11.6	6.6	10.2	26.5	42.5	2056
Median Age	39.7	37.0	6.6	32.0	37.0	42.0	52.0	15414
Yearly % Change - Unit Rents	3.9	3.3	18.8	-12.0	-0.4	8.1	20.9	10873
Yearly % Change - Unit Prices	4.7	3.6	28.0	-26.8	-2.8	11.4	39.2	9011
Yearly % Change - House Rents	4.4	3.6	9.9	-5.9	0.5	8.3	16.6	14585
Yearly % Change - House Prices	5.9	5.3	15.5	-11.8	-0.2	12.4	24.8	14680
Instrument ($\mathcal{Z}_{gst}$)	106.8	35.5	207.6	-25.1	9.5	118.5	504.3	15414
Shifts (nom_{ct})	912.2	10.0	5339.2	-100.0	0.0	180.0	4650.0	207

Notes: p5, p25, p75, and p95 refer to the respective percentiles of the distribution. The sample is restricted to years 2017-23, except migration and population data, which are based on financial years 2016-17 to 2022-23. Statistics calculated at the SA2-year level. The apartment ratio, the percentage of university educated, and the total number of residential dwellings are interpolated from census data. Rents and prices are aggregated to the annual level using weights proportional to the number of transactions in each month. For the shift-share instrument $\mathcal{Z}_{gst}$, the last column reports the number of SA2-year observations. For the shifts nom_{ct}, the last column reports the effective sample size computed as the inverse of the Herfindahl-Hirschman Index of the country weights $w_c \equiv \sum_{gs} s_{cgs}$, as suggested by Borusyak et al. (2025). The number of country-year pairs in the final sample is 2,880.

their distribution virtually unchanged, confirming that the identifying variation is predominantly cross-country rather than time-series.

Changing SA2 Boundaries Over Time. Australia's evolving statistical geographic standards (ASGS) present a significant challenge for measuring immigrant arrivals consistently over time. To address this, we employ population-weighted correspondence tables from the Australian Bureau of Statistics (2021), which enables the translation of data across geographic boundaries, specifically from older Statistical Local Areas (SLAs) in 2001, 2006, and 2011 to the more recent Statistical Area Level 2 (SA2) regions, implemented in 2016. We also construct population-weighted correspondence tables to align SA2 boundaries from 2021 (and beyond) to 2016, using the 2016-21 SA2 correspondence and deriving reverse weights based on 2021 SA2 population estimates.

5 Instrumental Variable Validation

We now assess the plausibility of the identifying assumption introduced in Section 3. Our strategy is threefold. First, we test whether country-level immigration shifts are systematically related to predetermined destination-side characteristics. Second, we examine instrument relevance by reporting first-stage coefficients and Kleibergen-Paap statistics across outcomes. Third, we address the concern raised by Jaeger et al. (2019) that serially correlated shifts may interact with dynamic adjustments to bias estimates. We discuss each in turn.[15]

Consider the following (static) setting with two groups of SA2s: Group H, consisting of areas with favorable unobserved local factors ("amenities") that influence housing prices, and Group L, consisting of areas where such factors are less pronounced. Suppose migrants from Country A predominantly locate in Group H, while migrants from Country B predominantly locate in Group L. If inflows are systematically larger for Group H SA2s, then shifts will be correlated with unobserved local amenities that also drive outcomes: our instrument would be invalid.

In our setting, the plausibility of shift exogeneity rests on the nature of the variation in origin-country migration flows. Annual fluctuations in net overseas migration by origin are driven by conditions in the source country as well as by Australian federal visa policy, which primarily determines the volume and composition of inflows at the national level. These generate variation in the volume of inflows from each origin that is plausibly orthogonal to unobserved local housing market conditions in individual SA2s. As illustrated in the example above, the identifying assumption would be violated if, for instance, Australia systematically admitted more migrants from origins whose historical settlement patterns happened to concentrate in SA2s experiencing idiosyncratic housing demand shocks, or particular price or rent trends.

Following Borusyak et al. (2025), we can assess the plausibility of this identifying assumption by examining whether immigration shocks are systematically related to a set of country-level covariates constructed from destination-side characteristics. Formally, for a given origin country, we construct covariates as weighted averages of SA2-level variables. These objects summarize the (characteristics of) typical destinations associated with each origin country. The underlying logic is that if shifts are orthogonal to observable destination-side characteristics, concerns that they also correlate with unobserved determinants of housing outcomes are mitigated.

Note that, because our main specification includes SA2 fixed effects, time-invariant level differences across neighborhoods are absorbed. The relevant diagnostic therefore concerns *changes* in destination characteristics, which would indicate differential trends correlated with immigration exposure. Because census covariates are observed at most twice in our sample period, we take five-year long differences, which is the cross-sectional analogue of including unit fixed effects.[16] For completeness, Table B.2 in Appendix Section B also reports the analogous tests using baseline *levels* of destination characteristics.

Panel A of Table 2 reports the results. Coefficients are statistically insignificant across all specifications for both census dates, suggesting that inflows are not disproportionately concentrated in SA2s with particular local characteristics. While these tests cannot rule out the possibility that shifts correlate with unobserved destination characteristics, such as local amenities or crime trends, the absence of systematic patterns across a broad set of observables lends support to the plausibility of the identifying assumption.

We complement the shift-level analysis by examining whether the shift-share instrument itself

[15] Note that shift exogeneity does not require exposure shares to be uncorrelated with outcomes or baseline local characteristics.

[16] Consequently, the incomplete share control is unnecessary in this specification.

Table 2: Balance Tests – Changes in Destination Characteristics

	Δ Apt. ratio	Δ Educ. share	Δ log Pop.	Δ Median age
Panel A: Shift-level				
NOM (2016)	0.140	0.038	0.100	-4.168
	(0.209)	(0.032)	(0.129)	(3.128)
NOM (2021)	0.080	0.014	-0.009	3.954
	(0.094)	(0.060)	(0.162)	(3.398)
Panel B: Instrument-level				
IV (2016)	0.202	0.109*	0.125	-8.018
	(0.183)	(0.056)	(0.229)	(5.252)
IV (2021)	0.112	0.124	-0.128	4.817
	(0.071)	(0.095)	(0.222)	(8.523)
Obs.	206	206	206	206

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SA2-level covariates are measured in five-year long differences and aggregated to the country level across SA2s using exposure shares as weights, which yields exposure-robust standard errors (Borusyak et al., 2022) . Row labels indicate the census year at which covariates are measured; long differences correspond to 2011-16 and 2016-21, respectively. In Panel A, covariates are regressed on average NOM (in millions) over 2017–2023. In Panel B, covariates are regressed on the share-weighted shift-share instrument (re-scaled by a factor of 1000, for readability).

correlates with trends in destination characteristics. To do so, we aggregate both the instrument and each covariate to the country level using exposure shares as weights, and regress the growth in covariates on the aggregated instrument, yielding exposure-robust standard errors. Panel B of Table 2 shows that results are largely insignificant, with the exception of the coefficient on the change in education share at the 2016 census. In all, the absence of systematic correlations between the instrument and pre-existing trends in destination characteristics provides further support for the plausibility of the identifying assumption. As before, we emphasize that the absence of correlation with observable trends does not rule out correlation with unobservables, but it lends credibility to the fact that such correlations are unlikely.

Relevance - First Stage Results. Table 3 reports the first stage of the country-level analogue regressions proposed by Borusyak et al. (2025). Note that, across outcomes, samples vary slightly due to differences in the availability of price and rent data by dwelling type. Yet the strength of the first stage is robust across all four outcome categories: the instrument is positively and significantly associated with the endogenous regressor at the 1% level, confirming that the shift-share instrument is a strong predictor of local immigration inflows. Kleibergen-Paap F-statistics range from approximately 21 to 28, comfortably exceeding conventional thresholds for weak instruments.

To assess whether the instrument satisfies a monotonicity condition, we compute country-

Table 3: First Stage to Regressions in Table 4

	Panel A: Unit Rents		Panel B: Unit Prices	
Instrument	0.005***	0.003***	0.004***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
# Country-level Obs.	2,880	2,880	2,880	2,880
Kleibergen-Paap F-stat.	22.420	24.421	20.847	22.445
Controls	No	Yes	No	Yes
	Panel C: House Rents		**Panel D: House Prices**	
Instrument	0.006***	0.004***	0.006***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
# Country-level Obs.	2,880	2,880	2,880	2,880
Kleibergen-Paap F-stat.	24.737	27.482	24.787	27.732
Controls	No	Yes	No	Yes

Notes: Each column reports the first stage regression results to the IV estimates reported in Table 4. Exposure-robust standard errors, shown in parenthesis, are computed as suggested by Borusyak et al. (2022). The dependent variable is the share-weighted average of residualized immigration flows, and the instrument is net overseas migration by origin country (in thousands). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Samples differ across panels because SA2-level price and rent data availability varies by dwelling type, which affects the set of country-year observations entering the share-weighted aggregation.

specific first-stage coefficients and examine those associated with the largest Rotemberg weights.[17] Among the top 30 origin countries ranked by the absolute value of their weight, all first-stage coefficients are positive. While 18 countries in the full sample exhibit negative first-stage coefficients, these are associated with negligible Rotemberg weights (averaging 0.0003 in absolute value). These results are reported in Appendix Section D.

Serial Correlation and Dynamic Adjustments. A remaining concern in shift-share designs is that serially correlated country-level migration shocks may interact with dynamic adjustment mechanisms to bias estimates, as emphasized by Jaeger et al. (2019). The intuition is as follows: if the spatial distribution of immigrant inflows is persistent over time, the shift-share instrument is correlated not only with current but also with past immigration. If the housing market is still adjusting to those past inflows, the conventional estimator conflates the contemporaneous demand effect with the ongoing adjustment to previous shocks. In our data, the shift-share instrument exhibits somewhat high serial correlation at the SA2 level (0.76).[18]

We address this concern in two ways. First, as discussed in Borusyak et al. (2025) (Online Appendix A.1), when shares are time-invariant, including unit fixed effects removes any time-invariant shift-level confounders, so that identification leverages only the idiosyncratic compo-

[17]By monotonicity, we mean that a positive national inflow from country c leads to an increase in local immigration in SA2s with high exposure shares from that country. Rotemberg weights measure the contribution of each origin country to the overall shift-share estimate. Countries with larger weights exert more influence on the estimated coefficient. See Goldsmith-Pinkham et al. (2020) for a formal derivation.

[18]For realized net migration flows at the SA2 level, the serial correlation is 0.46.

nent of shifts. Our shares are fixed at 2001 and SA2 fixed effects are included in all specifications. Indeed, after absorbing SA2 fixed effects, the instrument's serial correlation falls from 0.76 to 0.23, indicating that the bulk of the persistence is cross-sectional and absorbed by design.[19]

Second, while the fixed effect substantially reduces the instrument's serial correlation, the remaining deviations' small persistence could still lead estimates to partly reflect residual autocorrelation rather than the contemporaneous effect of immigration. To address this possibility, Section 6 presents specifications that include SA2-specific linear time trends and interactions of year fixed effects with baseline SA2 characteristics, thereby absorbing neighborhood-specific trends in outcomes that could otherwise be caused by persistent patterns in local immigration inflows. Our results are robust to these alternative specifications, although standard errors increase as expected.

Finally, we note that in our setting, the direction of this potential bias is conservative. The contemporaneous demand effect of immigration on rents and prices is positive, while the dynamic adjustment operates in the opposite direction. To the extent that the Jaeger et al. (2019) concern applies, the bias would therefore attenuate our positive estimates.

6 Results

The estimates of our main specifications, based on expression (1), are shown in Table 4.

Panels A and C display the impact of immigration on rents. As a starting point, note that the OLS coefficients are smaller than their IV counterparts. This is consistent, for instance, with a downward bias caused by immigrants selecting to live in locations with lower rent trends. It is also consistent with an attenuation bias stemming from measurement error.

An increase in net overseas inflow leads to a statistically significant rise in both house and unit rents. Specifically, in our preferred specification (IV with controls), a 1 percentage point increase in net overseas migration as a share of the SA2 population results in an increase in rents of approximately 2.8% for houses and 4.3% for units.

To the best of our knowledge, our study is the first to assess the short-term impact of immigration on rents at such a disaggregated level, setting it apart from existing research on short-term effects. With this distinction in mind, our estimates are notably larger than those found in comparable studies. For example, using yearly data at the Swiss canton level (average population circa 300,000), Helfer et al. (2023) estimate a 2.2% increase in apartment rents due to net immigration. Similarly, Unal et al. (2024) report a 0.9% effect at the German district level, comprising 382 administrative districts, close to Saiz (2007) estimate using Metropolitan-Statistical-Area-level data for the United States.[20]

The stronger effects observed in our study may reflect severe housing supply constraints in Australia (Kendall and Tulip, 2018; Productivity Commission, 2025). In the next section, we explore this possibility through a heterogeneity analysis, examining whether the effects vary based

[19] The autocorrelation is further reduced to 0.21 after the inclusion of state-year fixed effects and controls.

[20] Unal et al. (2024) do not report average district population, while the average MSA population in Saiz (2007) is around 2.4 million.

on local housing supply conditions.

Table 4: Impact of Immigration on Rents and Prices

	Panel A: Unit Rents				Panel B: Unit Prices			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	4.101***	4.316***	0.755***	1.007***	2.809***	3.217**
	(0.157)	(0.177)	(0.803)	(1.167)	(0.212)	(0.282)	(0.936)	(1.446)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	22.420	24.421	-	-	20.847	22.445
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.617***	2.819***	-0.300	-0.494**	-0.838	-1.053
	(0.141)	(0.145)	(0.451)	(0.668)	(0.184)	(0.218)	(1.063)	(1.426)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	24.737	27.482	-	-	24.787	27.732
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects.

Panels B and D of Table 4 display the impact of immigration on prices. The OLS coefficients are smaller in magnitude than their IV counterparts for units, but larger (less negative) for house prices. Again, the inclusion of controls has a limited impact on IV estimates. Notably, exposure-robust standard errors are relatively large, and the IV estimates for house prices are not statistically significant at conventional levels.

In our preferred specification (IV with controls), a one percentage point increase in net overseas migration as a share of the SA2 population results in a 3.2% increase in unit prices, significant at the 5% level. The IV estimates for house prices are too imprecise to draw any conclusions about the direction of the effect, although dynamic impacts shown in Section 6.1 below point to a negative impact.

Comparing with the three studies mentioned above, Unal et al. (2024) find no significant impact on house prices and a 2.4% impact on apartment prices, and Helfer et al. (2023) find the impact on house and apartment prices to be 1.8% and 2.4% respectively. Our estimate for unit prices of 3.2% is notably larger than all of these, and could reflect the severity of housing supply constraints in Australia during our sample period.[21] In the context of Australia, Moallemi and Melser (2020) estimate the impact of immigration on house prices over 5-year periods from

[21] A plausible explanation for the smaller rent effects in Germany and Switzerland is that both countries have institutional features that dampen rent responses to demand shocks, such as Germany's *Mietpreisbremse*. Such features are absent in Australia, where rents are allowed to adjust more freely.

2006 to 2016 to be 1.1% for houses and 0.7% for units. The results concerning house prices differ from those of Moallemi and Melser (2020), though given the imprecision of our estimates, this comparison should be interpreted with caution.

The fact that rents respond more strongly than prices is consistent with immigrants in Australia being predominantly renters upon arrival. This could be due to preferences or to credit and regulatory constraints on buying, as we detail in Section 2. When buying and renting are imperfect substitutes, the additional demand from immigrants falls primarily on rents rather than passing through to prices.

A complementary mechanism is based on the fact that immigrants can be perceived as a negative amenity by incumbent residents. To explore this mechanism, in Appendix F, we develop a simple spatial equilibrium model extending Accetturo et al. (2014), with two neighborhoods, a choice between renting and owning, and differential amenity sensitivity across tenure types. The model can also rationalize our central patterns, namely that rent effects exceed price effects, and that the latter can be negative, provided that homeowners are more sensitive to immigration-induced amenity changes than renters. In Section 7, we provide empirical support consistent with the amenity channel, showing that the negative house-price effect is concentrated in older neighborhoods, where residents tend to hold less favorable views toward immigration (Lowy Institute, 2019, 2024; Roy Morgan, 2025).

Another possible mechanism behind the estimated price and rent increases is that newly arrived immigrants pay a markup relative to natives, either due to discrimination or informational disadvantages, rather than reflecting, for instance, a broader shift in housing demand. Under this interpretation, our estimates would capture immigrant-specific price effects rather than market-wide consequences for the native population, with different policy implications.

We believe this mechanism is unlikely to account for a substantial share of our estimates. The share of housing transactions involving newly arrived immigrants is small in both markets. For sales, as explored in Section 2, fewer than 1% of temporary migrants purchased property in 2022-23, and even among permanent migrants, the majority remain renters at 18 months after arrival. For rents, Table 1 shows that net overseas migration averages 0.7% of SA2 population annually, with a 95th percentile of 2.6%. Rental market turnover in Australia is approximately 2-3% of rental properties *per month*, a rate that has remained stable over our sample period (Australian Bureau of Statistics, 2025). Combined with a renter share of roughly 30% of households, recent immigrants account for approximately 8% of rental transactions in a typical SA2, and around 30% at the 95th percentile. Since our outcome is the median rent, these shares are too small to mechanically shift it through immigrant-specific pricing.

Turning to inference, under our preferred identifying assumption, conventional SA2-level clustering overstates precision. In our setup, the difference is substantial: for house prices, the exposure-robust standard error is roughly three times the conventionally-clustered alternative (see Table D.5 in Appendix D.1). As a result, the IV point estimate for house prices, which would have been statistically significant under conventional clustering, becomes indistinguishable from zero under exposure-robust inference, highlighting the importance of aligning inference with the level of identifying variation.

Robustness - Serial Correlation and Dynamic Adjustments. As discussed in Section 5, a concern in shift-share designs is that serially correlated migration shocks may interact with dynamic adjustment mechanisms to bias estimates (Jaeger et al., 2019). The primary way in which we address this concern is by including SA2 fixed effects in all specifications, which extract the idiosyncratic component of shifts by absorbing their time-invariant component (Borusyak et al., 2025). As reported in Section 5, this reduces the instrument's serial correlation from 0.76 to 0.21, indicating that the bulk of the persistence is cross-sectional and absorbed by design. Nonetheless, as an additional safeguard, we estimate specifications that include SA2-specific linear time trends and interactions of year fixed effects with baseline SA2 characteristics, absorbing neighborhood-specific trends that could otherwise be confounded with the residual persistence in immigration patterns.

Table 5: Summary of Results and Robustness Checks

	Base	SA2-Trend	Apartments	Univ. Educated	Log Pop.	Pop. Density	Age	(Δ) Apt.	(Δ) Pop. Density	(Δ) Univ.	(Δ) Log Pop.	(Δ) Age
House Rents												
Coefficient	2.819***	3.078***	4.006***	5.475***	4.269***	5.616***	3.105***	3.169***	3.529***	4.158***	3.093***	3.105***
Country-level SE	(0.668)	(0.755)	(1.023)	(0.770)	(0.722)	(2.029)	(0.875)	(0.780)	(0.896)	(0.805)	(0.753)	(0.740)
House Prices												
Coefficient	-1.053	-0.773	-0.473	2.640	-0.120	-0.568	-1.807	-0.618	-1.107	1.499	-0.723	-0.600
Country-level SE	(1.426)	(1.630)	(2.030)	(1.666)	(1.641)	(3.299)	(1.911)	(1.697)	(1.928)	(1.942)	(1.634)	(1.613)
Unit Rents												
Coefficient	4.316***	4.796***	6.328***	7.411***	4.713***	7.917**	5.233***	4.951***	5.775***	5.960***	4.788***	4.818***
Country-level SE	(1.167)	(1.127)	(1.591)	(1.972)	(1.198)	(3.637)	(1.511)	(1.215)	(1.465)	(1.423)	(1.118)	(1.172)
Unit Prices												
Coefficient	3.217**	3.363**	3.855**	5.192**	2.934**	7.627*	2.903	3.582**	3.777**	3.668**	3.332**	3.218**
Country-level SE	(1.446)	(1.512)	(1.957)	(2.098)	(1.393)	(4.388)	(2.417)	(1.604)	(1.913)	(1.741)	(1.505)	(1.628)

Notes: Each column reports IV estimates from the baseline specification with controls (Table 4) under an alternative time trend. "SA2-Trend" adds SA2-specific linear time trends; remaining columns interact year fixed effects with the indicated covariate, measured either in 2016 levels or, for columns labeled "(Δ)", as the 2011–2016 change. Exposure-robust standard errors, shown in parentheses, are computed using country-level regressions as suggested by Borusyak et al. (2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 displays the results. House rents are positive and statistically significant across all specifications, with point estimates ranging from approximately 2.6% to 5.6%. Unit rents display a similar pattern, with estimates ranging from approximately 4.1% to 7.9% and remaining statistically significant throughout. Notably, specifications that more flexibly control for neighborhood-specific trends yield larger estimates, consistent with the dynamic adjustment mechanism emphasized by Jaeger et al. (2019): to the extent that ongoing adjustments to past inflows attenuate contemporaneous estimates, controlling for these dynamics removes the attenuation. As noted, our baseline estimates are, if anything, conservative.

Unit prices remain positive and statistically significant across nearly all specifications, with point estimates clustering close to the baseline estimate of 3.2%. The one exception is the median age specification, where the estimate loses significance, though the point estimate remains positive and within two standard errors of the baseline. House prices are imprecisely estimated throughout, with large standard errors and coefficients that vary in sign across specifications.

Robustness - Other Tests. In Appendix C we show that the results are virtually unchanged if we construct the instrument using data on the distribution of immigrants in 2006, instead of 2001. In addition, Table E.2 in the Appendix replicates Table 4 excluding the Covid years. Point estimates are reassuringly similar to the baseline, and house and unit rents remain positive and significant, though unit price estimates lose precision.

Table C.3 in the Appendix replicates our baseline specification including one lag of both the dependent variable and immigration flows. The effects on rents are robust to this modification, with point estimates similar to those in the baseline and main conclusions unchanged. For unit prices, the lag specification yields a somewhat smaller point estimate (from 3.2% to 2.2%), with significance weakening to the 10% level, though the estimate remains comfortably within one standard error of the baseline. The more striking change is for house prices: the coefficient becomes substantially more negative (-2.8%) and statistically significant under exposure-robust inference. One interpretation is that including lags absorbs residual serial correlation, isolating the contemporaneous effect more cleanly. Notably, this is the only specification we attempted where the negative house-price effect is this large in magnitude. Together with the cumulative estimates soon presented in Subsection 6.1, which also turn negative and significant at one- and two-year horizons, this is suggestive of a genuinely negative effect on house prices, consistent with the amenity-based mechanism formalized in Appendix F.

Finally, Table C.1 in the Appendix replicates Table 4 using the *average* change in prices or rents as the outcome variable. The results are qualitatively unchanged with one exception: the positive effect on apartment prices becomes statistically insignificant. Interestingly, the lower impact on average unit prices (compared to the median) seems to be driven by lower impacts of immigration at the bottom of the within-SA2 distribution, as shown in Figure 5 in the next section.

We conclude this analysis with a word of caution. Our estimates should be seen as the *partial equilibrium* relative effect of immigration at the neighborhood level. We refrain from asserting the impact of immigration on the evolution of Australian residential prices and rents over the sample period, given that those depend on general equilibrium effects, including spatial effects and across-neighborhoods spillovers. The simple spatial equilibrium model in Appendix F formalizes this distinction, showing that city-wide aggregate responses lie above neighborhood-level estimates. To the extent that we find limited evidence of (large) native out-migration in our setting, at least relative to other studies, the model suggests that the gap between neighborhood-level and aggregate responses is likely to be modest. We return to this point at the end of Section 7.

6.1 Dynamic Effects.

Figure 4 presents the cumulative effect of net overseas migration over extended periods on the cumulative change in prices and rents. The relevant first-stage coefficients and F-statistics are presented in Table E.1 in Appendix E.[22] Rent effects are positive and statistically significant across all horizons for both houses and units, and broadly stable over time.[23] Unit prices are also positive and statistically significant throughout. House prices display a negative point estimate that becomes more pronounced and, notably, statistically significant at horizons 1 and 2, though remaining negative throughout.[24] This pattern is consistent with the findings of Sá (2015), Stillman and Maré (2008) and Accetturo et al. (2014), who also document negative effects of immigration on house prices. A possible interpretation is that immigrants are perceived as a negative amenity by incumbent residents. Indeed, in the simple model of Appendix F, this amenity channel endogenously generates the out-migration of natives.[25]

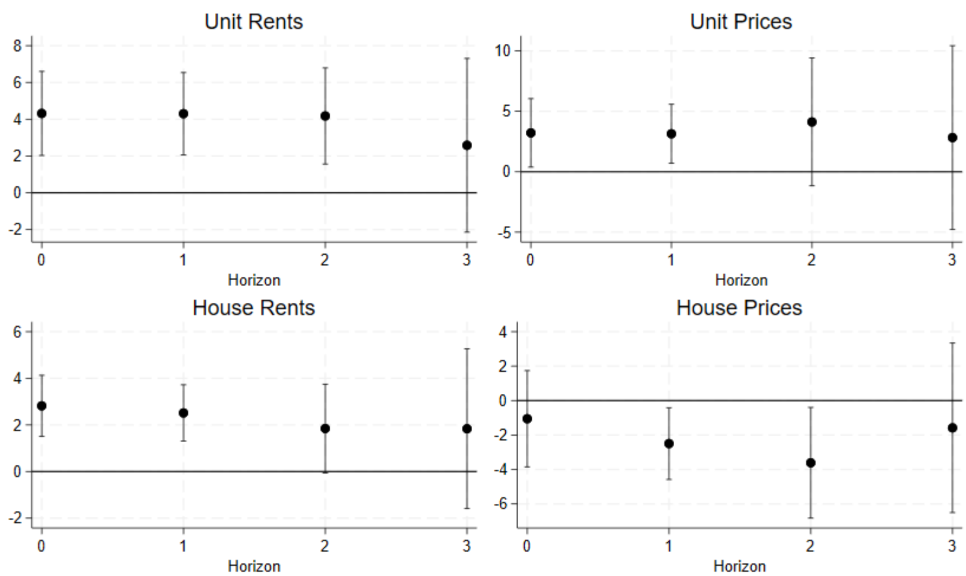


Figure 4: Cumulative Effects of Net Overseas Migration

Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Adão et al., 2019; Borusyak et al., 2022). All models include SA2 and state-year fixed effects, and controls include log population, population density, median age, apartment share, and university education rates. Regression replicates specification (1), but the dependent variable is replaced by $\log y_{gst} - \log y_{gs,t-1-J}$, the cumulative log change in the outcome over the last J periods, and the variable $\left(\frac{\text{immigrants}_{gs,t}}{\text{population}_{gs,t-1}}\right)$ is replaced by $\sum_{j=0}^{J} \frac{\text{immigrants}_{gs,t-j}}{\text{population}_{gs,t-1-J}}$, i.e., the cumulative immigration over the last J periods. The instrumental variable is adjusted accordingly.

[22] Instruments are strong up to horizon $h = 2$, but F-stats are low for $h = 3$.

[23] A similar comparison, but with annual vs. intercensal US data, is made in Saiz (2007), obtaining similar results.

[24] Standard errors become large at longer horizons due to the short length of our panel.

[25] Instead of cumulating both the outcome and immigration flows over time, in Appendix Subsection E.3 we estimate the impact of *current* immigration on outcomes at future horizons. For house prices, estimates are noisy at all horizons. For unit prices, the positive effect persists into $h = 1$ before fading. For rents, the contemporaneous effect turns negative and significant at $h = 2$, consistent with a medium-run supply response or with geographic resorting by immigrants.

7 Heterogeneous Effects and Mechanisms

This section explores how the effects of immigration on housing costs vary across the within-SA2 price and rent distribution and along several neighborhood characteristics, and examines whether immigration is associated with the internal migration of incumbent residents.

Effects Across the Price and Rent Distribution. Figure 5 presents estimates of equation (1) using different percentiles of the within-SA2 price or rent distribution as the dependent variable, rather than the median. The estimates corresponding to P50 match those reported in Table 4.

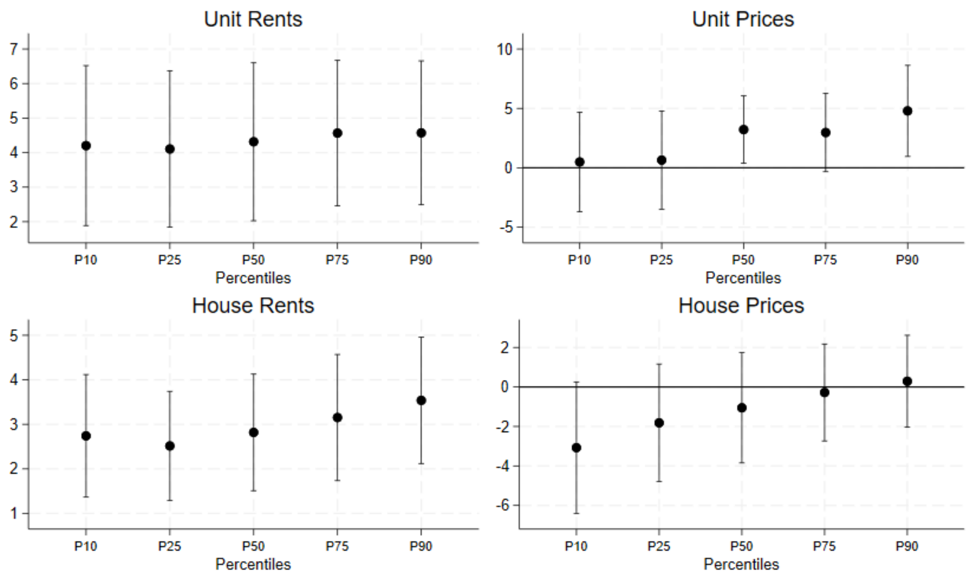


Figure 5: Effects Across the Price and Rent Distribution

Notes: Each panel plots IV estimates (with controls) of the effect of immigration on different percentiles of the within-SA2 price or rent distribution, using specification (1). Bands represent 95% confidence intervals based on exposure-robust standard errors (Adão et al., 2019; Borusyak et al., 2022). All models include SA2 and state-year fixed effects. Control variables include log population, population density, apartment ratio, median age, and university-educated share.

Rent effects are broadly flat, with point estimates similar across percentiles for both houses and units, though house rents exhibit a mild upward gradient. The overall pattern indicates that immigration raises rents across the board rather than in any particular segment. Price effects exhibit a more pronounced common gradient across dwelling types, with estimates becoming more positive as we move up the distribution. For units, the response rises monotonically with the percentile, with estimates insignificant and close to zero at the bottom and largest at the top, consistent with newly arrived immigrants competing for higher-quality dwellings. For houses, estimates follow the same upward gradient but largely remain in negative territory, most negative at the bottom and approaching zero at the top, suggesting that any negative house-price effect is concentrated in lower-valued properties.

Heterogeneity. We now investigate the heterogeneous impacts of immigration based on neighborhood characteristics and attempt to shed light on some of the mechanisms driving our results. To conduct a heterogeneity analysis, we modify equation (1) to:

$$\Delta \log y_{gst} = \alpha + \sum_{h=1}^{H} \beta_{h,W} \mathbb{I}(W \in \mathcal{Q}_h) \left(\frac{immigrants_{gst}}{population_{gs,t-1}} \right) + \delta \mathbf{X}_{gs,t-1} + \gamma_{st} + \lambda_g + \varepsilon_{gst}, \tag{3}$$

where $\mathbb{I}$ represents the indicator function, Q_h represents groups of a certain characteristics (for instance, quartiles), and W is the variable along which heterogeneity is analyzed.

Figure 6 displays the results, plotting point estimates along with exposure-robust standard errors.[26] Panel A (top-left) presents the heterogeneous effects across neighborhoods grouped by median age (fixed at 2016 values). There is a broad pattern of declining coefficients as we move from younger to older neighborhoods, suggesting that immigration has a weaker, or more negative, effect on housing costs in areas with older populations. This is particularly pronounced for house prices, where the estimated effect becomes significantly negative at the 10% level in the oldest quartile.[27]

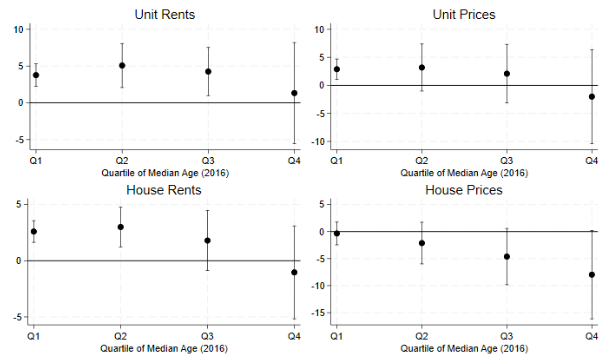
One possible interpretation is that immigrants may be perceived as a negative amenity by older homeowners, leading to a reduction in equilibrium rents and prices. Although attitudes towards immigrants in Australia are generally positive (see, e.g., O'Donnell et al. (2024)), there is substantial heterogeneity across age groups. In particular, older Australians are significantly more likely to view immigrants as a burden on the social welfare system and less likely to agree that immigrants strengthen the country (Lowy Institute, 2019), more likely to consider immigration levels too high (Lowy Institute, 2024), and more likely to cite managing immigration as a top-three concern (Roy Morgan, 2025). The pattern of declining coefficients in Panel A is consistent with these attitudinal differences.

Panels B and C examine heterogeneity by education, grouping neighborhoods by quartiles of the share of foreign-born residents with university education and the share of all residents with university education, respectively, both measured in 2016. Rent effects decline monotonically from the least- to the most-educated quartile in both panels, while price estimates show no clear pattern. If the share of foreign-born residents with university education is taken as a proxy for the education levels of newly arrived immigrants, the gradient in Panel B is inconsistent with an amenity-based interpretation in which attitudes toward high- and low-skilled migrants drive heterogeneity, as such a mechanism would predict the opposite pattern. The same gradient emerges in Panel C, where the grouping variable captures broader neighborhood education and not only the composition of the immigrant population, suggesting that the pattern reflects wider neighbor-

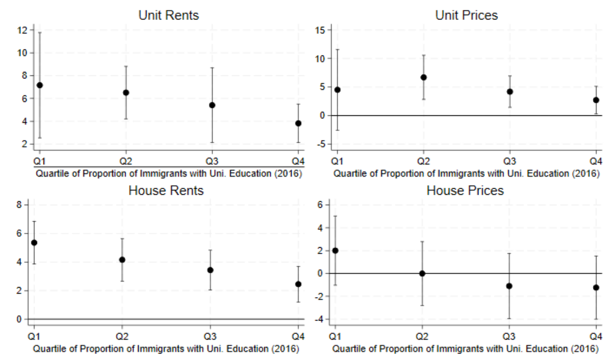
[26]We follow Borusyak et al. (2025), Online Appendices A.10 and B.1, in constructing exposure-robust standard errors in the heterogeneity analyses shown this section.

[27]We note that older neighborhoods tend to have substantially fewer property transactions, particularly for units. For example, the mean number of unit rental transactions over 2017-24 is approximately 7,000 in the youngest quartile but below 1,000 in the oldest. A similar pattern holds with respect to building approvals, where SA2s with fewer approvals also display fewer transactions. This lower transaction volume may contribute to the wider confidence intervals observed in certain subgroups.

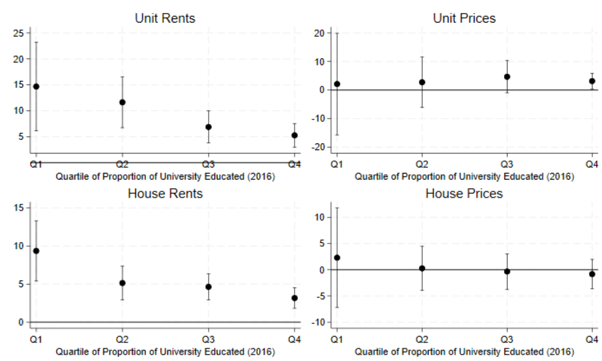
hood characteristics rather than immigrant skill mix alone. One candidate is that the education gradient partly reflects correlated neighborhood characteristics: median age correlates at around -0.2 with both the share of foreign-born residents with university education and the overall share of university-educated residents, and the university-educated share correlates at around -0.5 with a dummy for regional status, meaning less-educated SA2s are disproportionately regional. The urban/regional split in rent responses is explored shortly below in Figure 7.



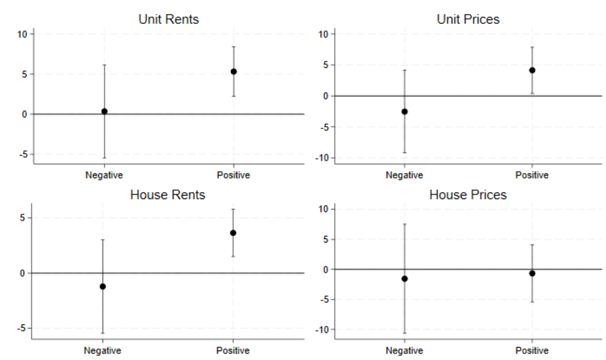
Panel A - Median Age



Panel B - Share of Existing Foreign-Born with University Education



Panel C - Share of Existing Population with University Education



Panel D - Positive vs. Negative Price/Rent Changes

Figure 6: Heterogeneity Analysis

Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Borusyak et al. (2025), Online Appendices A.10 and B.1). Panel A splits neighborhoods into quartiles of median age (fixed at 2016 values); Panel B splits neighborhoods into quartiles of the share of incumbent foreign-born individuals with university education (fixed at 2016 values); Panel C repeats Panel B using the share of the total SA2 population with university education; and Panel D separates price and rent responses into positive and negative net overseas migration flows. All models include SA2 and state-year fixed effects, and controls include log population, population density, median age, apartment share, and university education rates.

Finally, Panel D (bottom-right) presents the heterogeneous impacts of neighborhoods experiencing *positive* and *negative* net overseas migration flows. In particular, we consider the following regression specification:

$$\begin{aligned}\Delta \log y_{gst} = \alpha &+ \beta_+ \left(\frac{immigrants_{gs,t}}{population_{gs,t-1}}\right) \times \mathbb{I}\left[\left(\frac{immigrants_{gs,t}}{population_{gs,t-1}}\right) \geq 0)\right] \\ &+ \beta_- \left(\frac{immigrants_{gs,t}}{population_{gs,t-1}}\right) \times \mathbb{I}\left[\left(\frac{immigrants_{gs,t}}{population_{gs,t-1}}\right) < 0)\right] \\ &+ \phi \cdot \mathbb{I}\left[\left(\frac{immigrants_{gs,t}}{population_{gs,t-1}}\right) < 0\right] + \delta \mathbf{X}_{gs,t-1} + \gamma_{st} + \lambda_g + \varepsilon_{gst},\end{aligned}$$

where $\mathbb{I}(\cdot)$ is the indicator function. Note that because the sign-of-immigration indicator varies over time within neighborhoods, this specification constitutes an incomplete shares case under the shift-exogeneity framework, requiring a control for the sum of shares within SA2, which is achieved by the introduction of the first term of the last line.[28]

Since all negative changes occurred during the COVID-19 period, this exercise also serves as a robustness check for our estimates excluding that time.[29] In this sense, the results are robust: the point estimates for "positive immigration" remain largely unchanged.

In years of negative migration, the point estimates are close to zero and statistically insignificant across all outcomes. This stands in contrast to the significant and positive effects of immigration inflows, suggesting an asymmetry: while immigrant arrivals add further demand pressures, departures do not seem to reduce them by a comparable magnitude. This could reflect, for instance, a desire among residents or returning Australians to move into areas previously occupied by immigrants. We provide suggestive evidence in favor of this interpretation in Appendix Subsection E.2, where we show that Australian-born residents tended to move towards areas that were losing immigrants during Covid-19, while they tended to move towards the same areas that were gaining immigrants in other years of our sample.

Urban vs. Regional Areas. Figure 7 shows the results from modifying expression (1), instrumented and with controls, and interacting immigration flows with an indicator for the urban status of the SA2 in 2016. The specification is:

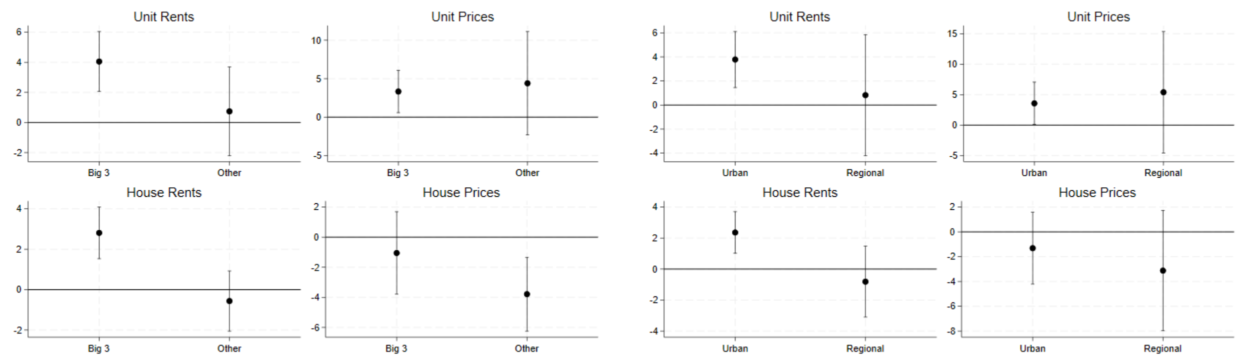
$$\begin{aligned}\Delta \log y_{gst} = \alpha &+ \beta_{reg} \left(\frac{immigrants_{gst}}{population_{gs,t-1}}\right) \times \mathbb{I}(g \in \text{regional}) \\ &+ \beta_{urb} \left(\frac{immigrants_{gst}}{population_{gs,t-1}}\right) \times \mathbb{I}(g \in \text{urban}) \\ &+ \delta \mathbf{X}_{gs,t-1} + \gamma_{st} + \lambda_g + \varepsilon_{gst},\end{aligned}$$

where $\mathbb{I}(\cdot)$ is the indicator function and urban and regional status are mutually exclusive.

[28]Following their guidance (Section 4.2), we include the negative immigration indicator as an additional control, which absorbs any level difference in housing outcomes between neighborhood-years with positive versus negative immigration flows. This control is required because the positive/negative classification varies over time within SA2s, creating an incomplete shares case (Borusyak et al., 2025).

[29]There are no negative changes outside the years 2020 and 2021 in our sample.

We consider two alternative definitions of urban versus regional status. Panel A distinguishes SA2s within the three largest metropolitan areas – Sydney, Melbourne, and Brisbane ("Big 3") – from all other locations, while Panel B contrasts SA2s located in urban areas of state capitals and the Australian Capital Territory (Canberra) with those classified as regional.



Panel A - Big 3 Urban Conglomerates vs. Others

Panel B - Urban vs. Regional Areas

Figure 7: Heterogeneity Analysis - Urban vs. Regional Areas

Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Adão et al., 2019; Borusyak et al., 2022). All models include SA2 state-year fixed effects and controls. Panel A separates neighborhoods that are part of the largest three urban conglomerates (Sydney, Melbourne, Brisbane) from the rest of the sample. Panel B compares SA2s located in state capitals and the ACT (Canberra) with all other SA2s.

Panel A reveals that the impact on rents is positive and significant only within the Big 3 cities.[30] Unit price estimates are positive in both groups but imprecisely estimated. Interestingly, the negative effect on house prices is concentrated entirely outside the Big 3, where it is significant at the 5% level. Together, these patterns are consistent with tighter housing supply within the Big 3 cities amplifying rent responses, and with less favorable attitudes toward immigration outside major urban centers driving the negative house price effect – over 2017-19, the share of regional Australians who considered immigration levels "too high" exceeded that of urban Australians by 11 to 18 percentage points (Lowy Institute, 2025). Panel B yields qualitatively similar but less precisely estimated patterns, particularly for prices.[31]

We also explored heterogeneous effects across other neighborhood characteristics, but found no clear patterns across them. The full set of results is presented in Appendix E.4.

Impacts of Overseas Migration on Internal Migration. Lastly, we examine whether immigration is associated with changes in the internal migration of incumbent residents. To do so, we estimate the specification in equation (1), using net internal migration flows as the dependent variable. The IV estimates are imprecise under our preferred inference procedure and we refrain

[30]In our sample, roughly 43% of SA2s belong to the Big 3 , and 36% are classified as urban.

[31]The education gradient in Panels B and C of Figure 6 is not straightforwardly reconciled with the urban/regional split documented here, since lower-education SA2s are disproportionately regional. This result likely reflects variation within urban areas rather than the urban/regional dimension itself.

from drawing strong causal conclusions.[32]

Table 6: Impact of Immigration on Internal Migration

	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)
Immigration Flows	-0.097***	-0.201***	-0.106	-0.162
	(0.025)	(0.066)	(0.119)	(0.182)
SA2 x Year Obs.	15,402	14,377	15,402	14,377
# Country-level Obs.	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	25.244	27.562
Controls	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is net internal migration inflows. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, and university-educated share. All specifications include SA2 and state-year fixed effects.

Comparing with existing studies, at an annual frequency Sá (2015) finds an analogous IV estimate of -0.868 (Table 7, Panel (a)), suggesting nearly one-for-one displacement of natives, while Braakmann (2019), who uses intercensal data, reports an outflow of 34 natives per 100 immigrants (page 512). Both estimates are statistically significant. Note, however, that we measure net internal migration of all residents, while they report the effect on net migration *of natives*. While our estimates are imprecise under exposure-robust inference, the confidence interval rules out displacement of the magnitude reported by Sá (2015) for the UK. Estimates of the magnitude reported by Braakmann (2019) are within our confidence interval, but so is zero. Our results therefore provide evidence against the near one-for-one native out-migration documented for the UK, while leaving open the possibility of more modest effects in the Australian context.[33]

Finally, we examine whether the impact of immigration on net internal migration varies across neighborhood characteristics, using specifications analogous to expression (3). Overall, we find limited evidence of heterogeneity when considering quartiles of the distributions. However, when splitting the sample into terciles, two patterns emerge: neighborhoods with lower population density and those with a lower share of university-educated residents exhibit positive net internal migration responses to immigration, while the response is indistinguishable from zero in the remaining terciles. These results are reported in Appendix E.5. Recall that earlier in this section we documented that rent responses to immigration are strongest in neighborhoods with a lower share of university-educated residents (Panels B and C of Figure 6). Taken together, these two findings suggest that in lower-education neighborhoods, demand pressure from international migration

[32] We also replicate a cumulative analysis analogous to Figure 4 for net internal migration, and find no statistically significant effects across horizons.

[33] In an earlier circulated version of this paper, we used SA2-level clustered standard errors, under which the point estimates are unchanged but become statistically significant. The cluster-robust standard errors without and with controls are respectively 0.079 and 0.046. Consequently, the estimate with controls would be significant at the 1% confidence interval.

is absorbed primarily through higher rents rather than through the displacement of incumbent residents, who if anything move into these neighborhoods on net. We note that the net internal migration estimates are noisy, and we view this interpretation as suggestive rather than conclusive. Other heterogeneity results for net internal migration are available upon request.

8 Conclusion

This paper examines the impact of net overseas migration on residential property prices and rents using annual, neighborhood-level data for Australia. We employ a shift-share instrumental variable strategy based on historical immigrant settlement patterns to isolate exogenous variation in migration flows. Following Borusyak et al. (2022), we adopt a shift exogeneity assumption: aggregate immigration inflows by country of origin are assumed to be orthogonal to unobserved neighborhood-level determinants of housing outcomes. Consequently, we report exposure-robust standard errors throughout, as suggested by Adão et al. (2019) and Borusyak et al. (2022). Balance tests confirm that country-level immigration shifts are uncorrelated with observable characteristics of destination neighborhoods, lending further credibility to the identifying assumption.

In our preferred specification, a one percentage point increase in net overseas migration as a share of the local population raises house rents by 2.8% and unit rents by 4.3%, and increases unit prices by 3.2%. The contemporaneous effect on house prices is negative but statistically insignificant, with a point estimate of -1.1%. However, cumulative estimates over one- and two-year horizons remain negative and turn statistically significant, consistent with findings in the existing literature (Stillman and Maré, 2008; Saiz and Wachter, 2011; Accetturo et al., 2014; Sá, 2015).

The effects of immigration on housing costs decline monotonically with the median age of the neighborhood: across quartiles of the 2016 median age distribution, point estimates are smaller in older areas for all four outcomes. This result is supportive of an amenity-based interpretation, whereby immigrants are perceived as a negative amenity particularly by older homeowners, consistent with survey evidence that attitudes toward immigration tend to be less favorable among older Australians (Lowy Institute, 2019, 2024; Roy Morgan, 2025). Turning to other dimensions of heterogeneity, rent effects are positive and significant only within Australia's three largest cities and in urban areas, while the negative house price effect is concentrated outside major urban centers. Finally, we find suggestive evidence that immigration is associated with net outflows of incumbent residents, though the IV estimates are imprecise and on the low end of prior findings.

While overseas immigration has well-documented positive effects on economic growth, innovation, and long-term public finances (Hernandez, 2024), public concern often focuses on its impact on housing affordability. Our results suggest that rising housing costs, especially rents, can indeed follow increases in migration. To the extent that supply constraints amplify these effects, expanding residential construction may help mitigate price and rent pressures, preserving the broader social and economic benefits of migration.

Disclosure Statement. *The authors report there are no competing interests to declare.*

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Online Appendixes

A Motivation - Additional Figures

Figure A.1 documents the recent evolution of immigration flows at the national and state level.

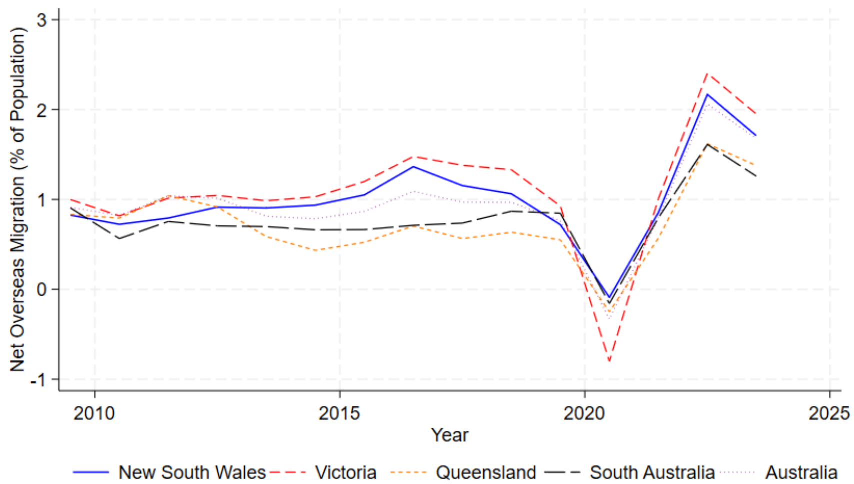


Figure A.1: Net Overseas Immigration in Australia and Selected States

Notes: The plot represents changes in yearly net overseas migration, relative to the total population, of Australia and selected Australian states. Source: Australian Bureau of Statistics (ABS) regional population components.

B Validation

Table B.1 reports descriptive statistics of the country-year immigration shifts, both in raw form and after residualizing on year fixed effects. The latter removes common time trends in immigration inflows, isolating the cross-country variation that identifies the instrument. Notably, the R^2 of the residualization regression is only 0.016, and, consequently, the standard deviation falls only marginally, from 5,339 to 5,297, suggesting that most of the variation in the shifts is cross-country rather than time-series. This is reassuring, as it implies that the instrument is not primarily driven by aggregate immigration waves that affect all origin countries simultaneously, but rather by differential shocks across countries of origin. The effective sample size of 207 is unchanged across both rows, as it depends only on the base-year settlement shares.

B.1 Additional Tests - Balance of Covariates in Levels

As a complement to the balance tests on changes in destination characteristics reported in Section 5, we examine whether immigration shifts and the shift-share instrument correlate with the *levels*

Table B.1: Descriptive Statistics of Shifts

Variable	Average	Median	Standard Deviation	p5	p25	p75	p95	Eff. Sample Size
Shifts (nom_{ct})	912.2	10.0	5339.2	-100.0	0.0	180.0	4650.0	207
Shifts, residualized on year FE	0.0	-817.8	5296.7	-2140.9	-969.6	111.3	3540.4	207

Notes: Descriptive statistics of the country-year immigration shifts nom_{ct}, raw and after residualizing on year fixed effects. The effective sample size is computed as 1/HHI of the country weights $w_c \equiv \sum_{gs} s_{cgs}$, following Borusyak et al. (2025).

of those same covariates. Table B.2 reports the results.

Panel A shows that NOM is not systematically correlated with baseline levels of destination characteristics at either census date. This is encouraging (beyond the results presented in Section 5): it suggests that origin countries with larger inflows do not disproportionately send migrants to SA2s with particular demographic or urban profiles.

Table B.2: Balance Tests – Levels of Destination Characteristics

	Apt. ratio	Education share	$\log$ Pop.	Median age	Pop. density
Panel A: Shift-level					
NOM (2016)	-0.082	0.141	1.833	-10.462	4.04e+06
	(0.664)	(0.320)	(1.196)	(12.307)	(8.55e+06)
NOM (2021)	-0.004	0.156	1.830	-6.547	4.53e+06
	(0.747)	(0.376)	(1.125)	(10.829)	(9.05e+06)
Panel B: Instrument-level					
	1.505***	-0.006	7.155***	-24.074	2.43e+07***
	(0.404)	(0.508)	(2.208)	(22.265)	(5.44e+06)
	1.617***	0.119	7.034***	-19.273	2.57e+07***
	(0.453)	(0.598)	(2.249)	(25.523)	(5.79e+06)
Obs.	206	206	206	206	206

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SA2-level covariates are aggregated to the country level across SA2s using exposure shares as weights, which yields exposure-robust standard errors (Borusyak et al., 2022). Row labels indicate the census year at which covariates are measured. In Panel A, covariates are regressed on average NOM (in millions) over 2017-23. In Panel B, covariates are regressed on the share-weighted shift-share instrument (re-scaled by a factor of 1000, for readability), aggregated to the country level using exposure shares.

Panel B reveals statistically significant correlations between the share-weighted instrument and baseline levels of covariates. This is not unexpected, as the instrument aggregates NOM using exposure shares that reflect historical sorting into denser and more urbanized areas. Reassuringly, these correlations do not extend to changes (Table 2, Panel B), indicating that the instrument does not predict differential *trends* in destination characteristics. Moreover, these level correlations are absorbed by the SA2 fixed effects included in all specifications.

C Robustness and Additional Results

Table C.1 below reproduces Table 4 with the outcomes representing their *averages* within a neighborhood.

Table C.1: Impact of Immigration on *Average* Rents and Prices

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.892***	1.954***	3.843***	3.942***	0.166	0.183	0.689	0.654
	(0.134)	(0.148)	(0.480)	(0.708)	(0.237)	(0.258)	(1.094)	(1.669)
SA2 x Year Obs.	14,041	13,058	14,041	13,058	12,861	11,887	12,861	11,887
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	24.346	26.756	-	-	23.622	25.921
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.408***	1.430***	2.957***	3.305***	-0.635*	-0.767***	-1.253	-1.382
	(0.135)	(0.153)	(0.438)	(0.660)	(0.348)	(0.268)	(0.918)	(1.317)
SA2 x Year Obs.	15,023	14,031	15,023	14,031	15,050	14,060	15,050	14,060
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	24.996	27.820	-	-	24.997	27.768
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their *averages* at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects.

Table C.2 below reproduces Table 4, but instead selecting 2006 as the base year for constructing the instrumental variable.

Table C.3 below reproduces Table 4, this time including one lag for the outcome and one lag the immigration flows variable.

Table C.2: Impact of Immigration on Rents and Prices - IV Based on 2006

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	3.518***	3.788***	0.755***	1.007***	2.422***	2.936**
	(0.157)	(0.177)	(0.675)	(0.973)	(0.212)	(0.282)	(0.795)	(1.232)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	24.925	26.831	-	-	23.894	25.596
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.409***	2.606***	-0.300	-0.494**	-0.450	-0.671
	(0.141)	(0.145)	(0.427)	(0.639)	(0.184)	(0.218)	(1.034)	(1.395)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	26.560	29.125	-	-	26.143	29.289
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects.

Table C.3: Impact of Immigration on Rents and Prices - Controls for Lags

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.916***	1.757***	4.770***	5.298***	0.595***	0.778***	1.812**	2.207*
	(0.177)	(0.210)	(0.557)	(0.906)	(0.204)	(0.263)	(0.756)	(1.163)
SA2 x Year Obs.	9,040	8,248	9,040	8,248	7,218	6,482	7,218	6,482
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	21.053	23.786	-	-	19.362	21.651
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.424***	1.285***	2.788***	3.023***	-1.218***	-1.480***	-2.227**	-2.771***
	(0.169)	(0.160)	(0.360)	(0.571)	(0.193)	(0.236)	(0.868)	(1.071)
SA2 x Year Obs.	12,443	11,600	12,443	11,600	12,495	11,665	12,495	11,665
# Country-level Obs.	-	-	2,880	2,880	-	-	2,880	2,880
Kleibergen-Paap F-stat.	-	-	23.181	27.144	-	-	23.253	27.430
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects.

D Rotemberg Decomposition and Instrument Sensitivity

To assess whether our estimates are driven by a small number of dominant countries, we follow Goldsmith-Pinkham et al. (2020) and decompose the IV estimator using Rotemberg weights, which characterize the contribution of each origin country's share to the overall IV estimate. Table D.1 reports the top 30 countries ranked by absolute Rotemberg weight, alongside their first-stage coefficients and standard errors. China and India account for the largest shares of the decomposition, with weights of 0.27 and 0.14 respectively, while the remaining weight is distributed across a broad set of origin countries. First-stage coefficients for all top-weighted countries are positive. Among all origin countries in the decomposition, only 18 display negative first-stage coefficients, with an average Rotemberg weight of -0.0003, indicating that they contribute negligibly to the overall estimate.

Table D.1: Top 30 Rotemberg Weights

Country	weight	first-stage coef. (x100)	s.e. (x100)
China (excludes SARs and Taiwan)	0.2704	0.0064	0.0006
India	0.1355	0.0086	0.0007
Nepal	0.0501	0.0114	0.0019
Colombia	0.0440	0.0425	0.0036
Taiwan	0.0399	0.0188	0.0058
Vietnam	0.0342	0.0116	0.0018
Indonesia	0.0293	0.0988	0.0090
Philippines	0.0276	0.0063	0.0014
Brazil	0.0270	0.0829	0.0077
Korea, Republic of (South)	0.0269	0.0380	0.0052
Mongolia	0.0226	0.0206	0.0051
Malaysia	0.0220	0.0346	0.0038
Pakistan	0.0220	0.0256	0.0039
Sri Lanka	0.0202	0.0380	0.0044
Thailand	0.0191	0.0711	0.0078
Japan	0.0134	0.1015	0.0103
Bhutan	0.0131	0.0007	0.0003
Hong Kong (SAR of China)	0.0116	0.0372	0.0037
Bangladesh	0.0089	0.0396	0.0123
Ireland	0.0081	0.1142	0.0088
South Africa	0.0081	0.1191	0.0123
New Zealand	0.0081	0.0505	0.0054
Iran	0.0079	0.1217	0.0116
France	0.0079	0.2471	0.0162
Argentina	0.0065	0.0659	0.0107
Saudi Arabia	0.0060	0.1521	0.0154
Afghanistan	0.0057	0.0134	0.0024
Fiji	0.0054	0.0282	0.0045
Syria	0.0051	0.0126	0.0021
Kenya	0.0049	0.2296	0.0184

Notes: Rotemberg weights and country-specific first-stage coefficients are computed following Goldsmith-Pinkham et al. (2020). The first-stage coefficient for country o is obtained by regressing SA2-level immigration inflows on the shift-share instrument constructed using only the settlement shares of country o. Coefficients and standard errors are multiplied by 100 for readability. Standard errors are clustered at the SA2 level.

Figure D.1 plots the country-specific IV estimates against their first-stage F-statistics, with marker size proportional to the absolute Rotemberg weight. Countries that contribute most to the instrument – those with large circles – cluster tightly around the pooled IV point estimate. Importantly, there is no systematic relationship between estimates and the first-stage F-statistic:

countries with the strongest first stages do not yield systematically different causal estimates, indicating that the pooled result is not driven by a small number of highly relevant but potentially unrepresentative origin countries.

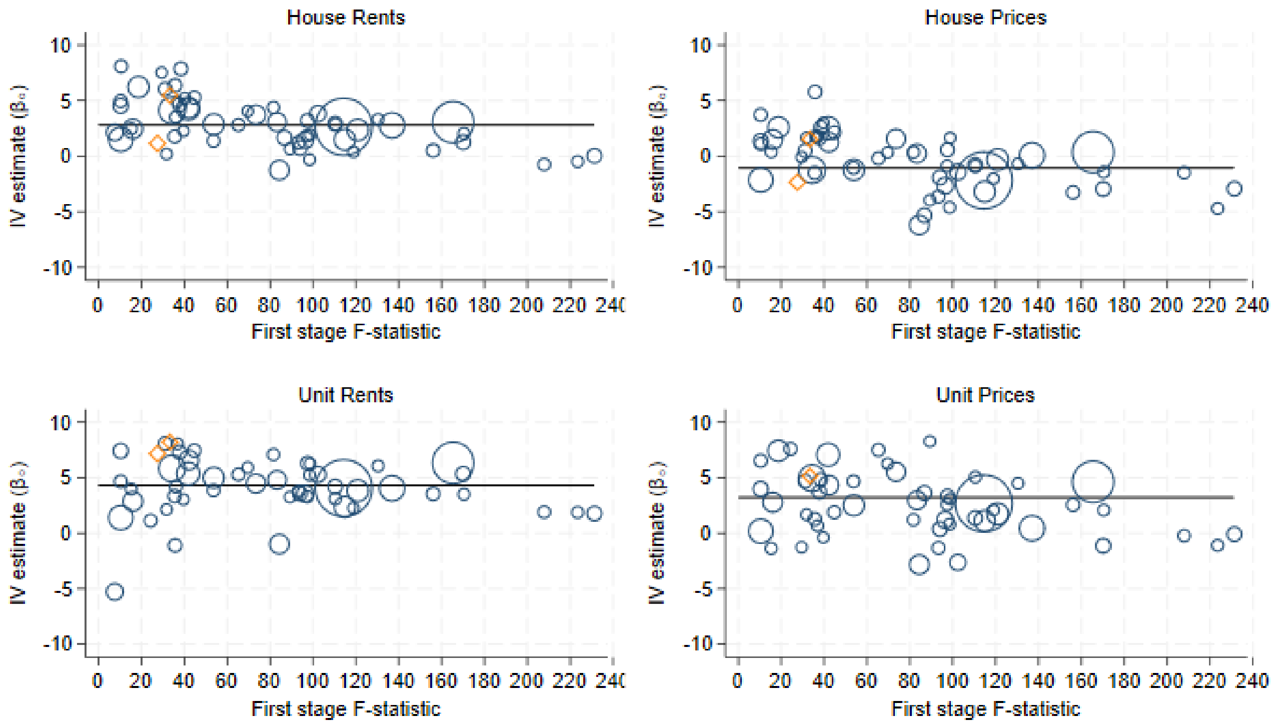


Figure D.1: Country-Specific IV Estimates and First-Stage F-Statistics by Origin Country

Notes: Each marker represents an origin country o, with size proportional to the absolute Rotemberg weight of that country. Yellow markers represent negative Weights. The y-axis reports the (just-identified) IV estimate obtained using only the settlement shares of country o, setting the others to zero. F-statistics are computed using SA2-level regressions. Countries with first-stage F-statistics below 5 are excluded. The horizontal line denotes the pooled IV estimate. Observations with $|\hat{\beta}_o| > 10$ are excluded from the plot; the largest Rotemberg weight among excluded observations is 0.0043 across all graphs. Full graph is available upon request.

We further assess sensitivity by re-estimating the baseline specification after excluding the two and five largest Rotemberg-weighted countries from the instrument construction. Appendix Table D.2 reports results excluding China and India (joint Rotemberg weight roughly 40%), and Appendix Table D.3 reports results excluding the top five countries (China, India, Nepal, Colombia, and Taiwan), with a joint weight of roughly 54%. .

Across all housing outcomes, point estimates are essentially unchanged when China and India are excluded from the instrument construction: the IV estimate for unit rents moves from 4.316 (s.e. 1.167) in the baseline to 4.035 (s.e. 1.125), house rents from 2.819 (s.e. 0.668) to 2.839 (s.e. 0.486), unit prices from 3.217 (s.e. 1.446) to 3.226 (s.e. 1.616), and house prices remain negative and insignificant. Again, these results corroborate the insights presented from Figure D.1 above.

Table D.2: Impact of Immigration on Rents and Prices - Excluding China and India

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	3.914***	4.035***	0.755***	1.007***	2.731***	3.226**
	(0.157)	(0.177)	(0.517)	(1.125)	(0.212)	(0.282)	(0.721)	(1.616)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	-	2,852	-	-	-	2,852
Kleibergen-Paap F-stat.	-	-	163.726	28.787	-	-	133.629	25.366
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.598***	2.839***	-0.300	-0.494**	-0.655	-0.852
	(0.141)	(0.145)	(0.230)	(0.486)	(0.184)	(0.218)	(0.400)	(0.963)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
# Country-level Obs.	-	-	-	2,852	-	-	-	2,852
Kleibergen-Paap F-stat.	-	-	226.805	33.220	-	-	255.752	33.512
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects. China and India are excluded from the IV construction.

Excluding the top five Rotemberg-weighted countries yields similarly stable results. Unit rents move to 4.107 (s.e. 1.367), house rents to 2.820 (s.e. 0.561), and unit prices to 3.691 (s.e. 1.997). The latter remains statistically significant, but only at the 10% level. Once again, this is consistent with the results presented in Figure D.1.

Finally, excluding the United Kingdom and New Zealand – countries for which the "immigrant enclave" logic might not apply – leaves the results virtually identical to the baseline (Appendix Table D.4): unit rents 4.271 (s.e. 1.137), house rents 2.869 (s.e. 0.667), unit prices 3.248 (s.e. 1.398), and house prices -0.962 (s.e. 1.428), with significance levels unchanged throughout. Taken together, these exercises indicate that the baseline estimates are not driven by any particular origin country or group of countries.

Taken together, the results in this section point to a high degree of robustness of the baseline estimates to the choice of identifying variation, and a low dependence of estimates on the highest Rotemberg weight countries. Notably, excluding origin countries that account for up to 54% of the total Rotemberg weight leaves point estimates and significance levels essentially unchanged across all four housing outcomes. The patterns suggest that the baseline IV estimate captures a common effect of immigration that is not specific to migrants from any particular origin.[34]

[34]Under the alternative identifying assumption of exogenous shares (Goldsmith-Pinkham et al., 2020), the stability of estimates across alternative subsets of origin-country shares, especially those with relatively high first stage F-stat, has a formal interpretation: it is consistent with passing an overidentification test. When shares are exogenous, *each*

Table D.3: Impact of Immigration on Rents and Prices - Excluding Top 5 Rotemberg Weights

	Panel A: Unit Rents				Panel B: Unit Prices			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	3.965***	4.107***	0.755***	1.007***	3.039**	3.691*
	(0.157)	(0.177)	(0.891)	(1.367)	(0.212)	(0.282)	(1.256)	(1.997)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	2,810	2,810	-	-	2,810	2,810
Kleibergen-Paap F-stat.	-	-	23.462	24.994	-	-	20.621	21.626
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.570***	2.820***	-0.300	-0.494**	-0.622	-0.783
	(0.141)	(0.145)	(0.418)	(0.561)	(0.184)	(0.218)	(0.804)	(1.055)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
# Country-level Obs.	-	-	2,810	2,810	-	-	2,810	2,810
Kleibergen-Paap F-stat.	-	-	26.723	28.495	-	-	26.849	28.756
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, mdian age, and university-educated share. All specifications include SA2 and state-year fixed effects. China, India, Nepal, Colombia, and Taiwan are excluded from the IV construction.

D.1 Main Regression Table with Clustered Standard Errors

Table D.5 below repeats Table 4 using SA2-level clustered standard errors. Standard errors of this type are the norm in the existing literature (e.g. Sanchis-Guarner (2023), Unal et al. (2024), Helfer et al. (2023), among others).

Among the studies cited in our paper, ours is the only one to employ exposure-robust standard errors, which account for the correlation in residuals across units with similar exposure shares, as initially pointed out by Adão et al. (2019).

Comparing Tables 4 and D.5, the use of exposure-robust standard errors substantially widens confidence intervals. This does not affect the conclusion that immigration raises rents and unit prices, but it does affect inference on house prices. The IV estimate, which is statistically significant under conventional clustering, becomes indistinguishable from zero under exposure-robust inference.

In this work, as the reader will have noted, we prefer to err on the side of caution, and thus refrain from putting excessive weight on the negative point estimates of immigration inflows on house prices. Our preferred identification strategy relies on shift exogeneity rather than share ex-

origin country's share constitutes a valid instrument in its own right, and the pooled shift-share estimator represents one particular linear combination of these. The near-invariance of estimates to which countries are included in the construction of the instrument therefore provides indirect support for the share exogeneity assumption, in addition to the shift exogeneity argument pursued in the main text.

Table D.4: Impact of Immigration on Rents and Prices - Excluding UK and New Zealand

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	4.075***	4.271***	0.755***	1.007***	2.823***	3.248**
	(0.157)	(0.177)	(0.790)	(1.137)	(0.212)	(0.282)	(0.912)	(1.398)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	2,852	2,852	-	-	2,852	2,852
Kleibergen-Paap F-stat.	-	-	21.426	23.382	-	-	20.140	21.837
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.654***	2.869***	-0.300	-0.494**	-0.771	-0.962
	(0.141)	(0.145)	(0.450)	(0.667)	(0.184)	(0.218)	(1.064)	(1.428)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
# Country-level Obs.	-	-	2,852	2,852	-	-	2,852	2,852
Kleibergen-Paap F-stat.	-	-	23.425	25.994	-	-	23.479	26.194
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects. The IV is constructed excluding immigrants from New Zealand, the UK, the Isle of Man, Northern Ireland, Scotland, Wales, Guernsey, Jersey, and Ireland.

ogeneity, as discussed Section 5 and in this section. The corresponding exposure-robust standard errors are substantially larger than conventional clustered standard errors, and it is under this inference procedure that the house price estimate loses significance.

Note, however, that point estimates of the impact of immigration on house prices are negative across nearly all specifications (see, e.g., Table 5), and become statistically significant at further horizons in cumulative regressions (Figure 4). While we refrain from drawing strong conclusions given the imprecision of our baseline estimates, the consistency of the sign across specifications is suggestive of a genuine negative effect.

Table D.5: Impact of Immigration on Rents and Prices

	Panel A: Unit Rents				Panel B: Unit Prices			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.732***	1.641***	4.101***	4.316***	0.755***	1.007***	2.809***	3.217***
	(0.157)	(0.177)	(0.468)	(0.614)	(0.212)	(0.282)	(0.661)	(0.934)
SA2 x Year Obs.	10,816	9,885	10,816	9,885	8,932	8,049	8,932	8,049
# Country-level Obs.	-	-	-	-	-	-	-	-
Kleibergen-Paap F-stat.	-	-	209.999	234.820	-	-	175.478	203.412
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.326***	1.243***	2.617***	2.819***	-0.300	-0.494**	-0.838**	-1.053**
	(0.141)	(0.145)	(0.219)	(0.263)	(0.184)	(0.218)	(0.352)	(0.438)
SA2 x Year Obs.	14,576	13,591	14,576	13,591	14,672	13,697	14,672	13,697
Kleibergen-Paap F-stat.	-	-	279.231	334.169	-	-	315.114	343.225
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: SA2-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, and university-educated share. All specifications include SA2 and state-year fixed effects.

E Additional Results

Table E.1 shows the first-stage coefficients and Kleibergen-Paap F-statistic of the underlying specifications in Figure 4, based on the country-level regressions. Instrument strength is high at short horizons but declines at longer horizons, which is not surprising, as identifying the cumulative effect at $h = 3$ places considerably greater demands on the data, both because the instrument must predict four years of accumulated flows simultaneously and because the sample shrinks as additional lags are consumed.

E.1 Results Excluding Covid Years

Table E.2 shows results analogous to those in Table 4, but excluding Covid years (2020-2021). Point estimates are similar to those obtained in Table 4, but the impact on unit prices and rents (with controls) become insignificant as a result of large standard errors.

E.2 Do Australians Move to Areas Previously Occupied by Immigrants?

To evaluate the possibility that Australians move to areas previously occupied by immigrants during the Covid-19 period, we provide suggestive evidence using census data on the Australian-born population at the SA2 level. Specifically, we correlate the change in the Australian-born population between 2016 and 2021, constructed from the ABS Census, with net overseas migration

Table E.1: First Stage: Cumulative Regressions

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 0$	$h = 1$	$h = 2$	$h = 3$
Immigration Flows	.0294***	.0159***	.0055***	.0022*	.0239***	.0131***	.0044***	.0018*
	(.0059)	(.0036)	(.0019)	(.0013)	(.005)	(.0031)	(.0015)	(.001)
Observations	2,880	2,674	2,468	2,262	2,880	2,674	2,468	2,262
Kleibergen-Paap F-stat.	24.42	19.27	8.94	3.11	22.44	18.2	8.78	3.18
	Panel C: House Rents				**Panel D: House Prices**			
	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 0$	$h = 1$	$h = 2$	$h = 3$
Immigration Flows	.0399***	.0213***	.007***	.003*	.0406***	.0216***	.0071***	.003*
	(.0076)	(.0048)	(.0024)	(.0017)	(.0077)	(.0048)	(.0024)	(.0018)
Observations	2,880	2,674	2,468	2,262	2,880	2,674	2,468	2,262
Kleibergen-Paap F-stat.	27.48	19.98	8.95	2.96	27.73	20.03	8.85	2.89

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Statistics are obtained via a country-level regression, as suggested in Borusyak et al. (2022). h stands for horizon (x-axis of Figure 4). Coefficients and standard errors are re-scaled by 10^6 for readability.

(NOM) measured in 2019 (pre-Covid) and 2021 (Covid).

Note that this exercise displays a few caveats. First, it is purely descriptive and cannot be given a causal interpretation. Second, and more importantly, the change in Australian-born population spans the full 2016-21 intercensal period, while NOM is measured at a single year (2019 or 2021).[35] The 2016-21 change therefore conflates pre- and during-Covid movements, and the correlation with 2021 NOM can only be interpreted as suggestive of a within-period reallocation rather than a precise estimate of the Covid-era effect.

Figure E.1 presents binned scatter plots for levels and per-capita measures for different years. The pattern is striking. In 2021 (right panels), when Australia's international borders were closed and NOM turned negative for most SA2s, the slope between NOM and the change in Australian-born population between 2016 and 2021 is negative. In other words, SA2s that lost most immigrants in 2021 are those whose number of Australians grew by most. What is more, if we correlate the growth of the Australian-born residents with net immigration flows in other years, we find a positive relationship. In the left panels of Figure E.1 we show the scatterplot for 2019, but the pattern is repeated for other years, including after 2021.[36] These results suggest that Australian-born residents tended to move – over the 2016-21 period – to areas with the highest international out-migration during Covid, whereas in other years they tended to move to neighborhoods with a high inflow of international migrants.

[35]We do not have information on the net flows of Australians at the SA2-year level.

[36]Results are readily available upon request. We select just one alternative year for brevity.

Table E.2: Impact of Immigration on Rents and Prices - Excluding 2020 and 2021

	Panel A: Unit Rents				**Panel B: Unit Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.926***	1.764***	4.207**	4.126	1.150***	1.835***	1.987	2.597
	(0.275)	(0.336)	(1.713)	(2.666)	(0.443)	(0.598)	(1.812)	(2.871)
SA2 x Country Obs.	7,734	7,070	7,734	7,070	6,540	5,905	6,540	5,905
# Country-level Obs.	-	-	2,468	2,468	-	-	2,468	2,468
Kleibergen-Paap F-stat.	-	-	8.160	8.613	-	-	7.805	8.262
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel C: House Rents				**Panel D: House Prices**			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Immigration Flows	1.149***	1.172***	2.859***	3.394**	0.434	0.507	0.469	0.769
	(0.193)	(0.229)	(0.982)	(1.540)	(0.414)	(0.431)	(2.214)	(3.189)
SA2 x Country Obs.	10,409	9,708	10,409	9,708	10,518	9,820	10,518	9,820
# Country-level Obs.	-	-	2,468	2,468	-	-	2,468	2,468
Kleibergen-Paap F-stat.	-	-	8.431	8.890	-	-	8.048	8.831
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Exposure-robust standard errors in parentheses (Adão et al., 2019; Borusyak et al., 2022). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes correspond to their median at the SA2 level. Immigration flows are measured relative to the local SA2 population in the prior year. Control variables include log population, population density, apartment ratio, median age, and university-educated share. All specifications include SA2 and state-year fixed effects.

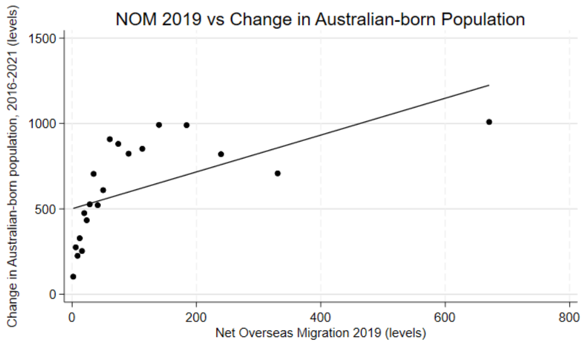
E.3 Dynamic Effects of Immigration on Housing Costs

To complement the dynamic cumulative effects shown in Figure 4, we examine the impact of migration flows at time t in prices and rents at future years $(t + h)$. Formally, for each horizon h, we estimate the following equation, analogous to expression (1):

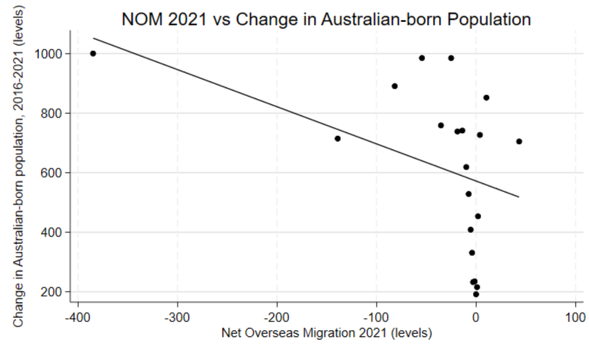
$$\Delta \log y_{gs,t+h} = \alpha + \beta \left(\frac{immigrants_{gs,t}}{population_{gs,t-1}} \right) + \delta \mathbf{X}_{gs,t-1} + \gamma_{st} + \lambda_g + \varepsilon_{gs,t+h} \tag{4}$$

where $\Delta y_{i,t+h} \equiv y_{i,t+h} - y_{i,t+h-1}$ denotes the outcome h periods ahead. Note that this specification differs from that in Subsection 6.1, as in that case both the outcome and the immigration variables were cumulated over time. The parameter β^h traces out the dynamic (future) response to current immigration. As in the baseline, migration flows are instrumented and we report exposure-robust standard errors obtained via the equivalent country-level regression.

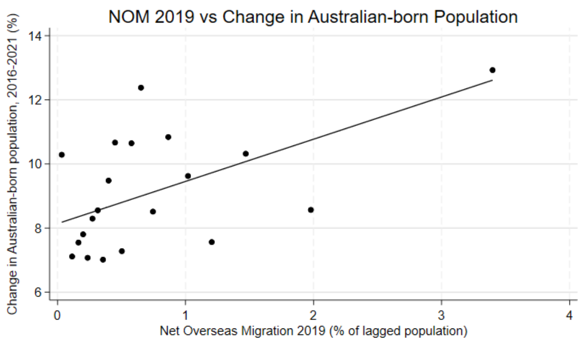
Figure E.2 presents the estimates of β^h for $h = 0, 1, 2, 3$, with 95% confidence intervals. Note that coefficients and standard errors at horizon $h = 0$ coincide with those reported in Table 4 (with controls and state-time fixed effects). For house prices, estimates are noisy at all horizons and never statistically distinguishable from zero. For unit prices, the positive contemporaneous effect persists into $h = 1$ before fading toward zero at $h = 2$ and $h = 3$, consistent with a transitory demand shock that takes slightly longer to dissipate in the apartment market.



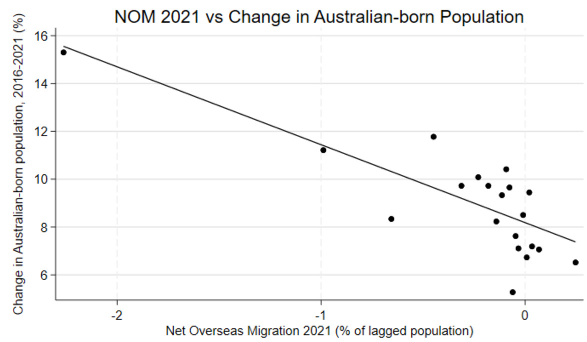
Panel A: Levels, 2019 (pre-Covid)



Panel B: Levels, 2021 (Covid)



Panel C: Per capita, 2019 (pre-Covid)



Panel D: Per capita, 2021 (Covid)

Figure E.1: Australians and Immigrants: Location (Choices) During Covid-19

Notes: Each panel shows a binned scatter plot of the change in Australian-born population at the SA2 level (2016-21) against net overseas migration in the indicated year. Panels A and B show levels; Panels C and D show per-capita measures, with NOM expressed as a share of lagged population and the change in Australian-born population expressed as a percentage. Variables are winsorized at the 0.5th and 99.5th percentiles.

Turning to rents, the pattern is similar across dwelling types: a positive contemporaneous effect that turns negative and statistically significant at $h = 2$, before reverting toward zero at $h = 3$. The effect on time $h = 2$ could reflect a medium-run supply response or geographic resorting, with immigrants relocating to other neighborhoods within a few years of arrival, reducing local housing demand.

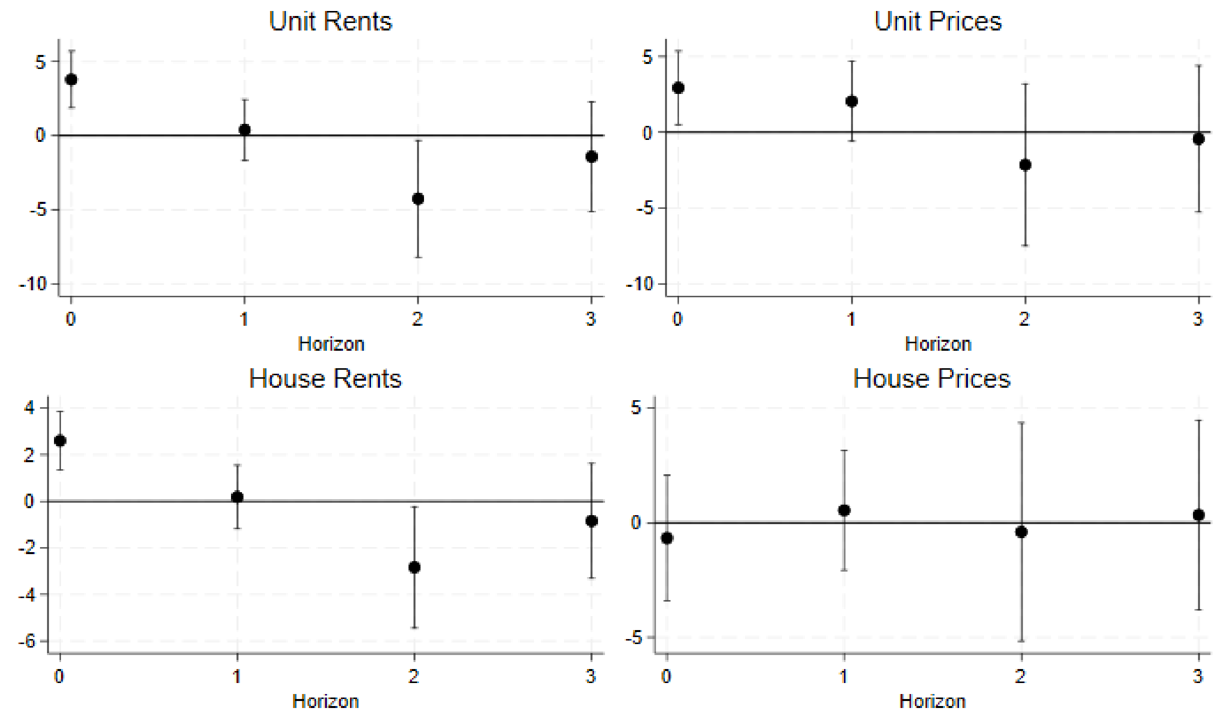


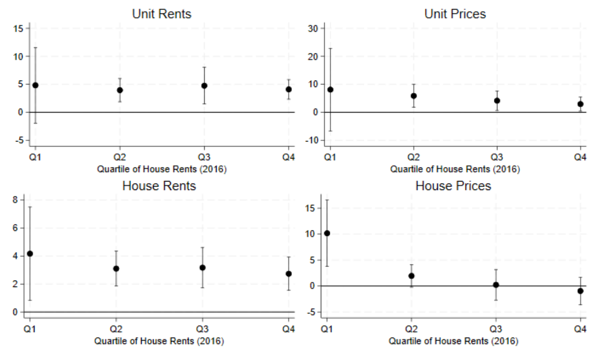
Figure E.2: Dynamic Effects of Immigration

Notes: Each panel plots estimates of β^h from equation (4), with 95% confidence intervals. All specifications include controls (log population, population density, apartment ratio, median age, and university educated share) and SA2 and state-year fixed effects. Standard errors are exposure-robust following Adão et al. (2019) and Borusyak et al. (2022), obtained via the equivalent country-level regression.

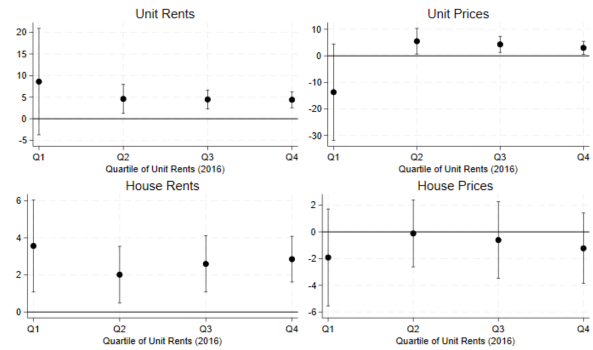
E.4 Heterogeneity - Additional Results

Figure E.3 presents additional heterogeneity results, splitting neighborhoods by quartiles of 2016 house rents, unit rents, house prices, and unit prices.

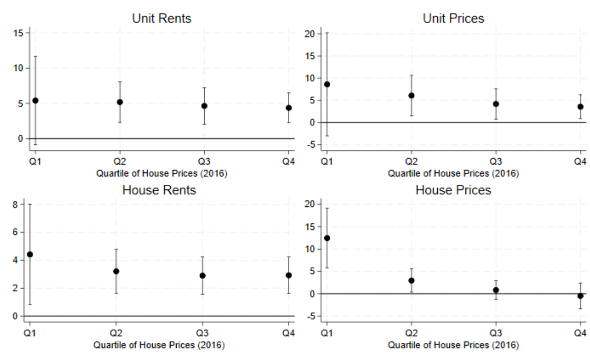
Figure E.4 presents additional heterogeneity results, splitting neighborhoods by quartiles of 2016 population, building approvals per area, apartment ratio, and population density.



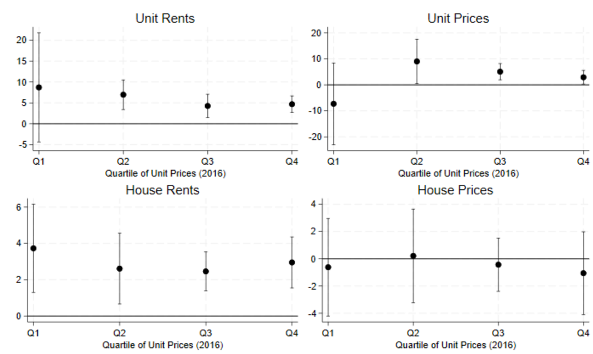
(a) By Quartile of House Rents (2016)



(b) By Quartile of Unit Rents (2016)



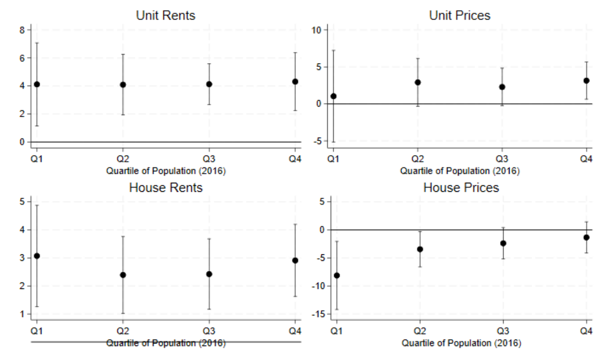
(c) By Quartile of House Prices (2016)



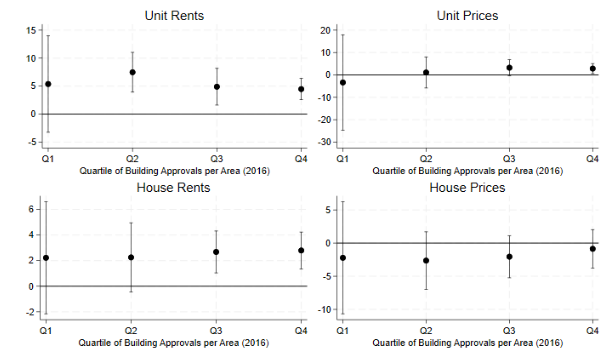
(d) By Quartile of Unit Prices (2016)

Figure E.3: Heterogeneous Effects by Quartiles of Initial Housing Costs

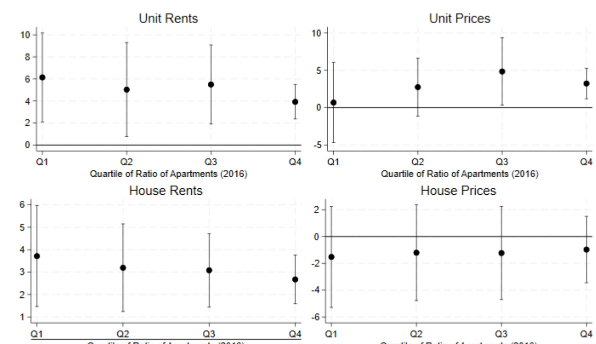
Notes: Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Borusyak et al. (2025), Online Appendices A.10 and B.1). Quartiles are defined using 2016 values and held fixed over time. The four outcomes in each panel are unit rents, unit prices, house rents, and house prices. All specifications include controls (log population, population density, apartment ratio, median age, and university educated share) and SA2 and state-year fixed effects. Q1 denotes the lowest quartile and Q4 the highest.



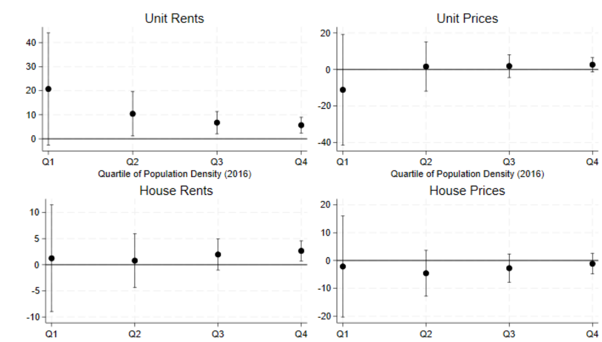
(a) By Quartile of Population (2016)



(b) By Quartile of Building Approvals per Area (2016)



(c) By Quartile of Apartment Ratio (2016)



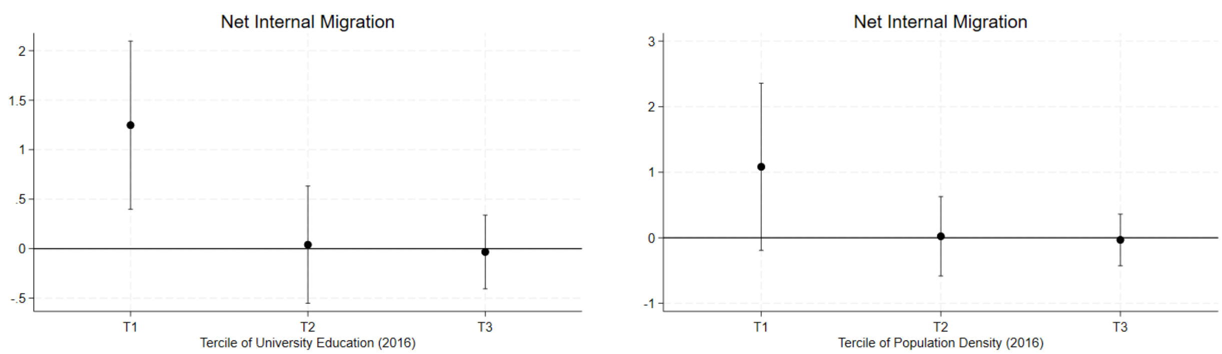
(d) By Quartile of Population Density (2016)

Figure E.4: Heterogeneous Effects by Neighborhood Characteristics

Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Borusyak et al. (2025), Online Appendices A.10 and B.1). Quartiles are defined using 2016 values and held fixed over time. The four outcomes in each panel are unit rents, unit prices, house rents, and house prices. All specifications include controls (log population, population density, apartment ratio, median age, and university educated share) and SA2 and state-year fixed effects. Q1 denotes the lowest quartile and Q4 the highest.

E.5 Heterogeneity - Net Internal Migration

Figure E.5 presents the heterogeneous effects of immigration on net internal migration across terciles of the share of university-educated residents (Panel A) and population density (Panel B), both measured in 2016. In the least-educated tercile, the estimated effect is positive and statistically significant, indicating that domestic residents move *toward* neighborhoods receiving immigrants. The effect is indistinguishable from zero in the remaining terciles. A similar pattern is observed for population density. One possible interpretation is that immigration into less-educated, lower-density areas stimulates local economic activity, attracting domestic residents in turn. Other heterogeneity dimensions were explored but yielded no meaningful patterns and are available upon request.



Panel A - Tercile of University Education (2016) Panel B - Tercile of Population Density (2016)

Figure E.5: Heterogeneous Effects of Immigration on Net Internal Migration

Notes: Bands represent 95% confidence intervals based on exposure-robust standard errors (Adão et al., 2019; Borusyak et al., 2022). All models include SA2 and state-year fixed effects, and controls include log population, population density, median age, apartment share, and university education rates.

F A Spatial Equilibrium Model of Immigration, Rents, and Prices

We develop a stylized spatial equilibrium model to formalize how immigration affects rents and prices within and across neighborhoods. We deliberately keep the model simple in order to obtain analytical characterizations of the key predictions. The model features two regions, immigrants and natives, and a choice between renting and buying. A key ingredient is that, unlike Accetturo et al. (2014), homeowners and renters display differential sensitivity to amenity changes brought about by immigration.

F.1 Single Dwelling Type

Setup. Consider a city with two neighborhoods, $d \in \{A, B\}$, that are initially symmetric. There is a single type of dwelling. The total housing stock in each area is fixed at $\bar{H}$, justified by the annual frequency of our data: construction decisions have already been taken and cannot respond to contemporaneous immigration shocks. There is a mass $n_A + n_B = N$ of natives in total. A mass m of immigrants arrives in neighborhood B and is assumed to live there. Immigrants can only rent, while natives may rent or own. This assumption is motivated by the fact that immigrants largely tend to be renters upon arrival (see Section 2).

Amenities. Assume a single amenity function $A(m_d)$, $A'(m) \neq 0$, scaled by a factor θ for owners. The parameter θ is a reduced-form representation of mechanisms we do not microfound in this simple model. First, $\theta > 1$ may reflect the fact that homeowners have different preferences over neighborhood composition than renters, including attitudes toward the presence of immigrants. Second, owners hold an asset whose value capitalizes neighborhood amenities, so amenity changes affect their wealth in addition to their flow utility. Third, higher transaction costs make ownership less reversible, implying that owners are exposed to local amenity shocks over a longer effective horizon than renters.

We assume utility to be Cobb-Douglas over a numeraire consumption good C_i and housing services H_i, with an additive amenity component:

$$u^R_{i,d} = A(m_d) + \frac{C_i^{1-\alpha} H_i^{\alpha}}{\alpha^{\alpha}(1-\alpha)^{1-\alpha}} + \varepsilon_{i,d} \qquad \text{(if renting)} \tag{5}$$

$$u^O_{i,d} = \theta\, A(m_d) + \frac{C_i^{1-\alpha} H_i^{\alpha}}{\alpha^{\alpha}(1-\alpha)^{1-\alpha}} + \varepsilon_{i,d} \qquad \text{(if owning)} \tag{6}$$

where $\alpha \in (0,1)$ is the housing expenditure share, $\varepsilon_{i,d}$ is an idiosyncratic *neighborhood preference* shock, and θ scales the amenity sensitivity for owners. The budget constraint is $C_i + p_d H_i = Y$, where Y is income (common across natives), $p_d = r_d$ is the rental price for renters, and $p_d = (i+\delta)P_d$ is the user cost of ownership for owners, with i denoting the interest rate and δ the depreciation rate.

Standard utility maximization yields $C_i = (1-\alpha)Y$, housing demand $H_i = \alpha Y/p_d$, and indirect utility:

$$V^R_{i,d} = A(m_d) + Y\, r_d^{-\alpha} + \varepsilon_{i,d} \tag{7}$$

$$V^O_{i,d} = \theta\, A(m_d) + Y\, [(i+\delta)P_d]^{-\alpha} + \varepsilon_{i,d} \tag{8}$$

Within-neighborhood indifference. Since $\varepsilon_{i,d}$ is realized at the neighborhood level – it enters both (7) and (8) identically – in equilibrium, a native in area d is indifferent between renting and owning.

We normalize $A(0) = 0$, so that owners and renters perceive amenities identically in the absence of immigration, and define $\psi \equiv \alpha Y (r^*)^{-(\alpha+1)} > 0$. The linearized indifference condition is:

$$r_d = (i+\delta)P_d - \frac{(\theta-1)A'(0)}{\psi} m_d \tag{9}$$

If immigrants are perceived as a disamenity ($A'(0) < 0$), then $(i+\delta)P_d < r_d$ for $m_d > 0$: rents rise above the user cost of ownership, as owners bear the amplified amenity loss. If instead immigrants are perceived as an amenity ($A'(0) > 0$), then $(i+\delta)P_d > r_d$: owners benefit disproportionately, and the premium is capitalized into higher prices. In both cases, the standard user cost relationship $r^* = (i+\delta)P^*$ holds only in the absence of immigration. We now focus on the case $A'(0) < 0$ and show that this assumption leads to conclusions consistent with those in Table 4.

Housing demand and market clearing. Cobb-Douglas preferences imply individual housing demand $H_i = \alpha Y/r_d$ for natives and $H_i = \alpha\gamma Y/r_d$ for immigrants, where $\gamma \in (0,1]$ is the immigrant-to-native income ratio.

In neighborhood A, where it is assumed there are no immigrants, the amount of residents is given by n_A, and total demand is $n_A \cdot \frac{\alpha Y}{r_A} = \bar{H}$. In neighborhood B, immigrants add to housing demand. Individual immigrant housing demand is $H_i = \alpha\gamma Y/r_B$, where $\gamma \in (0,1]$ is the immigrant-to-native income ratio.

Suppose a fraction ϕ of natives rent and the remaining $(1-\phi)$ own. Market clearing requires:

$$\phi\, n_B \cdot \frac{\alpha Y}{r_B} + (1-\phi)\, n_B \cdot \frac{\alpha Y}{(i+\delta)P_B} + \gamma m \cdot \frac{\alpha Y}{r_B} = \bar{H} \tag{10}$$

Substituting expression (9):

$$\phi\, n_B \cdot \frac{\alpha Y}{r_B} + (1-\phi)\, n_B \cdot \frac{\alpha Y}{r_B + \frac{(\theta-1)A'(0)m}{\psi}} + \gamma m \cdot \frac{\alpha Y}{r_B} = \bar{H} \tag{11}$$

The expression above shows that r_B depends on the percentage of renters in the economy. Intuitively, buyers must be compensated with a higher consumption of housing services, relative to rents, to be indifferent between the two options. Consequently, if a higher proportion of natives in region B are homeowners, all else equal housing consumption rises. In equilibrium, prices will rise to accommodate the additional demand.

Let $\hat{x}$ represent the deviation of x with respect to its no-immigration equilibrium, i.e. $\hat{x} = \frac{x-x^*}{x^*}$. With the further simplifying assumption that the tenure share ϕ is constant across neighborhoods, one can show that:

$$\hat{r}_B = \hat{n}_B + \frac{2\gamma m}{N} - \frac{(1-\phi)\, w(m)}{r^*} \tag{12}$$

$$\hat{q}_B = \hat{n}_B + \frac{2\gamma m}{N} + \frac{\phi\, w(m)}{r^*} \tag{13}$$

$$\hat{r}_B - \hat{q}_B = -\frac{w(m)}{r^*} > 0 \tag{14}$$

where $w(m) \equiv (\theta - 1)A'(0)\, m/\psi < 0$ is the amenity wedge.

The first two expressions share the same demand fundamentals: native outflows ($\hat{n}_B < 0$, pushing both rents and prices down) and immigrant arrivals ($2\gamma m/N > 0$, pushing both up). These terms capture the standard population driven housing demand channel and affect rents and prices symmetrically.[37]

The tenure composition, however, enters with opposite signs. In the rent equation, a higher share of owners (lower ϕ) increases the correction $-(1-\phi)\, w(m)/r^* > 0$: owners face a lower per-unit cost than renters and therefore consume more housing, adding to aggregate demand and pushing rents up. In the price equation, a higher share of renters (higher ϕ) increases the correction $\phi\, w(m)/r^* < 0$: fewer owners means a thinner ownership market, and the amenity loss is capitalized more heavily into each remaining unit, pushing prices down.

Despite these opposing effects on the individual levels, the third expression shows that they cancel exactly when differenced: the wedge between rents and prices is $-w(m)/r^* > 0$, independent of ϕ, $\hat{n}_B$, and γm. It is governed solely by the amenity wedge $w(m)$, which measures how much more owners suffer from the change in neighborhood composition relative to renters.

Since $n_A + n_B = N$ and, by simplicity, we assume that, initially, $n_A = n_B = \frac{N}{2}$, we have $\hat{n}_A = -\hat{n}_B$. Neighborhood A has no immigrants and no amenity wedge, so $\hat{r}_A = \hat{n}_A = -\hat{n}_B$ and $\hat{q}_A = \hat{n}_A = -\hat{n}_B$. The cross-neighborhood differentials in rents and prices are:

$$\hat{r}_B - \hat{r}_A = 2\hat{n}_B + \frac{2\gamma m}{N} - \frac{(1-\phi)\, w(m)}{r^*} \tag{15}$$

$$\hat{q}_B - \hat{q}_A = 2\hat{n}_B + \frac{2\gamma m}{N} + \frac{\phi\, w(m)}{r^*} \tag{16}$$

$$(\hat{r}_B - \hat{r}_A) - (\hat{q}_B - \hat{q}_A) = -\frac{w(m)}{r^*} > 0 \tag{17}$$

Expressions (15) and (16) are the model analogues of our empirical estimates: they measure the percent change in rents and prices in the immigrant-receiving neighborhood relative to that

[37] A feature of this setup worth noting is that amenities enter utility additively and therefore do not shift housing demand directly: individual housing consumption is independent of $A(m_d)$. Amenities instead affect housing outcomes through two indirect channels visible in (12): the location choice of natives across neighborhoods (through $\hat{n}_B$), and the tenure wedge between rents and prices (through $w(m)$). We view this as an attractive feature of the specification rather than a limitation. A worse amenity reduces the overall desirability of living in the neighborhood and therefore the mass of residents who choose to locate there, but conditional on living there, households still allocate the same share of income to housing.

with no immigration.[38] These differentials correspond directly to the coefficients estimated in our shift-share IV regressions, which identify the effect of immigration on housing costs in a neighborhood relative to neighborhoods that did not receive an inflow.

Equilibrium native reallocation. Combining the logit choice with the rent differential delivered by the market-clearing conditions, we obtain a closed-form expression for the equilibrium outflow of natives from the immigrant-receiving neighborhood:

$$\hat{n}_B = \frac{m}{2(\sigma + \psi r^*)} \left\{ A'(0) \left[1 + (1-\phi)(\theta - 1)\right] - \frac{2\psi r^* \gamma}{N} \right\}, \tag{18}$$

with $\hat{n}_A = -\hat{n}_B$ by the population constraint. Both terms inside the braces are negative, so $\hat{n}_B < 0$: natives flow out of neighborhood B. Two forces drive this outflow. The first, $A'(0)\,[1 + (1-\phi)(\theta - 1)]$, is the amenity channel: immigration makes B less desirable, and the effect is amplified in proportion to the share of owners, whose heightened amenity sensitivity $(\theta - 1)$ enters the location decision through the within-neighborhood indifference condition. The second, $-2\psi r^* \gamma / N$, is the cost-of-living channel: immigration raises rents in B relative to A, which independently pushes natives out.

Rent vs. Price Changes. Expression (17) shows that the change in rents in the immigrant-receiving neighborhood always exceeds the change in prices, regardless of the tenure composition ϕ, native outflows $\hat{n}_B$, or the immigrant demand shock γm. From the signs of (15) and (16) individually, three cases are possible:

- (i) Both rents and prices rise, with rents more (when demand $2\gamma m/N$ dominates);
- (ii) Rents rise while prices fall (when the amenity wedge is strong enough to overturn the price effect);
- (iii) Both fall, with prices more (when native outflows are large enough to pull rents in A up relative to B).

Our empirical results for houses are consistent with case (ii): rents rise significantly while the price effect is statistically insignificant and possibly negative. The results for units point to case (i), where both rents and prices rise. To account for this heterogeneity, a simple extension of the model, shown below in Subsection F.2, explores a case with two dwelling types.

Short run versus long run. The model can be reinterpreted as describing different stages of residential adjustment. In the short run, natives have not yet relocated ($\hat{n}_B \approx 0$) and the rent and price differentials are driven entirely by immigrant demand and the amenity wedge.[39] Over time,

[38] The factor of 2 arises because the initial equilibrium is symmetric ($n_A = n_B = N/2$), so that $\hat{n}_A = -\hat{n}_B$.

[39] A model-consistent way to ensure this is to introduce a (large) fixed utility cost of moving in the short run. Alternatively, we can set $\sigma \to \infty$.

native outflows materialize $\hat{n}_B < 0$, attenuating the rent and price differentials and potentially shifting the economy from case (i) toward case (iii).

We interpret our contemporaneous estimates, based on annual data, as capturing the short-run response before substantial resorting has occurred. [40] This may explain why our rent estimates are larger than those typically found in the literature: studies relying on decennial data, such as Saiz and Wachter (2011), implicitly estimate a weighted average of impact and adjustment periods, in which native outflows have partially offset the initial demand shock. Indeed, Saiz and Wachter (2011) find negative effects on neighborhood-level house prices using decennial US census data, consistent with the economy having moved toward case (ii) or case (iii) over a ten-year horizon. However, even at an annual frequency, Sá (2015) finds negative house price effects in UK local authorities, driven by "*almost one-for-one native displacement*". Through the lens of our model, this requires native mobility to be high enough to push the economy into case (ii) or case (iii) within a single year, which in turn requires the amenity force $A'(0)\left[1 + (1-\phi)(\theta - 1)\right]$ in (18) to be large, consistent with the dis-amenity interpretation emphasized by Sá (2015).

While the static cases above are stylized representations of an inherently dynamic process, we view them as sufficient to organize the cross-horizon and cross-dwelling-type patterns documented in Sections 6 and 7. A fully dynamic model with explicit moving costs and forward-looking location choice would preclude the simple analytical characterizations that make this framework a useful organizing device, and we leave such an extension for future work.

Discussion. If house and rental markets were segmented (or even partially so, e.g., due to credit constraints that prevent some immigrants from buying), the fact that immigrants are predominantly renters would by itself imply that their inflow puts more pressure on rents than on prices, generating a wedge between the two without any further mechanism. We deliberately abstract from this channel by imposing indifference between renting and owning, which makes the two tenure types perfect substitutes for natives. Under this assumption, the only force that can drive a wedge between rents and prices is the differential amenity sensitivity across tenure types. The fact that the model still delivers rent effects exceeding price effects, and house prices that may turn negative, shows that the amenity channel is sufficient to rationalize our patterns on its own. The goal is not to dismiss the role of immigrants being renters, but to offer a complementary mechanism that operates alongside it.

Aggregate rents and prices. Given the symmetry across regions, the response of city-wide aggregate rents and prices, defined as population-weighted averages across neighborhoods, is:

$$\hat{r}_{\text{agg}} = \frac{\gamma m}{N} - \frac{(1-\phi)\, w(m)}{2r^*}, \tag{19}$$

$$\hat{q}_{\text{agg}} = \frac{\gamma m}{N} + \frac{\phi\, w(m)}{2r^*}. \tag{20}$$

[40] In this simple model, as in Accetturo et al. (2014), we don't consider immigrants' resorting.

Under $w(m) < 0$ and $\gamma > 0$, aggregate rents rise unambiguously: the direct demand effect from immigrant arrivals is reinforced by natives shifting from owning to renting, as the larger amenity loss for owners makes ownership relatively less attractive. The response of aggregate prices is ambiguous, with the demand term pushing them up and the amenity term pushing them down. Which force dominates depends on the share of renters (ϕ) and the strength of the amenity response.

How do neighborhood-level estimates compare with the city-level responses? To answer this question, we consider the same per-capita immigration intensity at both scales: m immigrants arriving in B (density $2m/N$ of the local population) alongside $2m$ immigrants arriving in region B in a city of N natives (density $2m/N$ of the city population). Under this normalization, the demand and amenity channels enter with identical magnitude at both scales, and the difference between the neighborhood-level and aggregate responses reduces to

$$(\hat{r}_B - \hat{r}_A) - \hat{r}_{\text{agg}} = (\hat{q}_B - \hat{q}_A) - \hat{q}_{\text{agg}} = 2\hat{n}_B = \frac{m}{\sigma + \psi r^*} \left\{ A'(0) \left[1 + (1-\phi)(\theta - 1)\right] - \frac{2\psi r^* \gamma}{N} \right\} \leq 0$$

Both rent and price responses at the neighborhood level are strictly smaller than their aggregate counterparts, with the gap governed entirely by the strength of native out-migration. Intuitively, out-migration attenuates the neighborhood-level response both by increasing demand pressures in A and alleviating them in B, reducing the rent and price wedge. This reallocation cancels at the city level, implying a larger change in rents and prices.

The contrast between neighborhood-level and aggregate responses highlights what is distinctive about (empirical) results obtained using granular data. For one, native reallocation flows are null on aggregate; granular data therefore makes out-migration as a mechanism visible and quantifiable. Second, aggregate price and rent responses are unambiguously larger than neighborhood-level ones, with the gap governed by the strength of native out-migration. Our empirical results point to a small degree of out-migration; consequently, the gap between neighborhood-level and aggregate responses is likely to be modest in our setting. Finally, as noted before, granular data also enables the identification of heterogeneity in the amenity response across neighborhood types, as we exploit in Section 7, which the aggregate cannot deliver.

F.2 Extension to Two Dwelling Types

We extend the model to two segmented housing markets, $k \in \{u, h\}$ (units and houses). Natives are either unit-type or house-type and do not switch between them in the short run, reflecting limited substitution across dwelling types that cater to populations at different life stages.[41] For tractability, we assume that the two markets are identical at the initial equilibrium, with common income Y and a common housing-stock-to-population ratio across markets, so that $r_h^* = r_u^* = r^*$ and $\psi_h = \psi_u = \psi$. The two markets differ only in their amenity sensitivity θ_k, with $m_u +$

[41] At an annual frequency, transitions between house and unit markets are a small fraction of total housing transactions, making segmentation a reasonable approximation.

$m_h = m$ immigrants distributed across them. Importantly, the amenity function depends on total immigration m in the neighborhood, not on the dwelling-type-specific inflow: a house owner and a unit owner in the same neighborhood experience the same immigrant presence.

We assume $\theta_h > \theta_u > 1$: house owners are more sensitive to immigration-driven amenity changes than unit owners. This is motivated by the fact that house-owning neighborhoods tend to be older, and attitudes toward immigration are less favorable among older populations, as documented by survey evidence (Lowy Institute, 2019; Lowy Institute, 2024; Roy Morgan, 2025) and consistent with the age heterogeneity results in Section 7.[42]

The single-dwelling model cannot speak to the contrasting empirical patterns across houses and units documented in Table 4. The two-market extension allows us to rationalize these differences through a single parameter ordering. Since the two markets are segmented, the equilibrium in each market k is determined exactly as before, with type-specific parameters replacing their single-dwelling counterparts. In particular, both markets generate a dwelling-type-specific wedge $w_k(m) = (\theta_k - 1)A'(0)\, m/\psi$. As a consequence, cases (i), (ii), and (iii) apply independently to each market, and the rentprice wedge is larger for houses than for units if and only if:

$$\theta_h > \theta_u.$$

The empirical results in Table 4 map naturally onto this framework. Unit rents and prices both rise, consistent with case (i): demand pressures dominate, and the modest amenity wedge (θ_u close to 1) is insufficient to push prices negative. For houses, rents rise but prices seem not to, consistent with case (ii): the larger amenity sensitivity of house owners generates a wedge strong enough to overturn the positive demand effect in the price equation while leaving rents positive. The contrasting sign pattern across dwelling types is therefore not a puzzle requiring separate explanations, but a unified consequence of differential amenity sensitivity across tenure and dwelling types interacting with a common immigration shock.

[42] The correlation between SA2-level median age and the ratio of units among all dwellings is -0.20.