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Immigration and Regional House Prices in Iceland: Evidence from Transaction Data

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Abstract

This paper uses transaction-level data from Registers Iceland covering 225,000 residential sales across 60 Icelandic municipalities (58 retained in the merged panel) from 2006 to 2024, to examine the association between immigration and house prices. The central finding is a null result: in first difference panel regressions with year fixed effects, neither the capital region nor outside capital municipalities show a statistically significant association between changes in the immigrant share and changes in the price per square metre. A secondary finding is methodological: a positive and significant association found in aggregate regional price data disappears once year fixed effects absorb common time trends, illustrating how omitted variable bias from common time trends can produce spurious associations in aggregate regional data. One likely explanation is thin transaction markets outside the capital, where 63% of municipality-year cells contain fewer than ten sales, making annual mean prices too noisy to reveal a reliable price signal.

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Introduction

Between 2000 and 2024, the share of foreign citizens in Iceland's population rose from 2.6% to 16.6%. The increase came in two distinct surges: the banking boom of the mid-2000s drove the first, and tourism growth followed by the post-2022 Ukrainian refugee inflow drove the second. Over the same period, nominal house prices rose more than sevenfold. Whether these two facts are connected is a natural question, and one with policy relevance in a country where housing affordability has become a prominent public concern.

This paper addresses that question using a new dataset: 225,000 residential transaction records from Registers Iceland (Þjóðskrá Íslands), matched to annual municipality-level population data from Statistics Iceland's population register. The transaction data allow us to construct annual mean prices per square metre for each of 60 municipalities from 2006 to 2024, giving a panel of 870 municipality-year observations after taking first differences. This paper makes two contributions: it provides the first municipality-level test of this association using transaction data for all of Iceland, and it documents that a positive aggregate regional estimate reflects a common time trend rather than a genuine within-municipality relationship.

The regressions yield null results throughout. No specification produces a statistically significant association between changes in the immigrant share and changes in house prices at the municipality level. Outside the capital region, the point estimate is −0.8% per percentage-point rise in the immigrant share ($t = -0.80$, $p = 0.42$). Inside the capital, the estimate is positive at 6.9% but also insignificant ($t = 1.27$, $p = 0.20$). The null results hold across surge subsamples and when population growth is added as a control.

These results differ from a positive and significant association found when using aggregate regional price indices (the approach available before transaction data were obtained). In aggregate regional data (two series, 24 annual observations each), a one percentage-point rise in the immigrant share coincides with a 4.2% increase in the outside capital price index ($t = 3.21$, $p = 0.001$). We show that this apparent association reflects the common upward trend in both series rather than a genuine within-municipality relationship. When year fixed effects absorb these common trends and variation comes from cross-municipal differences in immigrant inflows, the association disappears. This contrast illustrates a general point in the housing literature: aggregate regional estimates can be misleading when underlying variation is at a finer geographic level.

The existing literature finds mixed results across settings. finds that immigration raises rents in U.S. cities. In UK local authorities, finds negative effects driven by native outmigration. Find positive effects in Spain over the 2000s boom. find negative rent effects from Germany's 2015 refugee wave. These divergent findings likely reflect differences in housing supply elasticity, markets where new construction absorbs demand quickly show smaller price responses native mobility responses and market liquidity across settings. Our null result for Iceland is consistent with thin outside capital transaction markets, where annual price signals are too noisy to detect demand responses even if they exist, and where rental market absorption or native outmigration may offset direct demand effects [1-7].

After documenting the two surges and the institutional setting in Section 2, we describe the two data sources in Section 3. Section 4 sets out the specification. Results appear in Section 5, including the contrast with aggregate estimates that motivates the analysis. Section 6 discusses the findings and Section 7 concludes.

Institutional Background

Before the Surges

Iceland was among the most ethnically homogeneous societies in the OECD at the turn of the millennium. Fewer than 3% of residents were foreign citizens in 2000, consisting mainly of workers from other Nordic countries and a smaller number from Southeast Asia employed in fish processing. Unemployment was below 2% through most of the 1990s, but labor shortages had not yet been met through systematic international recruitment.

Several features of the institutional setting bear on the analysis. Iceland joined the European Economic Area in 1994, giving EEA citizens freedom of movement without Iceland holding EU membership. Wages are set through sector-level collective bargaining with no statutory minimum wage, so immigrants and natives enter the labor market under the same agreed terms. Housing is heavily owner-occupied: historically more than 80% of households own their home, and most mortgages are indexed to the consumer price index. That indexation means inflation erodes real debt slowly, which shapes how housing demand responds to the macro shocks we study.

The First Surge, 2004–2008

The EU's eastern enlargement in May 2004 opened freedom of movement to Polish, Lithuanian, Latvian, and other central and eastern European citizens. Iceland's credit-fueled construction boom absorbed large numbers of new arrivals quickly. Banking assets grew from roughly 170% of GDP in 2003 to nearly 900% by 2008, financing a construction and real estate expansion that created strong demand for labor. Between 2004 and 2008, the share of foreign citizens rose from 3.5% to 7.4%, more than doubling in four years. Foreign inflows peaked at 9,318 in 2007. Poles became the largest immigrant group and have remained the largest ever since.

The three major commercial banks failed within a single week in October 2008. GDP contracted by approximately 10%, and unemployment rose from below 1% to nearly 9%. Construction stopped. Between 2009 and 2011, the foreign citizen share fell from 7.6% back to 6.0% as Polish and Lithuanian workers left in large numbers. This reversal is useful for the analysis: the panel includes both the surge and its unwinding [8].

The Second Surge, 2016–2024

Tourism became the main driver of labor demand from about 2010. Visitor numbers grew from roughly half a million in 2010 to over two million by 2018, pulling workers into hospitality, transport, and related services. The foreign citizen share passed its 2008 peak in 2016 and reached 11.7% by 2019.

The rate of increase accelerated sharply after 2019. Between 2019 and 2024, the number of foreign citizens rose from 40,772 to 63,528, an increase of roughly 22,800 over five years, equal to 6.5% of the 2019 population. By January 2024, foreign citizens made up 16.6% of the population.[1] Unlike the first surge, which was dominated by EEA labor migrants from a handful of countries, the second drew from a broader mix of origins and arrival channels: labor migration, family reunification, and humanitarian protection.

Geographic Variation

Across the 58 municipalities in our panel, the foreign citizen share ranged from below 4% to 58% in 2024. The capital region (Reykjavík, Kópavogur, Hafnarfjörður, Garðabær, Mosfellsbær, Seltjarnarnes, and Kjósarhreppur) held 63.6% of the national population and 62.4% of all foreign citizens in 2024. Outside the capital, immigrant concentrations were high in municipalities relying on labor-intensive industries: Reykjanesbær stood at 29.7% in 2024, and several fish-processing municipalities in the South and West had shares between 30% and 58%. Table 2 shows immigrant share trends by region.

Data

Transaction Price Data

Registers Iceland. The main price series comes from the residential transaction register maintained by Registers Iceland (Þjóðskrá Íslands), covering all registered property sales in Iceland. The dataset contains 225,361 records from 2006 to 2025, with each record including municipality, transaction date, sale price, property size in square metres, and property type. We restrict the sample to residential properties (Fjölbyli, Sérbyli, Einbyli), exclude contracts flagged as invalid (ONOTHAEFUR_SAMNINGUR = 0),2 and remove properties below 20 or above 1,000 square metres and transactions below 1 million krónur. After removing the top and bottom 2% of prices per square metre within each year, the working sample contains 31,590 transactions. The 2% symmetric trim is robust to alternatives of 1% and 5% (results available on request). The large reduction from 225,361 raw records reflects two steps: first, 173,780 records flagged as invalid contracts (ONOTHAEFUR_SAMNINGUR = 0) are excluded, these are the dominant source of attrition. Among the 51,581 valid contracts, a further 19,991 are removed by the residential property type filter (excluding commercial premises, garages, and summer houses), the size and price floors, and the 2% price trim, leaving 31,590 transactions.

We compute the mean price per square metre for each municipality-year cell as the primary outcome variable. The median municipality-year cell contains 6 transactions; 63% of cells outside the capital have fewer than ten transactions. This thinness is the primary limitation of the analysis.

Population register: Municipality-level population data come from Statistics Iceland's annual population register (table MAN04203), covering 1998 to 2025. The immigrant share is defined as the share of the population without Icelandic citizenship.

Panel Construction

We merge the two datasets at the municipality-year level, retaining the 58 municipalities (of the 60 in the transaction register, Ásahreppur and Grímsnes- og Grafningshreppur drop out due to missing population matches) and 19 annual observations (2006–2024), giving 870 municipality-year observations after taking first differences. The outcome variable is the annual log change in mean price per square metre. The main regressor is the annual change in the immigrant share, expressed in percentage points. Table 1 presents summary statistics.

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max
A. Population and immigration					
Total population	870	5,310	16,140	51	1,36,894
Immigrant share (%)	870	9.8	6.9	0.2	58
Δ Immigrant share (pp, annual)	870	0.72	1.87	−10.9	15.3
B. Transaction prices (Registers Iceland)					
Mean price per sq.m. (000 ISK)	870	178	114	32	81
Annual sales (per municipality)	870	18	47	1	482
Δ log price per sq.m.	870	0.068	0.442	-1.63	2.74

Notes: Municipality-year observations, 2007–2024 (first differences drop 2006).

Transaction price data from Registers Iceland; population data from Statistics Iceland table MAN04203. Price per sq.m. in thousands of krónur.

Immigrant share is foreign citizens as share of total population.

Table 2: Immigrant Share by Region, Selected Years

Year	Capital (%)	Outside Capital (%)	National (%)
2006	3.5	3.4	3.5
2008	7.2	7.9	7.4
2011	6.2	5.8	6.0
2016	7.4	7.6	7.5
2019	11.2	12.4	11.7
2022	13.0	13.3	13.1
2023	14.9	15.4	15.1
2024	**16.2**	**17.1**	**16.6**

Notes: Population-weighted shares within each region. Immigrant share is foreign citizens as share of total population. 2024 row in bold.

Empirical Approach

Specification

The baseline specification is a first-difference regression with year fixed effects:

$$\Delta \log(p/m^2)^m = \alpha + \beta\, \Delta s^m + \gamma + \varepsilon^m{}_{tt\ tt}$$

where m indexes the municipality, t indexes the year, p/m^2 is the mean transaction price per square metre, s^m is the immigrant share, and γ is a year fixed effect. Taking first differences removes any$_{tt}$ time-invariant municipality characteristics, including persistent differences in market depth, location, and housing stock composition. Year fixed effects absorb common national price trends. Standard errors are heteroskedasticity-robust (HC3).

The OLS estimate of β is a correlational measure. Both immigration surges coincided with economic expansions that would have raised prices regardless of immigration. We make no claim to causal identification. The estimates are partial correlations. Municipality-level fixed effects are removed through first differencing; common time trends are absorbed by year fixed effects. Standard errors follow HC3 (heteroskedasticity-consistent, thirdtype) [9].

Limitations

We attempted IV estimation using a shift-share design following with municipality-by-citizenship population data from Statistics Iceland, treating country of departure in the migration register as a proxy for citizenship. Country of departure matches citizenship for most arrivals, those transiting through a third country represent a minority that would attenuate rather than invalidate the instrument. The instrument allocates national inflows from each origin country across municipalities in proportion to that group's 2019 settlement shares [10,11].

Following the validity conditions set out in a Bartik instrument requires either exogenous shifts or exogenous shares [12]. The first-stage F-statistic is 0.3 and the correlation between the predicted and actual changes in immigrant share is near zero, rendering any 2SLS estimates inconsistent rather than merely imprecise. The shift-share design fails because settlement patterns changed substantially between the two surges: the 2019 base-year shares are heavily concentrated in the capital region, while post-2019 arrivals disproportionately settled in outside capital municipalities tied to fish processing, aluminium smelting, and tourism. When the base-year shares do not predict where new arrivals go, the standard Bartik design has no power. A valid instrument would require either a more recent base year that captures the new settlement pattern, or individual-level microdata that allow construction of origin-group-specific settlement shares at finer geographic detail. We therefore report OLS estimates throughout and treat them as correlational. The IV failure is itself informative: it confirms that 2019 settlement patterns have no predictive power for post-2019 immigrant location choices, which is consistent with the structural shift in destination markets described in Section 2.

Results

Main Results

Table 3 presents the main regression results. All specifications use municipality-year first differences with year fixed effects. The dependent variable is the annual log change in mean price per square metre.

Table 3: OLS First-Difference Estimates

	(1) Capital	(2) Capital	(3) Outside Capital	(4) Outside capital
Δ Immigrant share (pp)	0.0689	0.0681	−0.0084	−0.0127
	-0.0541	-0.054	-0.0105	-0.0146
	[1.27]	[1.26]	[−0.80]	[−0.87]
Δ Log population		0.123		0.0551
		-0.341		-0.147
Year fixed effects	Yes	Yes	Yes	Yes
Observations	116	116	754	754
R^2	0.186	0.187	0.078	0.079

Notes: Dependent variable is the annual log change in mean transaction price per square metre.

Key regressor is the annual change in the municipality-level immigrant share (percentage points). Year fixed effects included in all specifications. Heteroskedasticity-robust standard errors (HC3, see Section 4.1) in parentheses. t-statistics in square brackets. No coefficient reaches conventional significance levels. N = 116 (capital) and 754 (outside capital) municipality-year observations.

Specifications (1) and (2) cover the capital region. The point estimate is positive at 6.9% per percentage-point rise in the immigrant share, but the standard error is 5.4 percentage points and the t-statistic is 1.27. Adding population growth in specification (2) leaves the estimate unchanged. The capital area records several hundred transactions per municipality per year, so thin-market noise is an unlikely explanation given the volume of transactions. We cannot, however, distinguish the capital estimate from zero with the available data.

Specifications (3) and (4) cover the outside capital region. The point estimate is −0.8%, small and negative, with a standard error of 1.1 percentage points (t = −0.80). The 95% confidence interval runs from −2.9% to +1.2%. Adding population growth in specification (4) leaves the estimate near zero and insignificant. Year fixed effects account for about 8% of the variance in annual price changes; the immigrant share contributes no additional explanatory power. Section 5.3 examines whether this null result depends on the inclusion of municipalities with very few annual transactions.

Surge Subsamples

Table 4 reports estimates for the outside capital region split by surge period. Neither subsample shows a significant association. During 2006 to 2011 (covering most of the first surge), the coefficient is −1.7% (p = 0.11). During 2016 to 2024, it is near zero (−0.2%, p = 0.92). The firstsurge estimate is the closest to significance, but remains well outside conventional thresholds and has the wrong sign for a positive immigration effect.

Table 4: OLS Estimates by Surge Period, Outside Capital Region

	(5) Outside capital, 2006–2011	**(6) Outside capital, 2016–2024**
Δ Immigrant share (pp)	−0.0171	−0.0016
	-0.0106	-0.0159
	[−1.62]	[−0.10]
Observations	203	389
R^2	0.042	0.085

Notes: Outside capital region only, 2007–2024 first differences with year fixed effects.

Heteroskedasticity-robust standard errors (HC3, see Section 4.1) in parentheses. t-statistics in square brackets. N = 203 (spec 5) and 389 (spec 6).

Minimum Sales Threshold

The null result for outside capital municipalities in Table 3 pools all 51 outside capital municipalities with at least one transaction, including many with only 1 to 5 sales per year. Mean prices from such thin cells are dominated by the composition of the small number of properties that happened to sell in a given year rather than by genuine price movements. Table 5 examines whether the main result is sensitive to excluding municipalities below a minimum annual sales threshold.

Table 5: Outside Capital: Sensitivity to Minimum Annual Sales Threshold

Min. Sales	**Munis**	**N**	**β (%)**	**SE (%)**	**t**	**p**	**R^2**
≥ 0 (All)	51	754	−0.843	1.054	−0.80	0.424	0.078
≥ 5	39	425	0.390	0.838	0.47	0.641	0.133
≥ 10	31	277	1.822	0.834	2.18	0.029**	0.175
≥ 15	25	194	1.106	0.653	1.69	0.091*	0.253
≥ 20	21	142	0.687	0.630	1.09	0.276	0.327
≥ 30	14	95	0.704	0.950	0.74	0.459	0.429

Notes: Each row restricts the outside capital sample to municipality-year observations with at least the indicated number of annual residential transactions. Dependent variable is the annual log change in mean transactions. Dependent variable is the annual log change in mean transaction price per square metre. Year fixed effects included throughout. Heteroskedasticity-robust standard errors (HC3, see Section 4.1). ** $p < 0.05$, * $p < 0.1$.

The results are sensitive to the threshold. With no restriction, the coefficient is negative and insignificant (beta = -0.84%, p = 0.42). As the threshold rises to 10 sales per year, the coefficient turns positive and significant at the 5% level (beta = 1.82%, t = 2.18, p = 0.029, N = 277 observations across 31 municipalities). At thresholds of 15 and above, the estimate becomes insignificant again, consistent with the shrinking sample reducing statistical power as fewer municipalities remain. The result at the 10-sale threshold is most interpretable: below this count, annual mean prices are dominated by sample composition rather than genuine price movements.

We note that testing six thresholds and reporting the one that reaches significance raises a multiple testing concern; applying a Bonferroni correction for six tests raises the required significance level to $p < 0.008$, which the ≥10 result (p = 0.029) does not satisfy. The threshold result should therefore be read as suggestive rather than conclusive.

These results suggest that the null finding in Table 3 is partly a measurement problem: markets with fewer than 10 annual transactions produce price estimates too noisy to detect any association, whether or not one exists. In better-measured markets, a positive association is detectable in the second surge period. The magnitude (1.8% per percentage point) is smaller than the aggregate regional estimate (4.2%) and consistent in sign.

Among the 31 municipalities with at least 10 annual sales, the second surge period drives the result. During 2016 to 2024, the coefficient is 3.8% (t = 2.15, p = 0.032, N = 140), while during 2006 to 2011 it is 0.7% and insignificant (p = 0.38). This pattern reverses the surge-period finding in the full sample and is consistent with the aggregate regional estimates, which also show the stronger association in the second surge period when outside capital immigrant shares substantially exceeded capital levels.

Comparison With Aggregate Regional Estimates

Setting aside the threshold analysis, the broader pattern across all outside capital municipalities is a null result. A natural question is how this relates to the positive association found in aggregate regional price indices. Using Statistics Iceland's two regional HPI series (capital area multi-flat and outside capital total) over 2000–2024, a first-difference regression of the annual log change in the regional index on the population-weighted immigrant share yields a coefficient of 4.2% for the outside capital region (t = 3.21, p = 0.001, N = 24 region-year observations).

This positive result reflects the common upward trend in both series over the sample period rather than a genuine within-municipality relationship. When year fixed effects absorb these common time trends, as in the municipality-level regressions, the association disappears. The aggregate result reflects omitted variable bias from common time trends: the regional price index and the immigrant share both trended upward over 2000–2024, and without year fixed effects the regression attributes this shared trend to immigration.

Discussion

Thin Markets and Price Noise

One plausible explanation for the null result outside the capital is market thinness. With a median of 6 transactions per municipality-year, the annual mean price per square metre is a noisy signal of the true price level. Year-on-year changes in this thin-market estimate reflect the composition of the handful of properties that happened to sell in a given year as much as they reflect genuine price movements. The standard deviation of Δ log price per sq.m. outside the capital (0.46) is more than seven times larger than the analogous statistic from Statistics Iceland's aggregate outside capital index (0.063), confirming that municipality-level annual prices are dominated by noise.

In thin markets, a genuine price response to immigration is difficult to detect with annual data. The threshold analysis is consistent with this: a positive association of around 1.8% is suggested by the threshold analysis in better-measured markets, though not robustly significant. Assessed property values from the municipal tax register, available annually for every property in Iceland, would substantially reduce this measurement problem, assessed values are administrative appraisals that track market prices with a lag, but they cover every property annually, eliminating the thin-count issue.

Supply and Displacement

Beyond market thinness, two further channels could produce a genuine null at the municipality level. If immigrants predominantly enter the rental market, sale prices may not respond to immigration-driven demand even where supply is constrained (González and Ortega, 2013). Iceland's rental market is small and largely unregulated, and international evidence suggests recent arrivals tend to rent initially before transitioning to ownership. If the immigration effect operates through rents and only slowly capitalizes into sale prices, annual sale price data would miss it. A rough calibration illustrates the magnitude: if, for example, a 1 percentage-point rise in immigrant share raises rents by 0.5%, consistent with the order of magnitude found in U.S. settings and if price-to-rent capitalization takes five years, the implied annual transaction price effect is around 0.1%, well within our confidence intervals and undetectable with annual data [1,2].

Second, native outmigration could offset the direct demand increase from immigrants, as documents for the UK and for the United States. If higher-income Icelandic citizens leave municipalities receiving large immigrant inflows, the income loss reduces housing demand. The public population register does not record internal migration flows, so we cannot test this channel directly. The capital region's growing share of the national population over the sample period is, however, consistent with some net movement of Icelanders toward the capital, which may partly reflect outmigration of Icelandic citizens from high-immigration municipalities [2,11].

Data Limitations and Next Steps

The binding constraint in this analysis is not immigrant variation, which is substantial across Icelandic municipalities, but the thinness of outside capital transaction markets. Two improvements would advance the analysis. First, assessed property values from the municipal property tax register are available annually for every property in Iceland and would give a price observation for every municipality-year without the noise of thin transaction counts. Second, rental price data, Statistics Iceland does not currently publish municipal-level rental prices, would allow a direct test of whether immigration affects rents rather than sale prices. Researchers can apply for both datasets through the respective institutions. Provides a decomposition framework separating supply-side from demand-side price channels; assessed value data would enable this decomposition for Iceland [13].

A microdata approach that separates immigrants by occupation or wage quartile would allow a cleaner test of heterogeneous demand effects in the capital. Such data are available at Statistics Iceland under research access agreements and represent the most promising avenue for improving precision in the capital region estimates.

Interpreting The Capital Region Estimate

Three explanations are consistent with a large but imprecise capital estimate. First, the capital housing market is supply-elastic relative to outside capital municipalities. Reykjavík and the surrounding municipalities have developed new residential areas throughout the sample period; new construction absorbs demand shocks and suppresses price responses. Shows that supply elasticity is the primary determinant of price responses to local demand shocks, and Iceland's capital region would score as relatively elastic by European standards. Second, the capital receives immigrants from a broader range of origin countries and occupational groups than outside capital municipalities, which are dominated by workers in a single sector. If different immigrant groups have different housing demand profiles, the aggregate capital estimate masks heterogeneous effects. Third, the capital also receives the bulk of high-income returning Icelanders and foreign professionals in finance and technology, whose housing demand is qualitatively different from that of low-wage fish-processing workers. The immigrant share conflates these groups [5].

The capital region estimate deserves separate attention. At 6.9% per percentage-point rise in the immigrant share, the point estimate is economically large, comparable to the aggregate outside capital estimate of 4.2%, but the standard error of 5.4 percentage points makes it statistically indistinguishable from zero. This wide confidence interval reflects genuine uncertainty rather than a thin-market measurement problem: the capital has hundreds of transactions per municipality per year, so the outcome variable is well-measured. The uncertainty is in the relationship itself.

Settlement Patterns and The Limits of Bartik Identification

The failure of the shift-share instrument is itself informative about Iceland's immigration dynamics. In the standard Bartik design, the identifying assumption is that immigrants from a given origin country settle across destinations in proportion to historical co-ethnic networks. This assumption is plausible when migration is driven by family reunification or social chain migration, where established communities attract compatriots. It is less plausible when migration is driven by labour demand in specific sectors, where employer-led recruitment can redirect workers to destinations with no prior co-ethnic presence [11].

Iceland's second surge fits the latter pattern. Between 2019 and 2024, roughly 22,800 foreign citizens arrived, but they disproportionately settled in outside capital municipalities tied to fish processing, aluminium smelting, and hospitality, sectors that expanded rapidly and recruited internationally. The 2019 settlement distribution, which is capital-heavy, therefore has zero predictive power for post-2019 destination choices. The first-stage F-statistic of 0.3, essentially a coin toss, confirms this empirically. This pattern is not unique to Iceland: any country undergoing a rapid sectoral reorientation of labour demand will generate settlement pattern shifts that invalidate base-year Bartik shares. Researchers applying this identification strategy to small open economies with concentrated industrial geographies should test first-stage predictive power across subperiods before relying on the full-sample instrument.

The implication for identification is practical. A shift-share instrument that uses 2019 shares and post-2019 shocks will always be weak in this context, regardless of the quality of the shock data. A more recent base year, say, 2021 or 2022, after the new settlement pattern had stabilised, might restore first-stage power. Alternatively, an instrument based on exogenous variation in sector-level labour demand (for example, tourist arrivals by municipality as an instrument for hospitality-sector immigrant inflows) could sidestep the network-migration assumption altogether. Neither instrument is constructable from the currently available public data; both are feasible with employer-linked administrative records.

Conclusion

This paper uses 225,000 residential transaction records from Registers Iceland matched to municipality-level population data to examine whether immigration affects house prices in Iceland. The main finding is that there is no statistically significant association between changes in the immigrant share and changes in the price per square metre at the municipality level, either inside or outside the capital region.

The full-sample null result reflects, at least in part, the thinness of outside capital transaction markets. When the sample is restricted to municipalities with at least 10 annual transactions, a suggestive positive association of 1.8% per percentage point emerges ($p = 0.03$, not significant after Bonferroni correction), driven by the second surge period. The full-sample null is therefore consistent with a genuine positive effect that thin-market noise makes undetectable, but it does not establish that such an effect exists: the threshold estimate of 1.8% is OLS and carries upward bias from coincident economic expansions. The true effect, if any, likely lies between zero and 1.8%. Rental market channels and native outmigration may additionally dampen observable price responses.

The methodological contribution, showing that aggregate regional estimates conflate common time trends with genuine local associations, has implications beyond Iceland: it applies wherever researchers use coarse geographic price indices to study local demand shocks.

Two data improvements would sharpen the analysis. Annual assessed property values from the municipal tax register would replace noisy thin-market transaction means with comprehensive price coverage for every municipality in every year, eliminating the measurement problem that drives the full-sample null result. A valid shift-share instrument for Iceland would require individual-level microdata linking citizenship to municipality of residence over time, enabling settlement shares that reflect actual destination choices rather than historical patterns that no longer hold.

The absence of a detectable municipality-level price effect is consistent with, but does not establish, the view that immigration affects housing costs primarily through aggregate demand on the national stock rather than through local concentration effects. This paper does not test national level dynamics, and the evidence does not allow a strong preference for national over local policy instruments. What the evidence does suggest is that targeting high-immigration municipalities for supply intervention is not supported by the local price data [14-18].

- Statistics Iceland's published figure of 18.2% for January 2024 is based on country of birth. Our measure uses citizenship and gives 16.6%, because individuals born abroad who have been naturalized count as Icelandic citizens in our series.
- In the Registers Iceland dataset, ONOTHAEFUR_SAMNINGUR = 1 indicates a valid contract. We retain only valid contracts.
- The population register was revised in March 2024, with a corrected estimation method applied retrospectively from 2011. We use the revised figures throughout.

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