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Urban Employment Growth and the Housing Price-Productivity Gap: Evidence from 35 OECD Functional Urban Areas

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ABSTRACT

Urban economic growth can enhance productivity and employment while intensifying pressure on housing markets, raising questions about whether housing-price appreciation remains proportionate to improvements in metropolitan economic performance. This study examines the relationships among employment, labour productivity, and house-price indices using an unbalanced panel of 600 city-year observations for 35 OECD functional urban areas in 11 countries between 2000 and 2023. A house-price-productivity gap is constructed from rebased house-price and labour-productivity indices to identify periods in which housing-price growth outpaces productivity growth. Two-way fixed-effects models, first-difference estimation, and sensitivity analyses examine within-city co-movement while accounting for persistent metropolitan characteristics and common temporal shocks. Annual house-price growth is positively associated with contemporaneous employment and productivity growth. In the levels specification, a 10% increase in employment is associated with 3.2% higher labour productivity and a 0.132-log-point increase in the house-price-productivity gap, equivalent to a 14.1% increase in the index ratio. The gap coefficient is an accounting implication of the productivity and house-price equations rather than an independent causal effect. Results remain positive across samples, denominators, and fixed-effects specifications, although their magnitudes vary substantially. The findings demonstrate that urban economic expansion and housing-market appreciation frequently move together but do not necessarily proceed at comparable rates. The price-productivity gap therefore provides a transparent diagnostic indicator of metropolitan divergence rather than a measure of housing affordability or welfare. Its application can identify urban areas where employment growth, productivity performance, and housing-market change warrant detailed investigation of housing supply, land-use conditions, household incomes, and distributional outcomes.

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Highlights:

- Links employment, productivity, and housing-price indices across 35 OECD functional urban areas.
- Employment growth co-moves positively with urban productivity and house prices.
- House-price growth frequently outpaces productivity growth across metropolitan areas.
- A 10% employment increase corresponds to a 14.1% higher price-productivity index ratio.
- The price-productivity gap provides a diagnostic indicator of urban economic divergence.

Contribution to the field statement:

This study integrates employment, labour productivity, and housing-price trends across 35 OECD urban areas. It introduces the house-price-productivity gap as an indicator of divergence between housing appreciation and productive performance, while distinguishing it from affordability. The findings reveal systematic co-movement but substantial variation across cities, helping identify metropolitan areas requiring further investigation of housing supply, land use, incomes, accessibility, and distributional outcomes.

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1. Introduction

1.1 Context and research problem

Urban concentration is widely associated with economic advantages arising from the spatial proximity of firms, workers, infrastructure, and specialized services. Larger labour markets can improve the matching of workers to suitable jobs, enable firms to share suppliers and production inputs, and facilitate the transmission of knowledge across organizations and industries. Through these mechanisms, agglomeration can enhance productive efficiency and support innovation, specialization, and learning. These benefits help explain why large and economically diverse urban labour markets are frequently more productive than smaller or less connected locations (Duranton & Puga, 2020; Henderson & Thisse, 2024; Puga, 2010). Nevertheless, the economic advantages associated with urban concentration are not distributed automatically or evenly. They emerge through interactions among labour markets, firm location decisions, infrastructure provision, housing availability, and the capacity of urban institutions to accommodate additional economic activity.

The same attributes that make cities productive can also increase demand for urban land and housing. Productive and amenity-rich cities attract households seeking employment opportunities, services, accessibility, and quality-of-life advantages, while firms compete for access to workers, customers, and strategically valuable locations. Because well-located urban land is inherently limited, increases in demand may be reflected in higher rents and property prices, particularly where the housing stock adjusts slowly. Consequently, higher productivity and higher housing prices are not necessarily contradictory outcomes; both may arise from the concentration of economic opportunities and amenities within spatially constrained markets (Albouy, 2016; Glaeser & Gottlieb, 2009). The analytical challenge is therefore not merely to determine whether productive cities have expensive housing, but to examine whether housing-price appreciation remains proportionate to changes in the productive performance of those cities.

This distinction is central to the research problem addressed in the present study. When house prices increase more rapidly than labour productivity, the resulting divergence may indicate that access to productive urban locations is becoming increasingly costly relative to locally measured economic performance. Such a gap can emerge through several processes, including slowly adjusting housing supply, changes in credit conditions, expectations of future appreciation, evolving urban amenities, migration, or shifts in the composition and quality of transacted dwellings. However, the observed divergence cannot, by itself, identify which of these mechanisms is responsible. Nor does it establish who benefits from or bears the costs of housing-price appreciation. Owners, renters, prospective residents, recent entrants, and incumbent households may experience the same market changes very differently. Accordingly, a housing-price index should not be interpreted directly as a measure of affordability, welfare, or distributional incidence.

The relationship is also relevant to the spatial allocation of labour and economic activity. If housing costs rise sufficiently in productive cities, they may restrict the ability of workers—particularly lower- and middle-income households—to relocate toward areas offering stronger employment opportunities. In this way, housing-market pressures can weaken migration responses, constrain labour-market matching, and reduce the extent to which workers and firms benefit from productive urban environments (Ganong & Shoag, 2017; Hsieh & Moretti, 2019). At the same time, the strength and direction of these relationships are unlikely to be uniform across national and metropolitan contexts. Housing institutions, mortgage markets, planning systems, construction capacity, demographic pressures, and local economic structures differ considerably across cities. Empirical evidence similarly demonstrates that metropolitan price–income relationships vary across locations and periods, limiting the validity of a single universal interpretation of rising urban housing prices (Oikarinen et al., 2023). The COVID-19 pandemic introduced an additional source of complexity by altering the relationship between workplaces, residences, and access to central urban locations. The expansion of remote and hybrid work changed household preferences for space and accessibility, while monetary conditions, migration patterns, and expectations affected housing demand unevenly across cities. Evidence of pandemic-induced changes in the valuation of urban real estate therefore reinforces the importance of

distinguishing long-term metropolitan relationships from period-specific shocks (Gupta et al., 2022). These developments strengthen the need for a transparent comparative framework that can document how employment, productivity, and housing-price indicators move together without assigning causal interpretations that the available data cannot support.

Against this background, this article combines Organisation for Economic Co-operation and Development (OECD) employment, labour-productivity, and house-price indices for 35 functional urban areas (FUAs) observed between 2000 and 2023. FUAs are used because they approximate integrated metropolitan labour and commuting markets more closely than many administrative jurisdictions. The study examines whether changes in metropolitan employment are associated with changes in labour productivity and house prices after persistent city characteristics and common annual shocks are taken into account. It then evaluates whether the growth of the house-price index exceeds that of the labour-productivity index and how this divergence is connected to the estimated employment relationships.

The empirical strategy is deliberately descriptive and accounting based. Employment, productivity, migration, housing demand, and housing prices are jointly determined within urban systems, and the available international panel does not provide a common exogenous shock capable of isolating a causal employment effect across all sampled cities. The reported fixed-effects and first difference estimates must therefore be interpreted as conditional associations rather than structural parameters. In particular, the analysis does not estimate an agglomeration production technology, identify a housing-supply elasticity, or establish a causal capitalization effect. This bounded interpretation is essential because it aligns the substantive conclusions with what can be supported by the data and research design.

1.2 Contribution and scope

The study makes three closely related but deliberately bounded contributions. The first is the construction of a harmonized FUA-year analytical framework linking metropolitan employment, labour productivity, and house-price indices. Bringing these variables together at the functional urban scale improves the conceptual alignment between the unit of observation and the economic processes under investigation. Employment growth, commuting, residential choice, and housing demand frequently extend beyond municipal boundaries; consequently, analyses based exclusively on administrative jurisdictions may divide integrated labour and housing markets or combine areas that are not economically connected. The FUA framework does not eliminate all measurement differences across countries, but it provides a more defensible basis for international metropolitan comparison.

The second contribution is the definition of a transparent house-price–productivity gap. After both the house-price index and labour-productivity index are rebased to 2015, the gap is specified as $(P = \ln(HPI/PROD))$, which is equivalent to the difference between the logarithms of the two rebased indices. Positive changes in (P) indicate that cumulative house-price growth has exceeded cumulative productivity growth, whereas negative changes indicate that productivity has grown more rapidly. This measure is intentionally narrow. It compares the relative growth of two indices and does not incorporate household income, rents, mortgage payments, tenure, dwelling quality, or the distribution of housing expenditure. It should therefore be interpreted as an index-growth comparison rather than an affordability ratio or welfare indicator.

The third contribution is methodological clarification of the relationship among the estimated equations. Because (P) is constructed as $(h-q)$, where (h) denotes the logarithm of the house-price index and (q) denotes the logarithm of labour productivity, the employment coefficient in the gap equation is algebraically connected to the employment coefficients estimated in the productivity and house-price equations. When the equations use the same observations, transformations, and fixed effects structure, the gap coefficient is implied by the two component models. Reporting this relationship improves interpretive transparency and provides an internal accounting check. It does not, however, constitute a separate empirical test or an additional independent confirmation of the employment–housing relationship.

The analytical scope further distinguishes within-city evidence from between-city description. Pooled cross-sectional patterns combine persistent differences among cities with temporal changes within them, whereas fixed-effects specifications rely on deviations from each city's own average after common year effects are considered. These two sources of evidence address different questions and should not be interpreted interchangeably. The pooled comparison for the common 2015-2021 period, for example, indicates that approximately one-third of the sampled cities experienced declining labour productivity. A widening price-productivity gap can therefore result from rapid house-price appreciation, weak or negative productivity growth, or both. Recognizing this decomposition prevents the gap from being interpreted automatically as evidence that economic gains have been capitalized into housing prices.

The scope of the claims is also constrained by the available variables. The dataset does not contain a consistent annual city-level measure of housing construction, regulatory restrictiveness, developable land, vacancy, or supply elasticity for all 35 FUAs. The analysis consequently cannot estimate whether the relationship varies systematically according to housing-supply conditions. For this reason, the study does not characterize the observed differences as uneven capitalization or attribute them directly to supply constraints. Instead, housing supply, credit conditions, expectations, and institutional differences are treated as plausible mechanisms requiring direct measurement and subsequent empirical testing.

1.3 Research questions

The analysis is organized around three research questions that move sequentially from the economic foundation of the argument to its housing-market and accounting implications:

RQ1: Are within-city changes in metropolitan employment associated with changes in labour productivity after persistent city characteristics and common annual shocks are taken into account?

RQ2: Are changes in metropolitan employment and labour productivity associated with changes in house prices within cities?

RQ3: How does the house-price-productivity gap decompose into the estimated employment-productivity and employment-house-price relationships when the component equations are based on the same sample and fixed-effects structure?

RQ1 evaluates the underlying employment-productivity association without treating employment as an exogenous measure of urban scale. RQ2 extends the analysis to the housing market by examining whether house-price changes co-move with employment and productivity changes. RQ3 then integrates the two relationships through the accounting definition of the gap. This sequence makes explicit that the gap equation is derived from the component relationships and should not be regarded as an independent causal model.

A complementary nonlinear specification examines whether the association between employment and the price-productivity gap is convex within the observed employment range of each city. Its purpose is descriptive: it assesses whether the estimated relationship changes across the available within-city employment distribution. The nonlinear term is not interpreted as evidence of a universal threshold, turning point, or structural acceleration mechanism because such claims would require stronger identification, broader within-city variation, and dedicated influence diagnostics.

Taken together, the research questions progress from the employment-productivity relationship to housing-price co-movement and, finally, to the accounting decomposition of the divergence between housing-price and productivity indices. This structure connects the theoretical motivation to the empirical design while maintaining a clear distinction between observed association, accounting identity, and causal explanation. The following conceptual framework formalizes these relationships and establishes the interpretive boundaries that guide the subsequent data, methods, and results sections.

2. Conceptual framework

2.1 Urban scale, productivity, and housing

Agglomeration theory explains the productivity advantage of large urban labour markets through three broad channels. Density allows firms and workers to share indivisible inputs, infrastructure, and supplier networks; it improves the matching of workers to jobs and of firms to specialized suppliers in thicker markets; and it accelerates the accumulation and diffusion of knowledge through more frequent interaction (Duranton & Puga, 2020; Puga, 2010). Measured productivity differences across cities, however, cannot be attributed to these channels alone. Observed productivity also reflects the sorting of more able workers into larger places, the selection of more efficient firms in more competitive markets, differences in industrial composition, and shocks whose incidence varies over time (Combes et al., 2008, 2012). Meta-analytic evidence indicates that estimated elasticities fall substantially once such sorting and composition effects are accounted for (Melo et al., 2009), and the field's longer retrospective assessment is that separating agglomeration from selection requires either linked micro-data or a credible source of exogenous variation in urban scale (Henderson & Thisse, 2024). Neither is available for the harmonized international panel assembled here.

For this reason, the conceptual relationship underlying the empirical work is written directly as a reduced form rather than as a production function that would ambiguously use both labour input and total employment as separate arguments:

$$q_{it} = a_{it} + \beta e_{it} + \zeta_{it} \quad (1)$$

where q is logging labour productivity, e is logging metropolitan employment, and a collects observed and unobserved productivity shifters, including sectoral composition, capital intensity, human-capital stocks, and national institutional context; ζ is an idiosyncratic disturbance. Employment enters this expression as a proxy for the scale of the local labour market, not as an exogenous treatment. The coefficient β is accordingly an association: it summarizes how productivity and employment move together within cities once persistent differences have been absorbed, and it should not be read as a structural agglomeration elasticity. City fixed effects remove time-invariant heterogeneity—geography, port or capital status, long-standing specialization, and country-level institutions—but two sources of contamination survive that transformation. First, sorting may change over time, so that the composition of a city's workforce shifts with its employment level. Second, causation runs in both directions, because productive cities attract workers and firms, making employment partly an outcome of the productivity it is used to explain. Both considerations imply that β is best interpreted as a descriptive co-movement parameter carrying an upward bias of unknown magnitude.

Spatial-equilibrium reasoning extends the same logic to housing. In these models, productivity, amenities, housing costs, and location choices adjust jointly, so that mobile households are indifferent at the margin between locations and the value of access to a productive, amenity-rich city is capitalized into land and housing prices (Glaeser et al., 2001; Rosen, 1974). How a given increase in housing demand divides between additional construction and higher prices depends on local supply conditions, while credit availability and price expectations can amplify and prolong the resulting cycles (Duca et al., 2021; Favara & Imbs, 2015; Glaeser et al., 2014). Regulatory restriction and physical geography are widely documented as sources of inelastic supply (Glaeser & Gyourko, 2018; Hilber & Vermeulen, 2016; Saiz, 2010), and constrained supply has in turn been linked to slower metropolitan employment growth and to the misallocation of labour across regions (Ganong & Shoag, 2017; Hsieh & Moretti, 2019; Saks, 2008). These mechanisms motivate the measurement exercise, but the present panel contains no annual, city-level indicator of regulatory stringency, developable land, or construction volume. Supply is therefore treated as a hypothesis available for future interaction tests rather than as a mediator identified in this design, and no part of the observed gap is attributed to it.

2.2 Accounting structure of the gap

Let h denote the log house-price index and define the measured gap as $P = h - q$, with both indices rebased to a common base year. Because P is constructed as a difference of two logarithms, its response to its two components is mechanical rather than estimated: a one-unit increase in h raises P one-for-one, and a one-unit increase in q lowers P one-for-one. Two implications follow, and both shape the

interpretation offered in the results. First, the gap is symmetric in its sources. It widens when house prices grow faster than before, and equally when productivity growth slows or turns negative; the measure itself cannot distinguish which of the two is responsible, so any statement about a widening gap must be accompanied by evidence on the behaviour of each component. Second, because P is a deterministic function of h and q , any regression of P on a set of covariates, estimated on the same observations and with the same fixed-effect projection, is an exact linear combination of the corresponding h and q regressions. The employment coefficient in the gap equation therefore contains no information beyond the two component equations from which it is built. This restriction is formalized in Section 3.2 and used there as a transparency check on the estimation, not as independent empirical confirmation of the pattern it describes.

The construction also bounds what the gap can be said to measure. P compares the cumulative growth of two indices; it is not an affordability ratio, because it contains no information on household incomes, rents, dwelling quality, or the changing composition of transacted units. Rebasing sets each city's gap to zero in the base year, so cross-city comparisons concern trajectories since that year rather than levels of housing cost or productivity. A city with persistently expensive housing and a stable index may therefore record a small gap, while a less expensive city with rapid recent appreciation records a large one.

Figure 1 summarizes this structure. It distinguishes the deterministic accounting links, whose coefficients are fixed at plus and minus one by construction, from the estimated associations between employment, productivity, and house prices, whose magnitudes are recovered from the panel. Unobserved supply conditions, credit availability, and expectations are placed outside the measured system to make explicit that they are acknowledged determinants of the outcomes rather than quantities identified in this study.

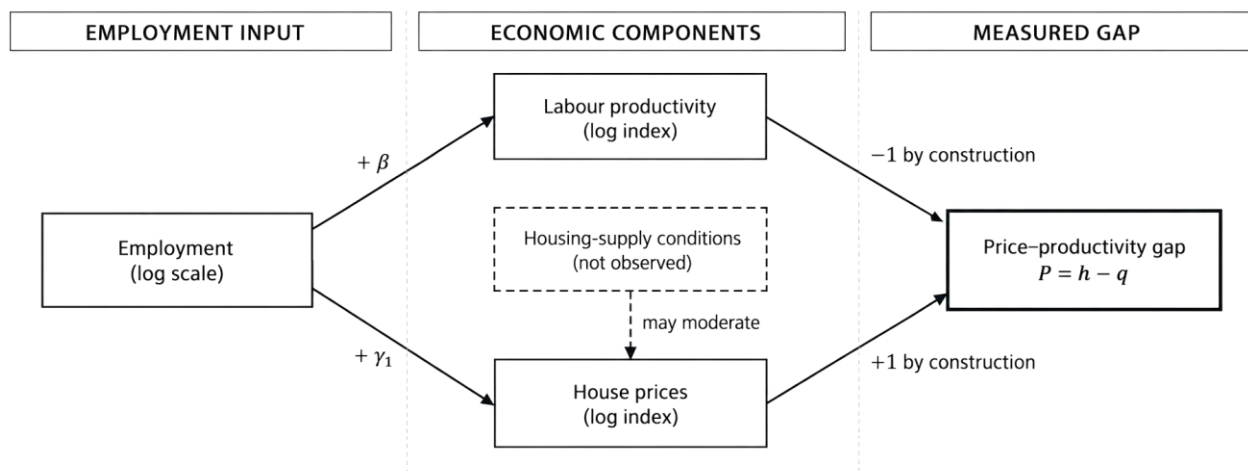


Figure 1. Conceptual and accounting structure of the measured gap.

3. Materials and methods

3.1 Design, sample, and sources

Building on the conceptual distinction between estimated associations and accounting identities, the empirical design uses the OECD FUA by year as its unit of analysis. The final unbalanced panel contains 600 observations for 35 FUAs in 11 countries between 2000 and 2023. Inclusion required matched city-level house-price indices, labour productivity, employment at place of work, GDP per capita, and 2015 population density. The intersection is a convenience sample of OECD cities with compatible series, not a random sample. No missing values were imputed.

Economic variables come from the Organisation for Economic Co-operation and Development (OECD) Functional Urban Areas—Economy data set (OECD, 2026a). Labour productivity is GDP per worker in US dollars at purchasing-power parity (PPP), employment is persons employed at place of work, and GDP per capita is PPP dollars per person. Population density is taken from the

corresponding FUA density data set (OECD, 2026b). The manuscript source metadata identify the PPP unit but do not fully resolve whether every archived release is expressed in current prices or common-year volume terms. Absolute monetary levels are therefore descriptive; the main gap uses rebased growth indices. A final replication archive should retain the exact unit, price-base, and vintage fields for every extracted series.

House prices come from the OECD National and Regional House Price Indices dataset (OECD, 2026c). The analysis selects annual OECD metropolitan house-price index observations referenced to 2015 = 100, untransformed, and not seasonally adjusted. The selected source rows, dwelling-coverage hierarchy, spatial crosswalks, and analytical transformations are documented in the accompanying replication package. The archived workflow should therefore be interpreted as analysing the OECD metropolitan house-price index used in the source export, rather than an independently reconstructed inflation-adjusted series.

When an all-dwelling series was unavailable, the longest available existing-dwelling or dwelling-type series was used under a prespecified hierarchy. That decision improves coverage but can introduce differential composition growth. City fixed effects and rebasing remove level differences; they cannot guarantee comparable quality adjustment or dwelling coverage. A homogeneous-coverage subsample should be added when the underlying series manifest and data are released. In the final 35-FUA panel, total dwelling-type coverage was used for 17 cities, including 4 total-vintage/total-dwelling series and 13 existing-dwelling/total-type series. Alternative dwelling-type series were used for 18 cities: 12 single-family and 6 multi-family series. The complete city-level series crosswalk is provided in the supplementary material.

To consolidate these definitions, Table 1 links each construct to its operational measure, source, and analytical role. In particular, the table distinguishes the measured outcomes from density, which is descriptive, and from P, which is a constructed growth comparison rather than an affordability indicator.

Table 1: Constructs, measures, and analytical roles.

Construct	Measure	Source	Role
Labour productivity	GDP per worker, USD PPP	OECD FUA Economy	Outcome; denominator of P
Employment	Persons employed at place of work	OECD FUA Economy	Time-varying scale proxy
GDP per capita	GDP per person, USD PPP	OECD FUA Economy	Alternative denominator
Population density	Persons per km ² , 2015	OECD FUA Density	Descriptive only
House-price index	OECD metropolitan HPI, 2015 = 100	OECD regional HPI	Outcome
Gap, P	ln(HPI/PROD), rebased	Author calculation	Principal constructed outcome

Note: FUA = functional urban area; GDP = gross domestic product; HPI = house-price index; PPP = purchasing-power parity. The selected HPI series used total dwelling-type coverage for 17 FUAs and single- or multi-family dwelling-type coverage for 18 FUAs; the full coverage crosswalk is reported in the supplementary material.

Having established the measurement framework, Table 2 reports the national composition and temporal coverage of the analytic panel. Its unequal country contributions and staggered observation windows explain why the subsequent estimates require cautious interpretation and explicit sensitivity analysis.

Table 2: Country coverage in the analytic panel.

Country	Cities	Observations	First year	Last year
Finland	4	34	2015	2023
France	4	64	2006	2021
Greece	2	34	2006	2022
Ireland	1	14	2010	2023
Italy	3	39	2010	2022
Korea	1	13	2010	2022
Lithuania	1	18	2006	2023
Netherlands	4	92	2000	2022
Norway	4	56	2008	2021
Slovenia	1	16	2008	2023
United States	10	220	2001	2022
Total	35	600	2000	2023

3.2 Variables and estimands

$$P_{it} = \ln(HPI_{it} / HPI_{i,2015}) - \ln(PROD_{it} / PROD_{i,2015}) \quad (2)$$

A value of $P = 0$ means cumulative HPI and productivity growth since 2015 are equal. $P = 0.20$ means the ratio of the two rebased indices is 22.1% above its base because $\exp(0.20) - 1 = 0.221$. P contains neither income, rents, mortgage costs, dwelling quality, tenure, nor distribution. It must not be called affordability.

Let $q = \ln(PROD)$, $h = \ln(HPI)$, and $e = \ln(EMP)$. The two-way fixed-effects models are:

$$q_{it} = \beta e_{it} + \alpha_i + \tau_t + \varepsilon_{it} \quad (3)$$

$$h_{it} = \gamma_1 e_{it} + \gamma_2 q_{it} + \alpha_i + \tau_t + u_{it} \quad (4)$$

$$P_{it} = \delta e_{it} + \alpha_i + \tau_t + v_{it} \quad (5)$$

The same observations and fixed-effect projection are used. By substitution and the Frisch–Waugh–Lovell theorem, Equation 5 is not an independent hypothesis test. Its employment coefficient must satisfy:

$$\delta = \gamma_1 + (\gamma_2 - 1)\beta \quad (6)$$

Using the reported estimates gives $1.424 + (0.886 - 1)(0.326) = 1.3868$, which equals the directly estimated $\delta = 1.387$ after rounding. Equation 5 is retained as an interpretable summary of the gap, not as a third independent result. A Wald test of $H_0: \gamma_2 = 1$ gives $t = (0.886 - 1)/0.278 = -0.41$ and $p = .68$. The data are compatible with the unit restriction, but the confidence interval $[0.320, 1.452]$ is wide. The nonlinear description replaces e with its city-demeaned value and square. Annual co-movement is examined in first differences:

$$\Delta h_{it} = \phi_1 \Delta q_{it} + \phi_2 \Delta e_{it} + \tau_t + \zeta_{it} \quad (7)$$

Because $\Delta P = \Delta h - \Delta q$, the first-difference pressure relationship is an exact transformation: $\Delta P = (\phi_1 - 1)\Delta q + \phi_2 \Delta e + \text{year effects and residual}$. It is not newly estimated and inherits the same standard errors for each transformed coefficient.

3.3 Estimation and inferential limits

Ordinary least squares includes FUA and year indicators in levels and year indicators in first differences. Reported standard errors are clustered by FUA (35 clusters), allowing within-city heteroskedasticity and serial correlation. They do not address dependence among cities exposed to the same national shocks. Country-by-year fixed effects absorb observed and unobserved shocks common to sampled cities within a country-year, but are not equivalent to country-level clustering. With only 11 countries, conventional country-cluster asymptotics are weak; country wild-cluster bootstrap inference would be preferable in the replication analysis.

The analysis does not report panel unit-root, cointegration, residual serial-correlation, cross-sectional-dependence, or slope-homogeneity tests. High level-model R^2 values and the difference between $\gamma_2 = 0.886$ in levels and $\phi_1 = 0.294$ in annual changes reinforce this concern. The interpretation therefore gives first differences primary weight for short-run co-movement and treats level equations as descriptive long-horizon sensitivities, not cointegrating relationships. Additional diagnostics and leave-one-city-out statistics require the analytical panel and covariance outputs.

Robustness checks change the sample window, exclude the United States, substitute GDP per capita for productivity, and use country-by-year fixed effects. They address different questions and are reported in separate panels. They do not solve simultaneity, measurement inconsistency, or cross-sectional dependence. The 2010-2021 window includes the pandemic and is not a pre-COVID test; common year effects absorb only shocks shared by all sampled cities.

3.4 Reproducibility

To support computational transparency, a complete replication archive accompanies this study. The archive reproduces the analytical panel from the archived OECD source files, verifies every reported regression coefficient, documents the provenance of all source series, preserves the selected metropolitan house-price series and spatial crosswalks, and archives the metadata, covariance validation, and robustness procedures used throughout the analysis. These materials enable independent verification of the reported findings and provide a transparent foundation for future extensions of the empirical framework.

4. Results

4.1 Descriptive evidence

The results begin with the distribution of the principal variables. Table 3 summarizes their pooled central tendency and dispersion across all available city-years, thereby providing scale and range information before correlations or fixed-effects estimates are considered.

Table 3: Descriptive statistics.

Variable	N	M	SD	Min	Median	Max
Labour productivity	600	120,957	25,853	57,764	118,672	268,752
Employment	600	2,243,857	2,734,737	103,537	1,058,258	13,448,561
GDP per capita	600	60,513	14,411	24,498	60,893	147,395
House-price index	600	106.53	27.72	48.87	101.91	218.70
Density (2015)	600	628	714	35	383	3,532
Price-productivity gap	600	0.053	0.203	-0.511	0.037	0.790

Note: Monetary measures are USD PPP as reported by OECD. Density is persons per km². HPI is 2015 = 100. The gap is a natural-log difference.

Mean productivity is USD PPP 120,957 per worker; median employment is 1.06 million. The gap averages 0.053 and ranges from -0.511 to 0.790. These pooled values combine permanent city differences, common time trends, and within-city changes. They should not be read as fixed-effects estimates.

As a complementary descriptive step, Table 4 reports pooled Pearson correlations among the six variables. These coefficients reveal the raw co-movement structure, while also showing why the mechanically strong HPI-gap association and the near-zero employment-gap correlation cannot substitute for the within-city models.

Table 4: Pooled Pearson correlations.

Variable	1	2	3	4	5	6
1. Productivity	—					
2. Employment	.284	—				
3. GDP per capita	.885	.164	—			
4. House-price index	.114	-.041	.194	—		
5. Density	-.089	.340	.010	.127	—	
6. Gap	.040	-.033	.134	.947	.164	—

Note. N = 600. The .947 correlation is partly mechanical because HPI enters the gap.

Figure 2 avoids the original dual-axis comparison by plotting a single quantity on one scale. From 2015 to 2021, the mean gap rises in all employment terciles. The underlying 2021 mean HPI values

are approximately 135.9-144.2, while mean productivity indices are 100.8-107.1. Tercile averages are descriptive and do not establish that larger cities have steeper within-city responses.

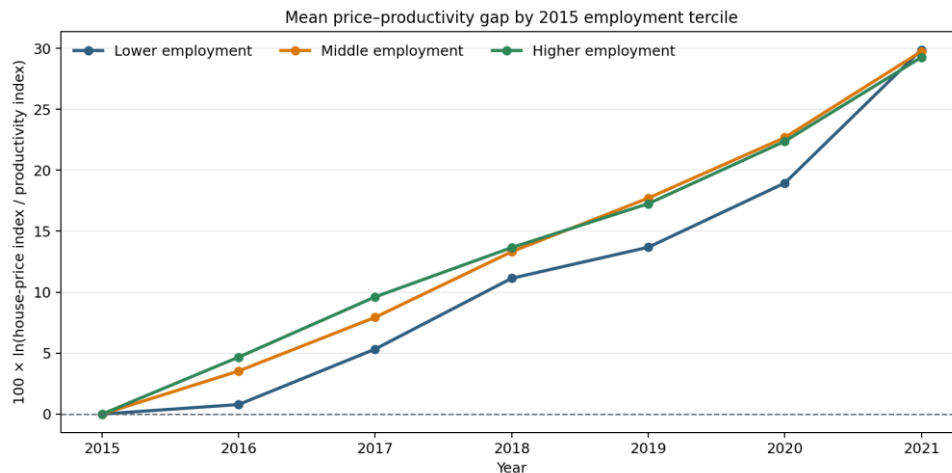


Figure 2. Mean price–productivity gap by 2015 employment tercile, 2015–2021.

Note: Each series is an unweighted mean of city indices within a 2015 employment tercile. Values are calculated from the group-mean HPI and productivity indices reported in the original analysis. The common window contains all 35 cities.

Figure 3 explains why the time-series and cross-sectional statements must be separated. Most points are above the 45-degree line, but the cloud does not display a strong positive between-city relationship between productivity growth and house-price growth. Approximately one third of cities have negative productivity growth over 2015-2021, so a positive gap sometimes reflects stagnating productivity rather than an “economic gain” being capitalized. Dutch cities show large positive gaps, while Dublin sits below equal growth because productivity grew faster than its house-price index. Labels have been repositioned to avoid overlap.

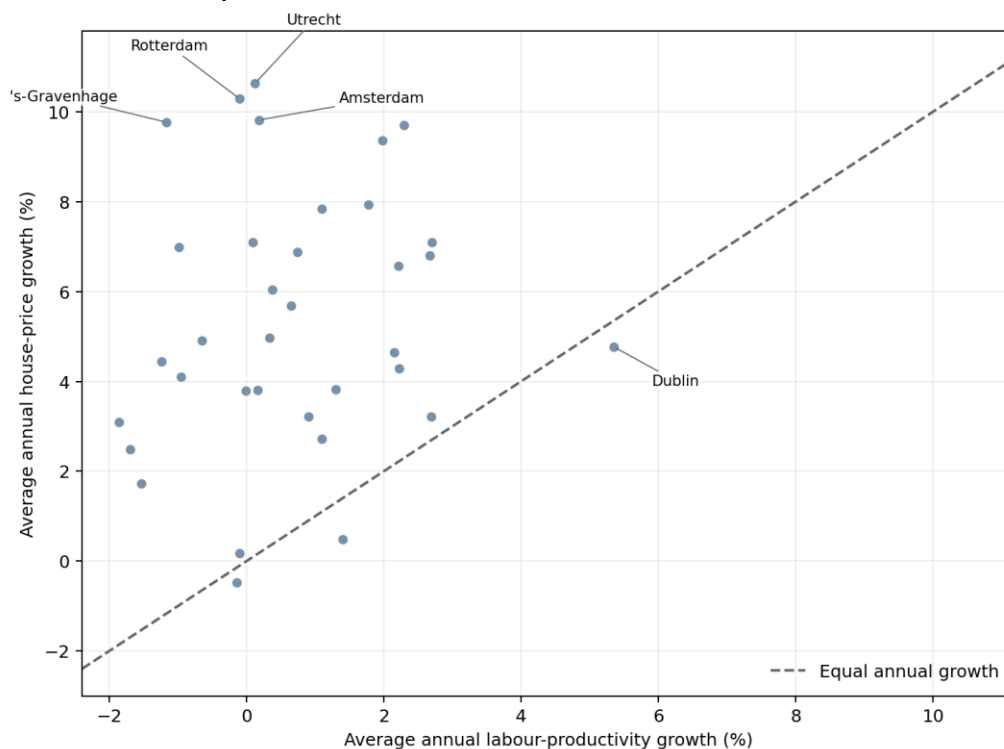


Figure 3. Average annual house-price and labour-productivity growth, 2015–2021.

4.2 First-difference evidence

Annual changes provide the most defensible evidence in the reported specifications because differencing removes city levels and reduces the influence of deterministic trends. In Model 5, ϕ_1 for productivity growth is 0.294 (SE = 0.104, 95% CI [0.083, 0.505], $p = .008$), and ϕ_2 for employment growth is 1.436 (SE = 0.161, 95% CI [1.109, 1.764], $p < .001$; $N = 565$). House-price growth therefore co-moves positively with both contemporaneous productivity and employment growth after common year shocks are removed.

The exact transformed pressure equation has a productivity-growth coefficient of $-0.706 (= 0.294 - 1$; SE = 0.104, 95% CI $[-0.917, -0.495]$) and an employment-growth coefficient of 1.436 (SE = 0.161). The negative productivity coefficient is partly definitional because productivity is subtracted from house prices. These annual associations remain vulnerable to simultaneity, national dependence, measurement error, and pandemic-era heterogeneity; they are not causal elasticities.

4.3 Levels estimates and accounting decomposition

With the descriptive patterns established, Table 5 assembles the reported regression results in a common format. Reading across its columns connects the employment-productivity model, the house-price model, the algebraically implied gap equation, the nonlinear description, and the first-difference benchmark without treating them as independent confirmations.

Table 5: Reported fixed-effects and first-difference estimates.

Term	M1: q	M2: h	M3: P	M4: Nonlinear gap model	M5: Δh
Log employment	0.326* (0.158)	1.424*** (0.174)	1.387*** (0.192)	—	—
Log productivity	—	0.886** (0.278)	—	—	—
Demeaned log employment	—	—	—	1.334*** (0.191)	—
Squared demeaned employment	—	—	—	3.056* (1.258)	—
Δ log productivity	—	—	—	—	0.294** (0.104)
Δ log employment	—	—	—	—	1.436*** (0.161)
Observations	600	600	600	600	565
Adjusted R ²	.931	.802	.702	.716	.441

Note: Standard errors clustered by FUA are in parentheses. M1–M4 include FUA and year fixed effects; M5 includes year effects. M3 is algebraically implied by M1 and M2, within rounding. Wald test of $H_0: \gamma_2 = 1$: $t = -0.41$, $p = .68$. * $p < .05$. ** $p < .01$. *** $p < .001$.

In Model 1, $\beta = 0.326$ (SE = 0.158, 95% CI [0.005, 0.647], $p = .047$). A 10% employment increase corresponds to $0.326 \ln(1.10) = 0.0311$ log points, or 3.16% higher productivity. Because the result is marginal under city clustering and alternative national or cross-sectional dependence corrections are unavailable, it is suggestive rather than confirmatory. The coefficient is also large relative to

conventional agglomeration estimates because it includes sorting, composition, and reverse causality (Melo et al., 2009).

Model 2 gives $\gamma_1 = 1.424$ (SE = 0.174) and $\gamma_2 = 0.886$ (SE = 0.278). Holding measured productivity constant, a 10% employment increase corresponds to 0.1357 log points, or 14.54% higher HPI. The implied reduced-form employment coefficient for h is $\gamma_1 + \gamma_2\beta = 1.713$; its 10% contrast is 0.1633 log points, or 17.75%. A standard error for this constructed quantity cannot be recovered without the covariance matrix, so no significance claim is made. The magnitude is unusually large and should not be called a structural demand or supply elasticity.

Model 3 gives $\delta = 1.387$ (SE = 0.192). For a 10% employment increase, the change in P is $1.387 \ln(1.10) = 0.1322$ log points. Equivalently, the HPI/productivity index ratio is 14.13% higher because $\exp(0.1322) - 1 = 0.1413$. Reporting both units prevents an elasticity from being confused with a percentage level change. The estimate is the accounting decomposition in Equation 6, not independent support for a third hypothesis.

The nonlinear squared term is positive (3.056, SE = 1.258, 95% CI [0.499, 5.614], $p = .021$). It describes convexity within the observed support, but may be sensitive to leverage and limited within-city variation. Without influence diagnostics and leave-one-city-out estimates, no universal threshold or acceleration mechanism is claimed.

4.4 Robustness and sensitivity

Finally, Table 6 examines whether the employment–gap coefficient retains its sign under alternative measurement, sample, and fixed-effect choices. The comparison is informative about sensitivity, although the substantial variation in magnitude prevents a claim of coefficient stability.

To examine whether the full-sample association is driven by pandemic-period observations, an additional pre-2020 specification was estimated using observations through 2019. The employment–gap coefficient remains positive and statistically significant (1.447, SE = 0.187, 95% CI [1.067, 1.826], $N = 498$), indicating that the principal association is also present before the COVID-19 period.

Table 6: Sensitivity of the employment–gap coefficient.

Panel / specification	Coefficient	SE	95% CI	N	Adj. R ²
A. Measurement: GDP-per-capita denominator	1.158***	0.188	[0.777, 1.540]	600	.629
B. Sample: pre-2020 window, 2000–2019	1.447***	0.187	[1.067, 1.826]	498	.643
B. Sample: 2010–2021 window	1.920***	0.320	[1.269, 2.571]	400	.657
B. Sample: exclude United States	1.967***	0.479	[0.979, 2.956]	380	.649
C. Fixed effects: country $\times$ year	1.289***	0.222	[0.836, 1.741]	539	.856

Note: Models include FUA fixed effects and year or country $\times$ year fixed effects. Standard errors are clustered by FUA.

All reported coefficients are positive, but their range 1.158 to 1.967 is substantively wide. The added pre-2020 specification gives an employment–gap coefficient of 1.447, close to the full-sample estimate of 1.387, suggesting that the principal association is not dependent on pandemic-period observations. Stability of sign should nevertheless not be described as stability of magnitude. The GDP-per-capita denominator changes the construct; excluding the United States changes sample composition; and country-by-year effects change the identifying comparison. These tests are informative but do not substitute for consistent dwelling coverage, pandemic-interaction models, national-cluster bootstrap inference, residual-dependence tests, or leave-one-city-out analysis.

5. Discussion

5.1 What the estimates establish

Taken together, the tables and figures establish a descriptive pattern: annual house-price growth co-moves with employment and productivity growth, and the rebased HPI frequently rises faster than the productivity index. The levels estimate describe a similar within-city pattern after common year

shocks, but are exposed to persistence and common national trends. The gap coefficient is a useful summary of the two modeled relationships; it is not an independent empirical pillar.

The γ_1 estimate of 1.424 is unusually large. Plausible contributors include simultaneous migration and job growth, expectations, credit conditions, composition of transacted dwellings, local amenities, and slow construction. Recent work similarly emphasizes that urban housing relationships change across periods and metropolitan contexts (Hoxie et al., 2023; Oikarinen et al., 2023). The present model cannot apportion the coefficient among these channels. Calling it “supply-constrained capitalization” would therefore exceed the data.

5.2 Within-city and between city evidence

The near-zero pooled employment-gap correlation and the fixed-effects coefficient answer different questions. The former mixes permanent cross-city differences and time variation; the latter uses deviations within each FUA after year effects. Figure 3 is also a between-city comparison of six-year average growth, not a visualization of the regression coefficient. Its diffuse cloud is compatible with substantial heterogeneity and with common drivers that vary across countries. This distinction should be maintained in any presentation of the findings.

The analysis does not report within- and between-city standard deviations of the logged regressors. Those quantities are necessary to judge how much identifying variation remains after demeaning and should accompany the next replication release. Likewise, slope-homogeneity tests would assess whether a single elasticity is an adequate summary across countries and housing systems.

5.3 Pandemic and remote-work period

The endpoint extends through 2023, and the robustness window extends through 2021. COVID-19 affected remote work, migration, household space demand, credit conditions, and the valuation of central locations (Gupta et al., 2022). Year effects absorb a global average shock but not country or city-specific exposure. Country-by-year effects help with national shocks but do not isolate metropolitan remote-work intensity. The estimates therefore average pre-pandemic and pandemic-era relationships. The added pre-2020 sensitivity specification partly addresses this concern by showing that the principal employment-gap association is also present before 2020. Further work should examine explicit interactions for 2020–2023, with inference robust to country-level dependence.

5.4 Policy relevance without causal overreach

The findings support joint monitoring rather than a policy treatment effect. Metropolitan dashboards should track productivity, employment, house prices, rents, construction, vacancies, rent burden, commute accessibility, and household income together. A rising P can flag that HPI appreciation is outpacing productivity, but it cannot diagnose regulation, affordability, or welfare.

Housing-supply and transport reforms may change how local demand is absorbed, as prior causal and quasi-experimental studies indicate (Gibbons & Machin, 2005; Glaeser et al., 2005; Hilber & Vermeulen, 2016; Saiz, 2010; Saks, 2008). Whether such mechanisms explain this sample requires direct interactions with permits, completions, land-use regulation, geographic constraints, and accessibility. Policy recommendations should therefore be conditional: if local diagnosis confirms inelastic supply, aligning land-use capacity, infrastructure, and housing delivery with employment growth is a plausible response. Distributional evaluation must distinguish owners from renters, incumbents from entrants, and income groups.

6. Limitations and research priorities

These interpretations must nevertheless be read against several limitations. First, the analysis is associational. Employment, productivity, migration, and house prices are jointly determined, and the fixed effects do not supply exogenous variation. Second, the level series may be nonstationary or cointegrated; those properties have not been tested. Third, FUA clustering does not address dependence among cities in the same country, and only 11 countries are observed. Fourth, heterogeneous slopes and pandemic effects are not estimated. Fifth, the unbalanced sample and mixed

dwelling coverage can create selection and measurement differences. Sixth, PPP price-base metadata and deflators need to be archived explicitly. Seventh, the gap is not household affordability and contains no rents, mortgage costs, incomes, tenure, or distribution.

Priority robustness work is therefore concrete: report within/between variation; panel unit-root and cointegration diagnostics where appropriate; residual serial-correlation and cross-sectional-dependence tests; country wild-cluster bootstrap or Driscoll-Kraay sensitivity; further pre-/post-pandemic comparisons and pandemic-interaction models; homogeneous HPI coverage subsamples; leave-one-city-out estimates; and interactions with directly measured supply conditions. These additions require the analytical panel and covariance outputs and cannot be reconstructed from rounded tables alone.

External validity is limited to the 35 sampled OECD FUAs, with the United States providing more than one third of observations and several countries represented by one city. The results should not be generalized to informal housing markets, rapidly urbanizing lower-income cities, or different land and credit institutions without replication.

7. Conclusions

This study examined the relationships among employment growth, labour productivity, and house-price dynamics across 35 OECD functional urban areas between 2000 and 2023. By integrating these variables within a harmonized city-year panel, the analysis provides a transparent descriptive framework for assessing whether metropolitan house-price growth systematically exceeds productivity growth. The resulting price productivity gap should be understood as an indicator of relative index growth rather than a direct measure of housing affordability, household welfare, or supply-constrained capitalization.

The empirical findings reveal consistent positive co-movement among employment, productivity, and house-price indicators. The first-difference estimates provide the most defensible short-run evidence, showing that annual house-price growth is positively associated with contemporaneous employment and productivity growth after common year effects are considered. In the levels specification, a 10% increase in employment corresponds to an estimated 0.132-log-point increase in the house-price–productivity gap, equivalent to approximately a 14.1% increase in the ratio of the two indices. However, this coefficient represents an algebraic decomposition of the productivity and house-price equations and should not be interpreted as an independent hypothesis test.

These findings indicate that metropolitan economic expansion and housing-market appreciation are closely connected, although their relative rates of adjustment vary substantially across cities. A widening gap may reflect rapid house-price appreciation, weak productivity performance, or a combination of both processes. Consequently, the indicator is most useful as a diagnostic signal that can identify cities requiring more detailed investigation. Meaningful policy interpretation therefore requires complementary evidence on housing supply, land-use regulation, construction activity, household income, rents, mortgage costs, accessibility, and the distribution of housing burdens across different population groups.

Overall, the study establishes a comparative empirical benchmark rather than a causal estimate of agglomeration benefits or housing-market capitalization. The results remain subject to simultaneity, no stationarity, cross-sectional dependence, heterogeneous housing-price coverage, and pandemic-related structural change. Future research should combine harmonized metropolitan data with direct measures of housing-supply conditions, pre- and post-pandemic specifications, country-dependence-robust inference, and household-level distributional outcomes. Such extensions would clarify the mechanisms underlying the observed gap and strengthen the evidence required for context-sensitive urban economic and housing policies. Beyond its substantive findings, the study demonstrates the value of transparent, reproducible metropolitan analysis for strengthening comparative urban research and supporting future investigations of housing-market adjustment across cities.

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Not applicable. This study used publicly available aggregate secondary data and involved no human participants, personal data, or identifiable individual-level information.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data Availability Statement

This study uses publicly available OECD datasets. The archived analytical dataset, metadata audit, computational analysis scripts, and replication materials supporting the reported findings are available from the corresponding author upon reasonable request. The underlying OECD datasets are publicly available through the OECD Data Explorer.

CRedit Author Statement

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