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**Working Paper**

## Mind the Lag: Using Assessed and List Prices as Proxies for Housing Market Values

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# Mind the lag: Using assessed and list prices as proxies for housing market values\*

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## Abstract

Property transaction prices are widely regarded as the best measure of property value, but are sometimes unavailable. Using data from the Netherlands and New York, we analyze whether list prices and assessed values are reliable substitutes. In the cross-section, both proxies strongly predict sales prices, and estimated hedonic implicit prices resemble those based on sales prices. Over time, there is a sluggish adjustment in both proxies, but much more so in assessed values—particularly when they are based on rental incomes. While assessed values are well-suited for cross-sectional hedonic modelling or the quantification of static quantitative spatial models, list prices are better suited for the estimation of hedonic implicit prices from variation over time, although some attenuation bias should be expected.

**Keywords:** assessed values, list prices, sales prices, transaction prices, hedonic pricing, historic amenities, wind turbines, solar farms, transit accessibility.

**JEL:** R31, C21, C23

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\*We thank NVM (Dutch Association of Real Estate Agents) for providing the microdata on house transactions in the Netherlands.

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# 1 Introduction

Hedonic valuation is a crucial tool in spatial economics and economic geography for assessing the value of non-market goods such as clean air, safety, or public school quality, just to name a few (Greenstone, 2017). In the growing field of spatial quantitative economics, mix-adjusted house price indices are also important as they, along with wages and the spatial distribution of employment and population, determine local amenity and productivity levels (Redding, 2023). While it is consensual that observed property transaction prices (henceforth sales prices) present good approximations of market value, such data are not always readily available, forcing researchers to rely on substitutes. The most widely used are list prices, which can sometimes be scraped from websites, and assessed values, which are often collected for tax purposes.<sup>1</sup> Little is known about the bias the use of these proxies may introduce into the estimation of hedonic implicit prices and the measurement of (dis)amenity values.

Our contribution is to exploit matched property data to examine whether and when list prices and assessed values serve as reliable proxies for sales prices. Previewing our results, we find that cross-sectionally, list prices and assessed values strongly predict sales prices. Consistent with this, we find that hedonic implicit prices are very similar to those estimated from sales prices in several applications of hedonic valuation that we borrow from the literature. This result is expected insofar as classical measurement error in the dependent variable should affect efficiency, but not consistency. However, we also find that measurement error along the time dimension is non-classical. There is a sluggish adjustment in list prices and, even more so, assessed values. Consistent with this, our applications reveal that estimated implicit prices from popular difference-in-differences strategies are generally smaller when using list prices or assessed values. For list prices, the point estimates are typically 10-30% lower. For assessed values, the point estimates are even lower in relative terms, or even flipping signs when the assessments are derived from methods other than transaction-based valuation. We conclude that assessed values are feasible proxies when the analysis is cross-sectional or the comparison is made over a longer time (such as several decades). While the use of list prices may also be justifiable in typical difference-in-differences settings where comparisons are made over a couple of years, we caution against their application in high-frequency settings.

The ideal dataset for the purpose set out above contains sales prices, list prices, and assessed values matched at the level of individual properties. To enhance the external validity of this study, we rely on two such datasets from two countries. First, we include

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<sup>1</sup>See Table A1 for more details on housing studies that use other price measures, such as list prices, assessed values, or owner-estimated values.

data from the Netherlands, covering approximately 3 million sales transactions since 1991, including both sales prices and list prices. Additionally, we collected the universe of assessed housing values from 2014 onwards, approximately 64 million observations, by web-scraping. Furthermore, we utilized data from New York City, which spans from 2003 to 2023 and includes information on sales prices of 626 thousand transactions, a subset of which also includes list prices, and assessed values.<sup>2</sup>

We begin our empirical analysis by examining the relationship between list prices, assessed values, and sales prices through the decomposition of each price measure into cross-sectional and temporal components. Our first stylized fact highlights the high cross-sectional correlation between sales prices and list prices, with an  $R^2$  of almost 1 ( $R^2 > 0.96$ ) in the Netherlands and New York. For assessed values, the  $R^2$  is still high as long as assessments are based on market transactions ( $R^2 > 0.85$ ), although somewhat smaller when assessments are based on rental incomes (0.74, for New York). Since list prices and assessed values typically scale in sales prices at elasticities close to one, using cross-sectional variation in list prices or assessed values is likely to yield hedonic estimates very similar to those obtained when using variation in sales prices.

The correlation between sales and list prices remains high when focusing on long-run temporal variation (*i.e.*,  $R^2 > 0.94$ ). The  $R^2$  for the temporal correlation of assessed values is similarly high when assessments are transaction-based, but somewhat lower when they are rental income-based (*i.e.*,  $R^2 = 0.71$  in New York). In the short run, however, notable lags emerge. List prices take at least one year to fully reflect changes in sales prices. At the monthly frequency, list prices barely respond to contemporaneous changes in sales prices. This sluggish adjustment is consequential for the measurement of the housing cycle: List prices are suitable for identifying the years marking the beginning and end of a recession, but not necessarily the specific months. Transaction-based assessed values (in both the Netherlands and New York) take 1-2 years to fully adjust to shocks capitalized in sales prices. Income-based appraisals in New York show a relatively strong contemporaneous response but still fall short of full capitalization, even in the longer run.

The weaker temporal correlation between sales prices and their substitutes suggests that the use of proxies is more likely to affect hedonic estimates in research designs that exploit the panel dimension. This is a potential concern since panel approaches, such as difference-in-differences, are becoming increasingly popular due to their ability to control for unobserved spatial heterogeneity. To put this intuition to the test, we replicate and extend three existing hedonic studies that originally used sales prices. Our contribution is to repeat the estimation of the central specifications using sales prices, list prices, and

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<sup>2</sup>For the sake of transparency, we acknowledge that our choice of these two countries was primarily driven by data availability. We also note that the data quality from New York City is somewhat lower than that of the Dutch data, with a higher share of missing data on list prices.

assessed values, in the latter case considering the matched sample and the universe in separate regressions.

The first application concerns the valuation of views of historic amenities in [Koster et al. \(2016\)](#). This study relies on a cross-sectional comparison of properties that have views of historic amenities, and buildings just around the corner that do not have these views. Just like in the original study, we find a positive and statistically significant capitalization effect of views on sales prices of about 3%. When replacing sales prices with list prices and assessed values as the dependent variable, we find that the resulting estimates are close to and not statistically different from those based on sales prices. This result holds across a range of model specifications, confirming our prior that we should recover similar hedonic implicit prices from alternative price measures when the identifying variation is cross-sectional.

In the second application, we turn to the effect of wind turbines on property prices in the Netherlands in [Dröes and Koster \(2016, 2021\)](#). Here, the capitalization effect is identified using a difference-in-differences comparison of prices before and after the opening of a facility at locations close to and further away from the facility. We estimate negative capitalization effects on sales prices ranging from 1.2 to 2.6% for wind turbines. When using list prices, the point estimates drop by about 20% on average. When using assessed values, the point estimates similarly drop in half of the matched-sample specifications and even approach zero in all specifications using the full sample.

In the third application, we replicate the analysis of the impact of the Second Avenue Subway extension in New York City on house prices in [Gupta et al. \(2022\)](#). Again, the capitalization effect is identified using a difference-in-differences comparison of prices before and after the extension in areas near and farther from the new line segment. In the preferred specification that controls for anticipation effects, we estimate a 5.8-6.8% increase in sales prices. Using list prices, the point estimate drops by about 30%. When using assessed values, the estimated capitalization effect becomes insignificant (transaction-based appraisal) or even switches sign (income-based appraisal), whether we use the matched sample or the universe of assessed values. The absence of statistically significant positive capitalization effects is counterintuitive and inconsistent with a large literature on railway station capitalization effects ([Debrezion et al., 2007](#)).

Taken together, our findings have important implications for at least three strands of policy-relevant literature. First, there is a large literature that infers the value of non-marketed goods from variation in property prices as a central input for the cost-benefit analysis of public goods provision. This literature combines the insight that hedonic implicit prices can be recovered from observed prices and characteristics of composite goods ([Rosen, 1974](#)) with the spatial equilibrium prediction that differences in the amenity

values of locations must capitalize into property prices (Roback, 1982). Applications are countless and include the valuation of clean air (Chay and Greenstone, 2005) and soil (Greenstone and Gallagher, 2008), public school quality (Cellini et al., 2010), safety (Linden and Rockoff, 2008), broadband infrastructure (Ahlfeldt et al., 2017), open spaces (Koster, 2023),<sup>3</sup> architectural amenities (Ahlfeldt and Holman, 2018), or place-based policies (Koster and Van Ommeren, 2019). Our results inform this literature in that list prices and assessed values prove to be reliable substitutes when sales prices are not available for hedonic valuation in the cross-section. However, more care is warranted in the application of difference-in-differences designs, which have become increasingly popular in this literature as they mitigate omitted variable bias from spatial confounders. Such research designs should ideally be applied to sales prices. Assessed values—and especially those based on rental incomes instead of market transactions—should be avoided if at all possible.

Second, quantitative spatial models (QSMs) have emerged as flexible tools for the evaluation of spatial shocks and policies since they account for spatial linkages between regional goods and factor markets. QSMs have, for example, been employed to evaluate the importance of agglomeration spillovers (Ahlfeldt et al., 2015), employment responses to local productivity shocks (Monte et al., 2018), or transport improvements (Heblich et al., 2020; Tsivanidis, 2025; Severen, 2021). We refer to Redding and Rossi-Hansberg (2017) and Redding (2023) for comprehensive reviews. These models rationalize the spatial distribution of economic activity via structural fundamentals capturing productivity and amenity values, whose quantification requires reliable measures of property value. Given the emphasis on structural fundamentals, there is a natural overlap with the above literature on the valuation of local public goods since their value will be captured by the amenity fundamentals of a QSM. For example, Diamond (2016) analyses amenity preferences to explain the sorting of high-skilled workers into high-rent cities. Koster (2023) uses the amenity fundamental to assess the economic effects of urban growth boundaries. Ang et al. (2024) correlate fundamental amenity values recovered from a QSMs with observed measures to understand group-specific preferences for various locational amenities. One challenge that arises in the quantification of QSMs at small spatial scales such as zip codes is that property transactions data may be sparse, leaving researchers with insufficient observations to compute a reliable price index. One strategy to address this problem is to use spatial interpolation techniques and rely on nearby transactions, adjusting for spatial trends (Ahlfeldt et al., 2023). Our results indicate that assessed values—which frequently provide broader, if not comprehensive,

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<sup>3</sup>See also Irwin (2002); Anderson and West (2006); Albouy et al. (2020) for related analyses of the effects of parks.

spatial coverage—constitute a viable alternative for quantifying static QSMs in which fundamentals are inverted in the cross section. List prices also represent a feasible substitute to sales prices, although they may be similarly sparse as sales prices and, thus, require spatial interpolation. Given the sluggish adjustment over time, we caution, however, against the use of assessed values in the quantification of dynamic spatial models (Caliendo et al., 2019; Kleinman et al., 2023; Greaney et al., 2025).

Third, our paper contributes to a literature that compares the evolution of sales prices to assessed values and list prices. Darling (1973) provides early empirical evidence using both assessed and market values to estimate the benefits of urban water parks, finding broadly consistent estimates in the majority of specifications. Malaitham et al. (2020) show that in Bangkok, assessed land values systematically underestimate market values and respond differently to property attributes due to lagged and simplified valuation methods. Similarly, Cotteleer and van Kooten (2012) find that assessed values in the UK deviate from sales prices, especially in capturing location-specific effects, due to institutional smoothing and standardized procedures. Both studies find that hedonic regressions using assessed values yield coefficient estimates that are broadly similar in sign and magnitude to those obtained using sales prices, which is a result we also find in our cross-sectional analysis. However, these studies primarily focus on spatial variation. Our study extends this literature by additionally examining temporal differences between assessed values and sales prices, providing the implications for panel-based identification strategies.

Haurin et al. (2013) show that in markets characterized by a downturn or normal conditions, list prices typically exceed the sales prices. Conversely, in a booming housing market, homes often sell for amounts that exceed their list prices. This observation supports the idea that sellers tend to adopt an auction-like approach during housing booms (see Albrecht et al., 2016; Koster and Rouwendal, 2024). In addition, there is evidence that list prices are sticky during market downturns (Haurin et al., 2013; Lyons, 2019). Knight et al. (1994) is, perhaps, closest to our analysis. They show that hedonic price estimates based on list prices data are comparable to those derived from transaction data, although the quarter-to-quarter changes in list and sales prices diverge. Our study further explores the long-run adjustment of list prices to sales prices, how short-run responses differ at multiple frequencies, and whether list prices can capture the change of the housing market cycle.

The rest of the paper is organized as follows. First, in Section 2 we introduce the data. Section 3 analyzes relationships between list prices, assessed values, and sales prices. In Section 4 we describe three replications with alternative dependent variables. Section 5 concludes.

TABLE 1 – DESCRIPTIVE STATISTICS - THE NETHERLANDS

	N	Mean	S.d.	Min	Max
<b>Panel A: Full sample</b>					
Sales price/m <sup>2</sup> (in €)	3,287,365	2,293	1,158	100	33,333
List price/m <sup>2</sup> (in €)	3,287,365	2,368	1,125	99	33,333
Assessed value/m <sup>2</sup> (in €)	63,800,824	2,312	1,199	50	98,707
<b>Panel B: Matched sample</b>					
Sales price/m <sup>2</sup> (in €)	1,305,612	2,747	1,293	144	29,545
List price/m <sup>2</sup> (in €)	1,305,612	2,755	1,207	177	29,545
Assessed value/m <sup>2</sup> (in €)	1,305,612	2,533	1,138	70	36,440

*Notes:* In the full sample, sales price data are available for the period 1991-2022, list price data for 1980-2022, and assessed value data for 2014-2022. The matched sample covers the period 2014-2022.

## 2 Data and descriptives

**The Netherlands.** We use NVM (Dutch Association of Real Estate Agents) micro-data on house transactions from 1991 to 2022. This provides information on around 80% of transactions in the Netherlands. Data for each transaction includes the sales price, list price, the exact location, and a wide variety of house characteristics such as size, number of rooms, construction year, type of house, date of listing and date of transfer deed, etc. Listing dates are available as far back as 1980. A few outlier observations are excluded.<sup>4</sup>

The assessed value (which is called “WOZ value” in the Netherlands) data is web-scraped from the website of the Dutch Land Registry (the *Kadaster* in Dutch) and provides information on assessed values from 2014 to 2022. It is determined by each municipality for residential properties. The municipality collects the market value of comparable residential properties that are sold around the valuation date, which is usually around 1 January. It then takes differences in housing attributes into consideration and uses a valuation model to estimate the property value (Ministerie van Algemene Zaken, 2014). The assessed values are maintained up to current and re-established every year, therefore, it is believed to be in line with housing market sales prices, but with a lag since it is assessed only once per year and there might be limited number of nearby transactions within the assessment period. Outlier observations in this dataset are also excluded.<sup>5</sup> We conduct several analyzes using the “matched” dataset, which contain transactions with information on sales prices, list prices and assessed values, spanning from 2014 to 2022. The NVM micro-data and assessed values can be linked together by using the unique address identifier. Table 1 reports the main descriptive statistics.

<sup>4</sup>Transactions with sales prices that are above €10 million or below €10,000 or a price per m<sup>2</sup> below €100 or above €100,000 are excluded. Properties that are larger than 2,500 m<sup>2</sup> or smaller than 25 m<sup>2</sup> or have more than 25 rooms are also removed.

<sup>5</sup>Assessed values above €10 million, below €10,000, or with a price per m<sup>2</sup> below €50 or above €100,000 are excluded. We also exclude observations where the assessed values are more than twice as low as in either the preceding or the following year.

The average list and sales prices are lower in the full sample spanning 1991-2022 because prices have increased strongly since the 1990s. In the matched sample (Panel B), list prices are 1.2% higher than sales prices. The past 10 years can be characterized as a seller’s market, in which sales prices frequently exceeded list prices, due to bidding wars (see [Koster and Rouwendal, 2024](#), for more details), which is why the list prices are only a little higher. At the same time, because the past decade is also characterized by high house price growth, and because the assessed values are likely lagged, the mean assessed values is 8% below the average of sales prices.

**New York City.** Data of housing transactions is web-scraped from MarketProof. The data covers transactions in New York City during 2003-2023. The data also includes sales prices, list prices, transaction date, listing date, property characteristics such as location, size, floor, year of construction, type of property, etc. Outlier observations are excluded.<sup>6</sup>

The assessed values from 2007 to 2023 are obtained from the NYC Department of Finance (DoF). They provide a yearly dataset containing the assessed market value of every building within this time period. Buildings are valued based on their condition on the valuation date January 5. For Tax class 1 (one- to three-unit buildings), valuations rely on comparable sales data from the neighborhood in the prior three years. For Tax classes 2 and 4—buildings with four or more residential units, including condominiums and mixed-use buildings with at least some residential floor space—assessed values are derived using the income capitalization method, which estimates property value from the income-generating potential of comparable rental buildings in the preceeding year, even if the property itself is not rented out. We exclude Tax class 3 properties, which are utilities buildings, from the analysis. The information about market value before 2007 is obtained from the annual Primary Land Use Tax Lot Output (PLUTO) data. While they do not provide directly the assessed market value, that information can be recovered by dividing the assessed taxable value by 6% or 45% depending on the tax class ([NYC Department of Finance, 2024](#)). Measurement errors are corrected by extrapolation.<sup>7</sup> Other property characteristics, such as size, year of construction, number of floors, etc, are also obtained from both DoF and PLUTO data. This data can then be linked with transaction data by the unique Borough-Block-Lot identifier. We then calculate the assessed value per m<sup>2</sup> by

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<sup>6</sup>Transactions with sales prices that are above \$10 million or below \$10,000 or a m<sup>2</sup> price below \$1,000 or above \$100,000 are excluded. Observations with list prices that are more than to two times the sales prices are also excluded. We further drop observations from postcodes with fewer than 10 transactions. For list prices, values corresponding to postcodes with fewer than 10 observations are replaced with missing values.

<sup>7</sup>The correction factor adjusts extrapolated market values prior to 2007 to align them with post-2007 data, correcting for changes in assessment calculation introduced in 2019. The correction factor is computed as  $1 + \frac{\text{average extrapolated assessed price 2006}}{\text{average assessed price 2007}} - \frac{\text{average assessed price 2006}}{\text{average assessed price 2007}}$ .

TABLE 2 – DESCRIPTIVE STATISTICS - NEW YORK CITY

	N	Mean	S.d.	Min	Max
<b>Panel A: Full sample</b>					
Sales price/m <sup>2</sup> (in \$)	626,254	8,475	8,691	1,000	99,958
List price/m <sup>2</sup> (in \$)	195,108	11,502	7,600	1,001	99,495
Tax class 1 assessed value/m <sup>2</sup> (in \$)	14,051,672	3,925.59	2,234.34	500	99,959
Tax class 2 and 4 assessed value/m <sup>2</sup> (in \$)	3,685,871	2,291.37	1,503.29	500	93,791
<b>Panel B: Matched samples</b>					
<i>Panel B.1: Sales price and list price matched sample</i>					
Sales price/m <sup>2</sup> (in \$)	195,108	10,926.80	7,076.74	1,001	97,854
List price/m <sup>2</sup> (in \$)	195,108	11,501.80	7,599.51	1,001	99,495
<i>Panel B.2: Tax class 1 sales price and assessed value matched sample</i>					
Sales price/m <sup>2</sup> (in \$)	201,269	4,303.56	3,092.54	1,000	96,524
Assessed value/m <sup>2</sup> (in \$)	201,269	4,223.61	2,429.48	500	92,296
<i>Panel B.3: Tax class 2 and 4 sales price and assessed value matched sample</i>					
Sales price/m <sup>2</sup> (in \$)	283,049	11,022.08	9,523.36	1,000	99,934
Assessed value/m <sup>2</sup> (in \$)	283,049	2,360.35	2,583.07	500	90,604

*Notes:* The dataset covers sales prices and assessed values during the period 2003 to mid-2023, list prices during the period mid-2000 to mid-2023. Tax class 1 includes up to three-unit residential properties. Tax class 2 and 4 includes residential properties with more than three units, including coop and condo.

dividing the building assessed value by the total building floor space.<sup>8</sup> Outliers are set to missing.<sup>9</sup>

Table 2 presents the descriptive statistics of the New York City dataset. The average list prices in the full sample (Panel A) are the highest because the list prices are mainly available for condominium and co-ops, which generally have higher prices per m<sup>2</sup>. Panel B reports descriptive statistics for the matched sample. Because of data availability and the difference in assessment methods, we use three matched samples. Panel B.1 matches sales prices with list prices, resulting in a mean sales prices much closer to the mean list prices, consistent with the expectation that the matched units are mostly condominiums and co-ops. Panel B.2 matches sales prices with assessed values of Tax class 1 buildings. Here, assessed values are lower than sales prices on average, likely due to the lagged nature of the assessment method, which uses transaction data of 3 past years. In Panel B.3, which covers sales prices matching assessed values of Tax class 2 and 4 buildings, the gap between assessed values and sales prices is even larger. This is expected, as these valuations are based on income capitalization using comparable rental properties, which may not fully reflect market sales prices.

<sup>8</sup>Some buildings are mixed-use, therefore, the assessed value per m<sup>2</sup> of residential area can be over or undervalued. An analysis restricted to residential-only buildings is provided in Appendix B.

<sup>9</sup>Outliers are assessed values per m<sup>2</sup> that are above \$100,000 or below \$500.

## 3 Analyzing list prices, assessed values and sales prices

### 3.1 Decomposing the prices

We first aim to decompose the log of sales prices, list prices, and assessed values per m<sup>2</sup> into cross-sectional and time variation. To begin with, let  $p_{ijt} \in \{p_{ijt}^s, p_{ijt}^l, p_{ijt}^a\}$  denote sales prices, list prices, or assessed values per m<sup>2</sup>, respectively, of observation  $i$  in a particular postcode  $j$  and month (for sales prices and list prices) or year (for assessed values)  $t$ . Further, let  $x_{itm}$  denote the housing characteristics  $m$  of house  $i$  at time  $t$ ,  $\gamma_j$  be a location fixed effect,  $\delta_t$  be a time fixed effect and  $\epsilon_{ijt}$  be the error term. The following model is then estimated:

$$\log p_{ijt} = \alpha_0 + \sum_{m=1}^M \alpha_m x_{itm} + \gamma_j + \delta_t + \epsilon_{ijt} \quad (1)$$

We estimate the model separately for the Netherlands and New York City with the definition of fixed effects specified according to each context. For the Netherlands, the location fixed effects correspond to four-digit postcodes, while for New York City, we use postcodes, which generally cover several city blocks.<sup>10</sup> Regarding the time fixed effects, we use January 2014 as the reference period for the Netherlands and January 2003 as the reference period for New York City. For sales and list prices, the time fixed effects correspond to the month of sale and the month of listing respectively. Since assessed values in both datasets are updated annually, the corresponding time fixed effects are defined at the year level. When decomposing assessed values in New York City, we account for the fact that these assessed market values are assigned at the building level instead of unit level. To ensure that buildings with more residential units contribute proportionally to the estimation, we apply weights based on the number of residential units per building.

### 3.2 Cross-sectional component

After obtaining the postcode fixed effects from the decomposition model, we conduct bivariate regressions to examine the extent to which variation in the spatial structure of sales prices can be explained by the spatial patterns of list prices and assessed values. Specifically, we aim to assess whether list prices or assessed values can serve as reliable proxies for sales prices in capturing location-based pricing differences. In these regressions, the dependent variable is the predicted sales price postcode fixed effect,  $\hat{\gamma}_j^s$ , and the

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<sup>10</sup>In 2023, a four-digit postcode in the Netherlands has 5,000 residents on average, while a postcode in New York City has 11,000 residents on average.

TABLE 3 – CROSS-SECTIONAL CORRELATIONS (THE NETHERLANDS)

	(1)	(2)	(3)	(4)
	<i>Full sample</i>		<i>Matched sample</i>	
Postcode FE list prices	1.016*** (0.0021)		1.031*** (0.0021)	
Postcode FE assessed values		0.783*** (0.0072)		1.008*** (0.0026)
Constant	-0.224*** (0.0164)	1.930*** (0.0519)	-0.277*** (0.0169)	-0.0817*** (0.0208)
Number of observations	3,968	3,968	3,895	3,895
R <sup>2</sup>	0.989	0.848	0.988	0.981
<i>p</i> -value <i>t</i> -test coef = 1	0.000	0.000	0.000	0.0015

Notes: Dependent variable is postcode FE related to sales prices. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

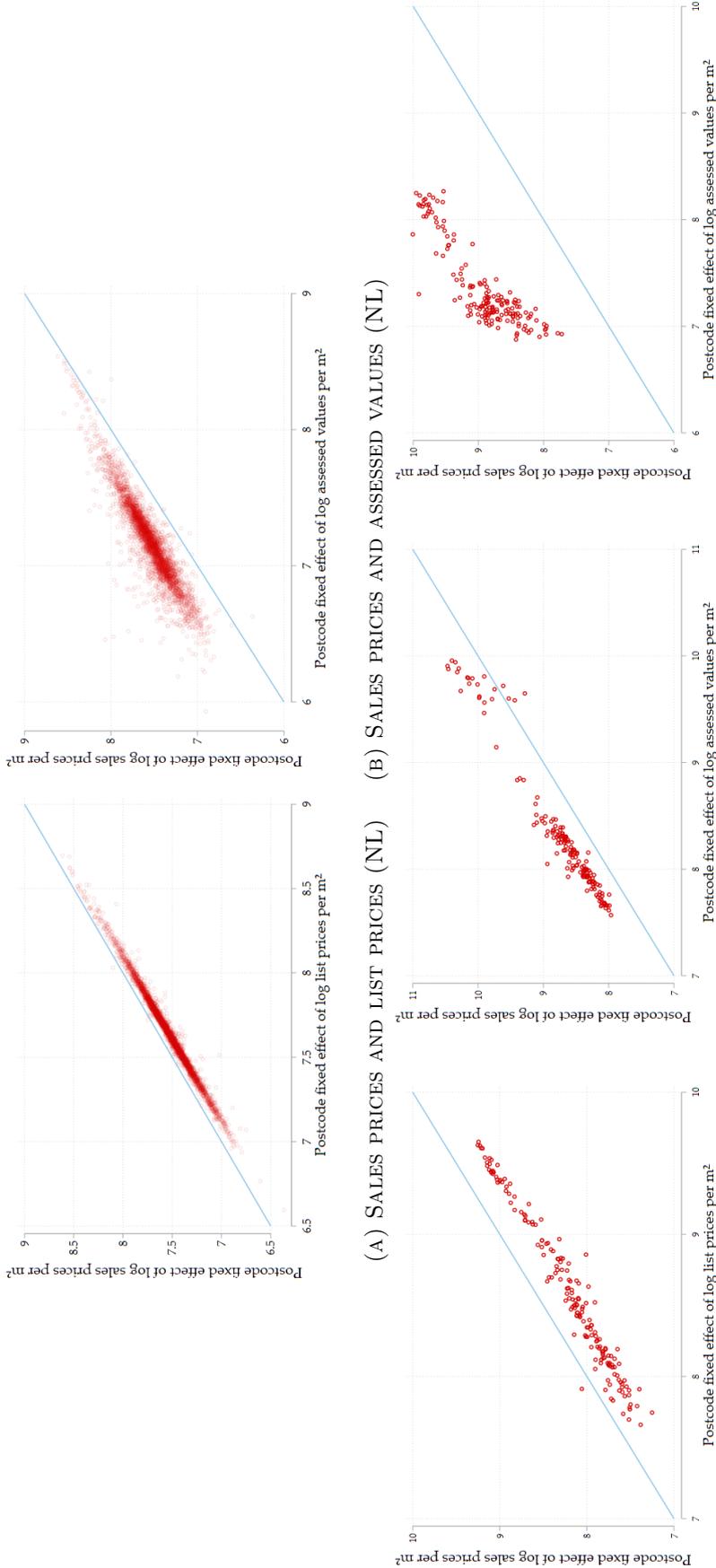
explanatory variable is the predicted postcode fixed effect of either list prices,  $\hat{\gamma}_j^\ell$ , or assessed values,  $\hat{\gamma}_j^a$ . Our bivariate regressions go as follows:

$$\hat{\gamma}_j^s = \beta_0 + \beta_1 \hat{\gamma}_j + \epsilon_j, \quad (2)$$

in which  $\hat{\gamma}_j \in \{\hat{\gamma}_j^\ell, \hat{\gamma}_j^a\}$ . We illustrate the associations between the fixed effects recovered from Eq. (1) in Figure 1. We report the estimates of  $\beta_1$  from Eq. (2) in Tables 3 and 4, including a *t*-test to assess whether the fixed effects scale linearly with one another (which would imply  $\beta_1 = 1$ ) and the  $R^2$  as a measure of the strength of the correlation between the fixed effects.

**The Netherlands.** Figure 1a, together with Columns (1) and (3) of Table 3, Panel A shows a strong linear relationship between postcode fixed effects of list prices and sales prices. Given the location of the scatter points below the 45-degree line, properties generally sell at a discount relative to the list price. This is the typical result for balanced markets, where sellers set a slightly higher asking price, anticipating that buyers will negotiate downward. The estimated slope coefficients are significantly higher than one, implying that this discount is slightly higher in high-price regions. This could be driven by greater market liquidity in high-priced areas such as downtowns (Amaral et al., 2023).

Figure 1b, along with Columns (2) and (4) in Table 3, also shows a strong association between the fixed effects of sales prices and assessed values. Assessed values are slightly lower than sales prices (reflected by the location of the scatter points above the 45-degree line), possibly due to the timing and methodology of property valuation. Assessed values are derived from transactions around the first day of the year, regardless of when or whether the house is transacted. Therefore, the data might be outdated and may not fully capture more recent changes in market conditions, typically leading to undervaluation. Secondly, the assessment date falls in winter, a season typically associated with lower sales



(A) SALES PRICES AND LIST PRICES (NL) (B) SALES PRICES AND ASSESSED VALUES (NL)

(C) SALES PRICES AND LIST PRICES (NYC) (D) TAX CLASS 1 SALES PRICES AND ASSESSED VALUES (NYC) (E) TAX CLASS 2 AND 4 SALES PRICES AND ASSESSED VALUES (NYC)

Notes: Figures 1a and 1b report the postcode fixed effects of prices derived from the full sample in the Netherlands while Figures 1c, 1d, and 1e refer to New York city. The blue lines are 45-degree line.

FIGURE 1 – CROSS-SECTION VARIATION IN SALES PRICE, LIST PRICES, AND ASSESSED VALUES.

prices compared to other seasons (Kaplanski and Levy, 2012).

In column (2), the estimated slope coefficient is well below one. This likely reflects sample selection, since transacted properties represent a non-random subset of all properties. Indeed, when we re-examine the correlation between postcode fixed effects in column (4), the coefficient is essentially equal to one. Taken together, Columns (2) and (4) therefore make a strong case for using assessed values in cross-sectional analysis: they capture variation in sales prices remarkably well (as shown in Column (4)) while at the same time being more comprehensive and less subject to sample selection than sales prices (as suggested by Column (2)).

Overall, the main takeaway is that list prices and assessed values do a very good job of predicting cross-sectional differences in sales prices in the Netherlands.

**New York City.** The findings for New York City, in many respects, exhibit a similar pattern to those observed in the Netherlands. As shown in Table 4, Figures 1c, 1d and 1e, list price postcode fixed effects are consistently higher than those of sales prices, while assessed value fixed effects are lower. From columns (1) and (4) of Table 4, we observe that list prices scale less than proportionately in sales prices, but the coefficients are again close to one in both full and matched sample, with the latter statistically indistinguishable from 1. This suggests that list prices almost perfectly track sales prices across space. In contrast, sales prices scale more than proportionately in assessed values, especially in the matched sample (Column 2, 3, 5, and 6). The implication is that the undervaluation of assessed values is particularly large in high-price areas such as Manhattan. It is noteworthy that while assessed values are good proxies for sales prices for 1-3 unit homes (tax class 1), it understates price variation across space for multi-unit buildings (tax class 2 and 4). Note also that the  $R^2$  is considerably lower for the latter properties, suggesting that the valuation technique matters for the results. Specifically, assessed values in tax classes 2 and 4 are derived using the income capitalization method, which seems to perform considerably poorer than the method based on past comparable transactions.

### 3.3 Time-variation component

The month fixed effects  $\{\hat{\delta}_t^s, \hat{\delta}_t^\ell, \hat{\delta}_t^a\}$  from Eq. (1) are displayed in Figure 2. We find that, over time and across market conditions, month fixed effects for sales prices are generally higher than those for list prices (Figure 2a and 2c). Two notable exceptions are the 2011-2014 period in the Netherlands, which was the only recession in its housing market over the past three decades (Jadecivius and van Gool, 2020), and the 2008-2016 period in New York City, which coincided with and followed the Great Recession. During this phase, the month fixed effects for list prices exceed those for sales prices. This pattern is

TABLE 4 – CROSS-SECTIONAL CORRELATIONS (NYC)

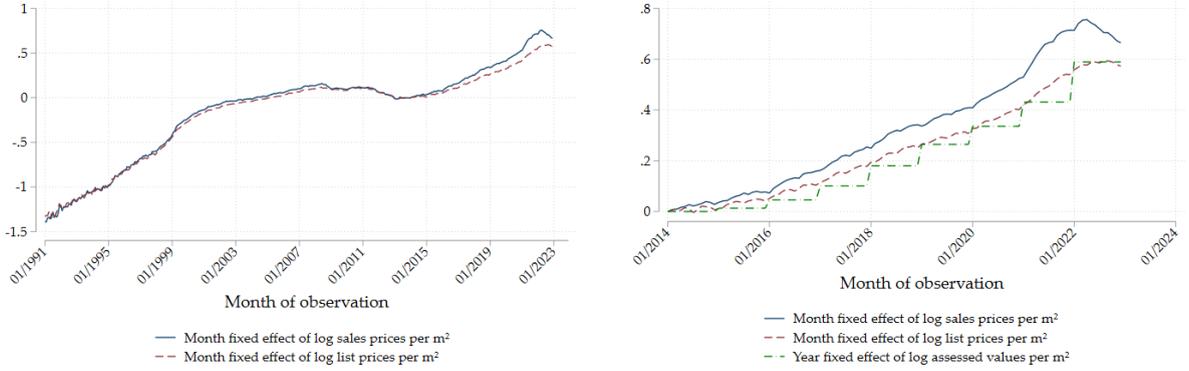
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Full sample</i>			<i>Matched samples</i>		
<i>Sub-sample:</i>	—	Tax Class 1	Tax Class 2 & 4	—	Tax Class 1	Tax Class 2 & 4
Postcode FE list price	0.924*** (0.0136)			0.986*** (0.0061)		
Postcode FE assessed value		0.949*** (0.0263)	1.178*** (0.0454)		1.101*** (0.0255)	0.824*** (0.0706)
Constant	0.274** (0.118)	0.828*** (0.213)	0.256 (0.343)	0.0456 (0.0548)	-0.403* (0.209)	2.781*** (0.500)
Number of observations	178	167	168	178	167	158
R <sup>2</sup>	0.962	0.952	0.741	0.994	0.926	0.531
<i>p</i> -value <i>t</i> -test coef = 1	0.000	0.0566	0.0001	0.0211	0.0001	0.0137

*Notes:* Dependent variable is postcode FE related to sales prices. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

consistent with [Haurin et al. \(2013\)](#) (UK market) and [Lyons \(2019\)](#) (Irish market) in that in a booming market, sales prices tend to be higher than list prices, while the reverse is true during a bust phase.

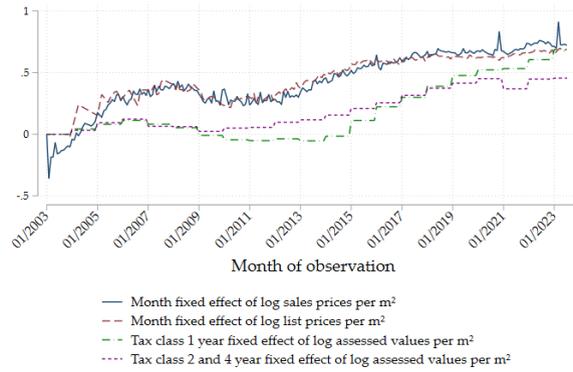
Turning to assessed values, we observe a relatively smooth upward trend over time in both countries (Figure 2b and 2c), consistent with their institutional role in property taxation. However, notable exceptions appear in the New York City data. Specifically, we identify two periods of decline in the assessed value fixed effects: in 2009, following the Great Recession, and in 2021, after the COVID-19 pandemic. These inflection points reflect the lagged but partially responsive nature of property assessments. Notably, in the case of New York City, the time fixed effects of assessed values are much lower and less volatile compared to the time fixed effects of sales prices. The smoother trend in assessed values likely reflects the nature of New York City’s assessment process. Assessed values are updated annually using lagged data. As a result, they respond more slowly to rapid changes in the housing market but tend to continue rising or decrease with lower rate even when sales prices stall or fall.

**The Netherlands.** To quantify the strength of the temporal correlation between different price measures, we run a series of bivariate regressions using the predicted time fixed effects obtained from the hedonic decompositions (Eq. 1). In addition, we estimate an alternative specification in which sales prices are decomposed into year fixed effects instead of month fixed effects, which are more comparable with the year fixed effects of assessed values. We regress the month or year fixed effects of sales prices on those of list



(A) MONTH FIXED EFFECTS 1991-2022 (NL)

(B) TIME FIXED EFFECTS 2014-2022 (NL)



(C) TIME FIXED EFFECTS 2005-2023 (NYC)

Notes: The time fixed effects for the Netherlands are shown in Figures 2a and 2b. Figure 2a plots the month fixed effects of sales and list prices over the full sample period 1991-2022. Figure 2b displays the time fixed effects of sales prices, list prices, and assessed values from 2014 using the matched sample. For New York City, Figures 2c present the time fixed effects. The reference period where time fixed effects are normalized to zero is January 2014 for the Netherlands and January 2003 for New York City.

FIGURE 2 – TEMPORAL VARIATION IN SALES PRICES, LIST PRICES, AND ASSESSED VALUES

prices and assessed values, depending on the specification. Our bivariate regressions are:

$$\hat{\theta}_t^s = \beta_0 + \beta_1 \hat{\theta}_t^a + \epsilon_t \quad (3)$$

in which  $\hat{\theta}_t \in \{\hat{\theta}_t^l, \hat{\theta}_t^a\}$ . We then run a  $t$ -test to test whether the time fixed effects grow at comparable rates in the *long run* ( $\beta = 1$ ). The estimation results are in Table 5.

Column (1) reveals that sales and list prices grow at nearly the same rates in the long run, as the coefficient, at 1.055, is close to one. This is consistent with the graphical evidence from Figure 2a. In the matched sample which covers the period 2014-2022 (Column (3)), the estimated coefficient increases substantially to 1.244 and is statistically different from 1. This is consistent with the graphical evidence from Figure 2b, revealing that sales prices outpace list prices during periods of growth.

Columns (2) and (4) examine the relationship between the year fixed effects of sales prices and assessed values. Since both the full sample and the matched sample cover the

TABLE 5 – TEMPORAL CORRELATIONS (THE NETHERLANDS)

	(1)	(2)	(3)	(4)
	<i>Full sample</i>		<i>Matched sample</i>	
<i>Dependent variable:</i>	Month FE sales prices	Year FE sales prices	Month FE sales prices	Year FE sales prices
Month FE list prices	1.055*** (0.0041)		1.244*** (0.0135)	
Year FE assessed values		1.201*** (0.0663)		1.151*** (0.0451)
Constant	0.0434*** (0.00203)	0.0427* (0.0193)	0.0184*** (0.00225)	0.0215 (0.0147)
Robust standard errors	✓		✓	
Number of observations	384	9	108	9
R <sup>2</sup>	0.996	0.979	0.995	0.989
<i>p</i> -value <i>t</i> -test coef. = 1	0.000	0.0188	0.000	0.0122

*Notes:* Column (1) covers the period 1991-2022. Columns (2), (3), and (4) cover the period 2014-2022. Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

growth period from 2014-2022, it is, perhaps, no surprise that sales prices outpace assessed values. The coefficients are only borderline statistically different from 1, possibly owing to the small number of observations. It is interesting to observe that the gap in growth rates, consistent with Figure 2b, is roughly similar for list prices and assessed values in the matched sample. More generally, the consistently high  $R^2$  confirms that list prices and assessed values follow transaction prices relatively closely in the long run.

**New York City.** We examine the long-run relationship between different price measures according to Eq. (3), using bivariate regressions with the predicted time fixed effects obtained from the hedonic decompositions (Eq. 1). The results are shown in Table 6. In the full sample, sales prices grow more strongly in the long run than list prices, as already suggested by Figure 2c. In the matched sample, however, the coefficient is close to 1 and not statistically different from it, suggesting that list prices still track sales prices relatively well over time.

Turning to assessed values, for Tax Class 1, the estimates show a relatively strong alignment with sales prices in both the full and matched samples. The results based on the preferred matched sample reveal that assessed values grow at a somewhat slower rate than transaction prices, as already suggested by Figure 2c. In contrast, assessed values for Tax Classes 2 and 4 grow at a faster rate than sales prices, on average, which is consistent with the convergence observed over the second half of our study period in Figure 2c. As in the Netherlands, the consistently high  $R^2$  reflects that both list prices and assessed values follow transaction prices in the long run.

TABLE 6 – TEMPORAL CORRELATIONS (NYC)

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Full sample</i>			<i>Matched samples</i>		
<i>Sub-sample:</i>	—	Tax class 1	Tax Class 2 & 4	—	Tax class 1	Tax Class 2 & 4
<i>Dependent variable:</i>	Month FE sales price	Year FE sales price	Year FE sales price	Month FE sales price	Year FE sales price	Year FE sales price
Month FE list price	1.200*** (0.0250)			1.018*** (0.0187)		
Year FE assessed values		0.889*** (0.0566)	1.321*** (0.195)		1.034*** (0.0727)	0.914*** (0.0324)
Constant	-0.101*** (0.0182)	0.186*** (0.0173)	0.391*** (0.0495)	-0.051*** (0.0166)	0.193*** (0.0187)	0.205*** (0.0391)
Robust standard errors	✓			✓		
Number of observations	246	21	21	246	21	21
R <sup>2</sup>	0.936	0.928	0.706	0.936	0.914	0.863
<i>p</i> -value <i>t</i> -test coef. = 1	0.000	0.0649	0.117	0.335	0.642	0.315

Notes: The data covers the period 2003-mid 2023. Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

### 3.4 Short-run adjustments

We now turn to the *short-run* temporal relationship between sales prices on the one hand, and list prices and assessed values on the other. Our central question is whether short-run movements in sales prices are reflected in these two proxy variables. Only in this case can identification strategies that exploit short-run variation recover the correct hedonic implicit amenity prices from list prices or assessed values. To allow for the possibility that the two proxy variables adjust with a lag, we estimate distributed lag models (see Almon, 1965; Schmidheiny and Siegloch, 2023).

We regress the first difference of the yearly fixed effects of list prices and assessed values, on the contemporaneous and lagged first differences of the yearly fixed effects of sales prices from the full sample. That is, we estimate the following regression:

$$\hat{\theta}_t - \hat{\theta}_{t-1} = \alpha + \sum_{k=0}^K \beta_k (\hat{\theta}_{t-k}^s - \hat{\theta}_{t-k-1}^s) + \epsilon_t \quad (4)$$

where  $\hat{\theta} \in \{\hat{\theta}^\ell, \hat{\theta}^a\}$ , thus  $\hat{\theta}_t - \hat{\theta}_{t-1}$  denotes the change in the fixed effect for list prices and assessed values, and  $\hat{\theta}_{t-k}^s - \hat{\theta}_{t-k-1}^s$  is the change in the fixed effect for sales prices lagged by  $k$  periods. In a slight abuse of terminology, we refer to the sequence of estimated  $\beta_k$  coefficients as the *impulse response*, and to the cumulative sum,  $\sum_{j=0}^k \beta_j$ , as the *cumulative*

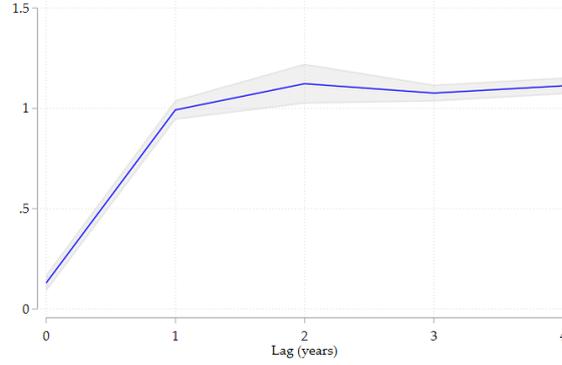
*impulse response*.<sup>11</sup> When the cumulative impulse response reaches 1 at horizon  $t + k$ , this indicates that the full effect of a price shock occurring at time  $t$  has been reflected in list prices or assessed values by time  $t + k$ . In other words, it takes  $k$  periods for list prices or assessed values to fully adjust to changes in transaction prices. To account for potential serial correlation and heteroskedasticity, we use Newey-West standard errors with automatic bandwidth selection (Newey and West, 1994). For assessed values, we carry out the analysis at the yearly level. For list prices, our data additionally allow us to conduct the analysis at the quarterly and monthly levels to gain insights into how well list prices reflect changes in transaction prices in higher-frequency settings.

**Assessed values and sales prices.** We illustrate the estimated cumulative impulse responses of assessed values to sales prices in Figure 3. In the Netherlands (Figure 3a), we find little contemporaneous response of assessed values. However, the cumulative response quickly approaches 1 after one year. This lag structure aligns with the institutional design, in which assessed values are updated annually based on sales data around 1<sup>st</sup> January including the sales data of the preceding year. In New York City, the cumulative impulse response for tax class 1 properties (Figure 3b) is close to 0 contemporaneously and increases toward 1 after roughly two years. The longer adjustment period compared to the Netherlands is intuitive as assessors incorporate transaction data from the previous three years. Still, assessments eventually reflect market conditions within a reasonable time frame. In contrast, for tax classes 2 and 4 (Figure 3c), the cumulative impulse response begins at a relatively high value of 0.8 and remains statistically indistinguishable from (though consistently below) 1 thereafter. This high contemporaneous response suggests that assessed values co-move strongly with sales prices, perhaps because of the less mechanical and more judgment-based appraisal process. However, the result that the cumulative impulse response does not reach one and wide confidence intervals indicate that income-based assessments do not fully incorporate new information from short-run changes in sales prices.

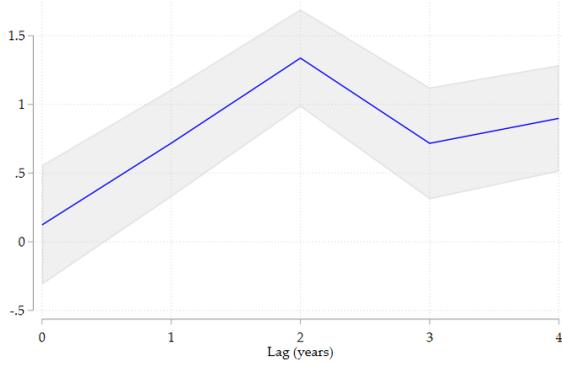
**List prices and sales prices.** The estimated cumulative impulse response of list prices to sales prices is illustrated in Figure 4. At the yearly frequency (Figures 4a and 4d), there is a strong contemporaneous response of list prices to sales prices in both markets. In the Netherlands, the contemporaneous response is particularly high (around 0.7), indicating that a substantial portion of sales price shocks is immediately incorporated into list prices. The adjustment then continues gradually over time, reaching about 0.9 after four years.

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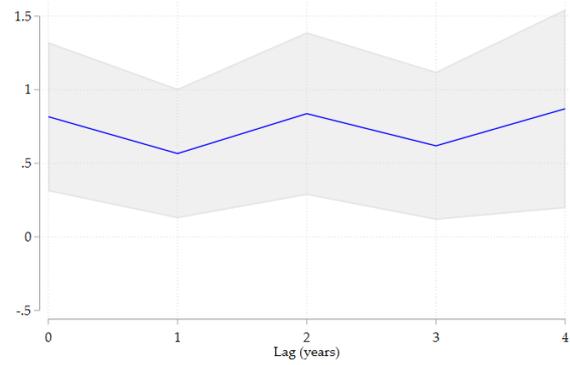
<sup>11</sup>The terms “impulse response” (IR) and “cumulative impulse response” (CIR) are typically used in the context of vector autoregression (VAR) models, but the interpretation is analogous in our distributed lag model.



(A) CIR OF ASSESSED VALUES (NL)



(B) CIR OF ASSESSED VALUES TAX CLASS 1 (NYC)



(C) CIR ASSESSED VALUES TAX CLASS 2 AND 4 (NYC)

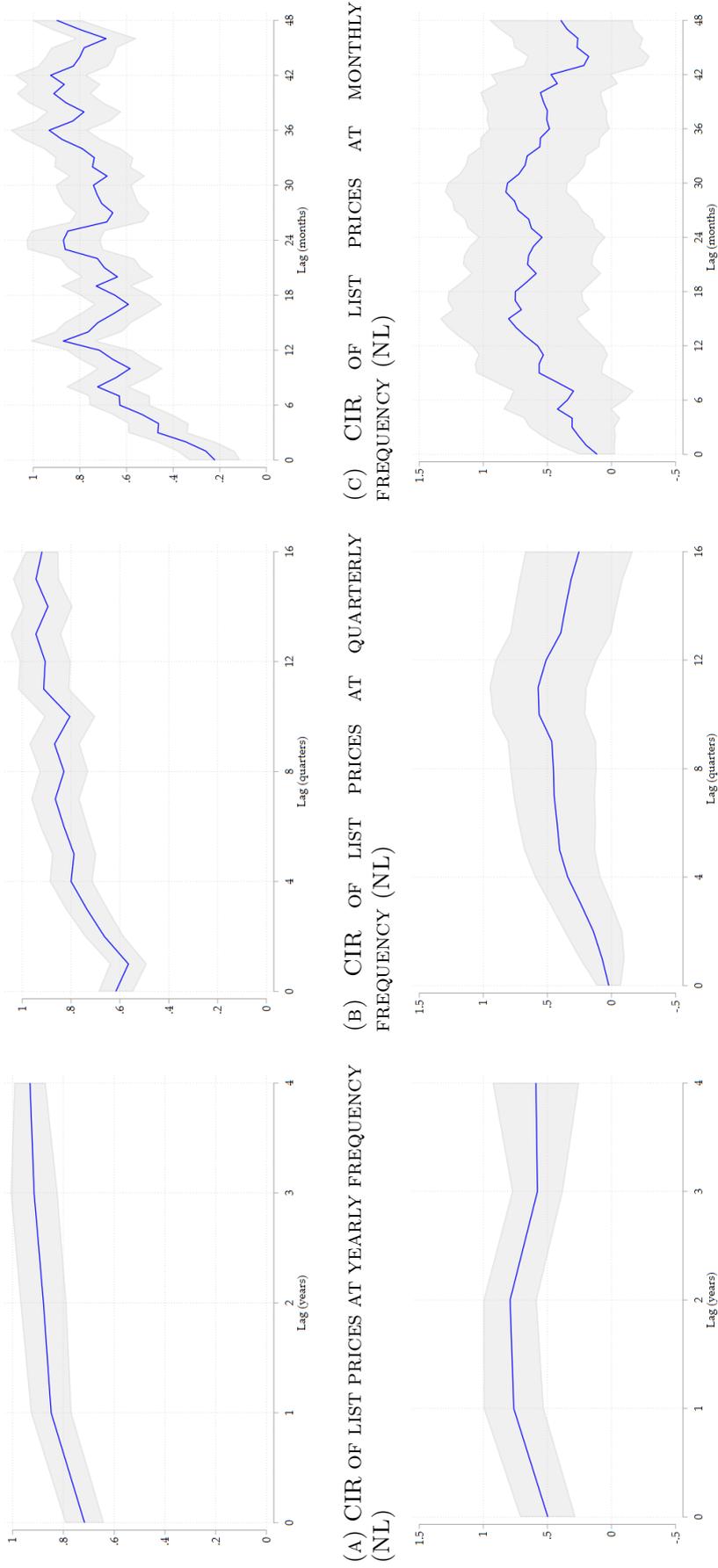
*Notes:* The figure displays cumulative impulse response (CIR) of changes in assessed values to changes in sales prices. Estimates are obtained from Equation 4 using the time fixed effects derived from prior price decomposition. The standard errors are Newey-West standard errors with with automatic bandwidth selection.

FIGURE 3 – CUMULATIVE IMPULSE RESPONSE OF ASSESSED VALUES TO SALES PRICES

In contrast, in New York City, the contemporaneous response starts lower—around 0.5—rises to nearly 0.8 after one year, and then declines around year 3. This pattern suggests a weaker transmission of sales price changes into list prices in New York City.

At the quarterly frequency (Figures 4b and 4e), the difference between the two markets becomes more pronounced. In the Netherlands, list prices still exhibit a contemporaneous response of roughly 0.6, followed by a gradual increase to about 1 after seven quarters. In New York City, however, the contemporaneous response is close to zero. Although the cumulative impulse response rises during the first few quarters—reaching about 0.4 by quarter five—it soon flattens, showing little further adjustment.

At the monthly frequency (Figures 4c and 4f), the contemporaneous response is modest, at around 0.2, even in the Netherlands. As before, the cumulative impulse response continues to rise toward 1 after roughly 24 months. In contrast, list prices in New York City display an even smaller contemporaneous response of about 0.09, followed by a gradual increase to around 0.7 after one year. Beyond that point, the response flattens



Notes: We display cumulative impulse response of changes in list prices to changes in sales prices at yearly frequency (Figure 4a and 4d), quarterly frequency (Figure 4b and 4e), and monthly frequency (Figure 4c and 4f). Estimates are obtained from Equation 4 using the time fixed effects derived from prior price decomposition. The standard errors are Newey–West standard errors with automatic bandwidth selection.

FIGURE 4 – CUMULATIVE IMPULSE RESPONSE OF LIST PRICES AT YEARLY AND QUARTERLY FREQUENCY

and slightly declines after 15–18 months. The wide confidence intervals also underscore the higher volatility and lower precision of monthly movements in the New York City market.

The main takeaway from Figure 4 is that the contemporaneous response of list prices to changes in sales prices is strong at the yearly frequency but weak at sub-yearly frequencies.<sup>12</sup> Hence, some caution is warranted when using list prices as proxies for market values in high-frequency settings. One reason for the fuzziness of the short-run relationship we document is that list prices may lead sales prices if forward-looking expectations play a role (Knight et al., 1994; Lyons, 2019). While this is not the focus of our analysis—we view sales prices as the best measure of market value, reflecting equilibrium outcomes between buyers and sellers, and are primarily interested in how closely proxies track these outcomes—we provide a complementary analysis of the response of sales prices to past changes in list prices for interested readers in Appendix C.1.<sup>13</sup>

### 3.5 Turning points

Having established that list prices (and assessed values) are good proxies for transaction prices in the long run, but fuzzy proxies in the short run, we now ask how well list prices capture the housing market cycle—a medium-run phenomenon. Specifically, we estimate turning points, defined as the moments in the housing cycle when price trends reverse direction—either from rising to falling or from falling to rising. Our focus is on identifying the extent to which turns in list price trends lead, lag, or occur at the same time as turns in sales price trends, and whether this dynamic varies across spatial units. To this end, we estimate a piecewise linear spline specification with two endogenous knots using non-linear least squares:

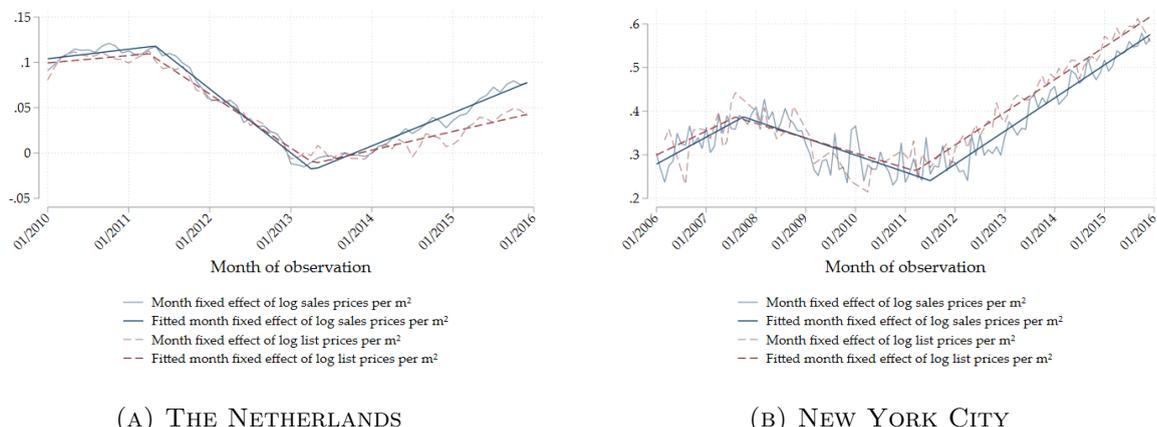
$$\hat{\gamma}_t = \beta_0 + \beta_1 t + \beta_2(t - b_1) \cdot \mathbb{1}(t \geq b_1) + \beta_3(t - b_2) \cdot \mathbb{1}(t \geq b_2) + \epsilon_t, \quad (5)$$

where  $\hat{\gamma}_t$  is the time fixed effects obtained from the decomposition in Eq. (1) as dependent variable,  $t$  is the monthly trend, and  $\epsilon_t$  is the residual. The parameters to be estimated are  $\beta_0$ , the intercept,  $\beta_i$ , the slope of the  $i$ -segment with  $i = 1, 2, 3$ , and  $b_j$ , the month of the  $j$ -th turning points with  $j = 1, 2$ . We are primarily interested in  $b_1$ , which corresponds to the date when a market enters the recession in the aftermath of the financial crisis, and  $b_2$ , which marks the exit date.  $\hat{b}_1^s - \hat{b}_1^\ell$  and  $\hat{b}_2^s - \hat{b}_2^\ell$  then provide accessible statistics

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<sup>12</sup>Pairwise  $z$ -tests (Clogg et al., 1995) reveals that contemporaneous responses of list prices to changes in sales prices across frequencies differ significantly in almost all cases. The only exceptions are year versus quarter frequency in the Netherlands ( $p$ -value = 0.0607), and quarter versus month frequency in New York City ( $p$ -value = 0.284).

<sup>13</sup>Readers can also find the distributed lag model results for different market cycle phases in Appendix C.2.



Notes: The solid lines represent the estimated month fixed effects derived from the price decomposition equation (Eq.1). We then use the obtained month fixed effects to run non-linear least squares regressions as specified in Eq.5, where the turning points are estimated endogenously. The dashed lines show the fitted values from these non-linear least square regressions.

FIGURE 5 – TIME FIXED EFFECTS WITH FITTED LINE

that inform us whether, and how, the measurement of a fundamental change in housing market conditions depends on the underlying data. Intuitively, a positive value implies that list prices turn earlier than sales prices, while a negative value implies the opposite.

**The Netherlands.** We begin with the national-level analysis using monthly fixed effects for the period 2010-2016. The fitted values derived from the non-linear least squares regressions are plotted in Figure 5a. The regressions identify the start of the recession, captured by  $\hat{b}_1^s$  in April 2011. The trend in list prices signals the start of the downturn one month earlier. The end of the recession, captured by  $\hat{b}_2$ , is estimated to occur around April 2013, regardless of whether sales prices or list prices are used.

Of course, it is entirely conceivable that the turning points in the housing market cycle differ across regional housing markets in the Netherlands. Therefore, we extend the decomposition model from Eq. (1) to incorporate interactions between time and local labor market regions, i.e., 40 travel to work areas (TTWAs). Specifically, we estimate the following model including TTWA-month fixed effects  $\theta_{xt}$ :

$$\log p_{ixt} = \alpha_0 + \sum_{m=1}^M \alpha_m x_{itm} + \theta_{xt} + \epsilon_{ijt}, \quad (6)$$

where the dependent variable  $p_{ixt} \in \{p_{ixt}^s, p_{ixt}^l\}$  denotes the sales prices and list prices, respectively, of property  $i$  located in TTWA  $x$  at time  $t$ . The vector  $x_{itm}$  contains housing characteristics  $m$  of property  $i$  at time  $t$ .  $\theta_{xt}$  is a fixed effect that captures all TTWA-month interaction effects. Finally,  $\epsilon_{ixt}$  is the error term. We apply the same non-linear least squares regressions separately to each time series of  $\theta_{xt}$  for housing market  $x$  to find

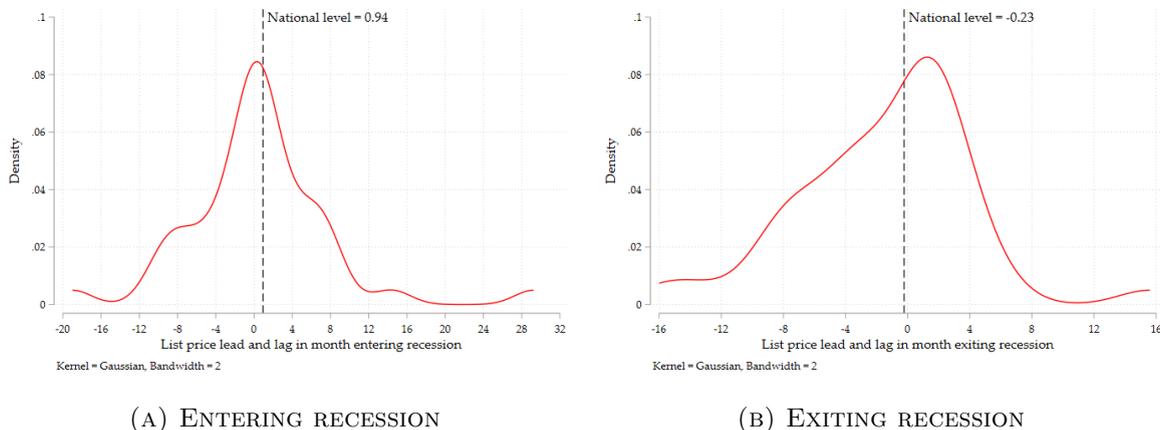


FIGURE 6 – TIMING DIFFERENCE BETWEEN TURNING POINTS IN LIST AND SALES PRICES

the market-specific turning points  $b_{1,x}$  and  $b_{2,x}$ .

Figure 6 presents the kernel density of estimated lead-lag differences between list prices and sales prices across TTWAs. The most important insight is that the regional turning points inferred from list and sales prices typically occur within one year of each other. However, they can still differ by several months. This finding is consistent with the results from the previous section, which indicated a strong relationship at the yearly frequency but a less pronounced relationship at higher frequencies. Figure 6 also suggests that turning points inferred from list prices are measured more accurately when entering the recession than when exiting it. When entering the recession, turning points differ by less than four months for the majority of regions, with list price turning points lagging in about as many regions as they lead. In contrast, when exiting the recession, differences in measured turning points are generally larger, with list prices lagging in roughly two thirds of the regions.<sup>14</sup>

**New York City.** For New York City, we estimate the timing of market turning points using city-level month fixed effects obtained from the decomposition model (see Eq. (1) and Fig. 5b). We focus on the housing market cycle from 2006 to 2015. The results show that the turning points in list prices occur slightly earlier—by about two months when entering the recession and about three months when exiting it. This finding is consistent with the main result for the Netherlands: list prices accurately capture the timing of turning points at the yearly level, but not necessarily at the monthly level.<sup>15</sup>

<sup>14</sup>See Appendix D for the spatial distribution of timing differences.

<sup>15</sup>Due to insufficient monthly list price data in Staten Island and the Bronx during the period 2006-2015, we are unable to conduct a short-term change analysis at the borough level or for smaller spatial units.

## 4 Hedonic pricing analyses

The results presented so far have important implications for hedonic valuation studies. Put simply, we expect list prices and assessed values to be good substitutes for sales prices in their cross-section, owing to their strong predictive power. Hedonic studies adopting difference-in-differences methodologies, there is potential cause for concern due to sluggish adjustment in list prices and, in particular, assessed values. In this section, we put this intuition to a test by replicating selected studies that employ hedonic pricing models, using different house price measures as dependent variables.

In each case, we extend the dataset to include the most recent available data, thereby allowing for a broader temporal analysis. We begin with a study that focuses on cross-sectional variation, examining how spatial differences in amenities influence house prices. Specifically, we replicate a study that investigates the capitalization of historic amenities into housing prices in the Netherlands. Next, we turn to two studies that rely on temporal variation in (dis)amenities. The first examines the impact of wind turbines on house prices in the Netherlands. The second focuses on New York City, analyzing the effects of the subway expansion on housing values using a difference-in-differences framework. These cases allow us to assess how using different house price measures affects the estimated hedonic implicit prices, depending on whether the identification variation is cross-sectional or spatio-temporal.

In all replications, we use the matched samples introduced in the previous sections to ensure consistency across price measures and comparability of results. In addition, since assessed values are typically available for virtually the entire housing stock, we also conduct the analysis using the full dataset of assessed values. Descriptive statistics for each replication are in Appendix E. Replications based on the exact same time spans used in the original studies reported in Appendix F produce estimates that are very close to those in the original studies.

### 4.1 Historic amenities and sorting of households in the Netherlands

We first replicate [Koster et al. \(2016\)](#), who investigated how historic amenities influence household location choices in the Netherlands. For our research, the dataset is extended from 2014 to 2022 and only includes buildings constructed after 1970 and are, therefore, not listed. We use the following hedonic pricing model:

$$\log p_{it} = \alpha h_{it} + \gamma z_{it} + \phi_t + \xi_{it} \tag{7}$$

TABLE 7 – HISTORIC DISTRICTS AND HOUSE PRICES

	(1)	(2)	(3)	(4)
<i>Sample:</i>	Matched sample			Full sample
<i>Dependent variable:</i>	Log sales price/m <sup>2</sup>	Log list price/m <sup>2</sup>	Log assessed value/m <sup>2</sup>	Log assessed value/m <sup>2</sup>
<b>Specification A: Properties &lt;200m HD boundary</b>				
In historic district	0.0392*** (0.008)	0.0447*** (0.0086)	0.0355*** (0.008)	0.0538*** (0.008)
Number of observations	34,616	34,616	34,616	2,175,365
R <sup>2</sup>	0.743	0.707	0.738	0.791
<i>p</i> -value <i>z</i> -test	—	0.6398	0.7438	0.1982
<b>Specification B: Properties in HDs and &lt;200m HD boundary without a view</b>				
In historic district	0.0542*** (0.0078)	0.0603*** (0.0084)	0.0498*** (0.0078)	0.0642*** (0.0078)
Number of observations	28,256	28,256	28,256	1,800,661
R <sup>2</sup>	0.749	0.714	0.743	0.797
<i>p</i> -value <i>z</i> -test	—	0.5954	0.6915	0.3647
<b>Specification C: Properties outside HDs, but &lt;200m HD boundary</b>				
View on historic amenities	0.0298*** (0.0073)	0.0309*** (0.0077)	0.0254*** (0.0068)	0.0319*** (0.0053)
Number of observations	23,674	23,674	23,674	1,526,036
R <sup>2</sup>	0.760	0.727	0.757	0.795
<i>p</i> -value <i>z</i> -test	—	0.9162	0.6572	0.8213

*Notes:* HD stands for historic district. All specifications include only properties located within 200 meters of a historic district boundary. In Specification A, the treatment group consists of properties inside the historic district, and the control group consists of properties outside the district. In Specification B, the treatment group includes properties inside the historic district without a view of historic amenities, while the control group includes properties outside the district that also lack such a view. In Specification C, the treatment group includes properties outside the historic district with a view of historic amenities, and the control group includes properties outside the district without a view. The control variables used in every specification include housing characteristics, such as size, type of housing, dummy for garden, dummy for maintenance condition, and construction period. The control variables for the full sample of assessed values, however, do not include dummy for garden and dummy for maintenance condition. Historic district boundary fixed effects and year fixed effects are included in every specification. *z*-tests are conducted to test the equality of coefficients compared to the coefficient in column (1). Standard errors are clustered at the postcode four-digit level and in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

in which  $p_{it} \in \{p_{it}^s, p_{it}^l, p_{it}^a\}$  denote the price per m<sup>2</sup> (sales prices, list prices and assessed values respectively) of property  $i$  in year  $t$ ,  $h_{it}$  is a dummy variable that equals 1 when a property is exposed to a historic district,  $z_{it}$  is a vector variable including property characteristics,  $\phi_t$  are year fixed effects; and  $\xi_{it}$  is an identically and independently distributed error term. In this model,  $\alpha$  is the parameter of interest which indicates the impact of historic amenities on housing prices by measuring the percentage change in property price relative to the control group.

We run this model using two subsamples, taking into account the distance to boundary of historic district and the view on historic amenities. In the third specification, we focus on properties that are within 200m of the historic district boundary but outside a historic district, to investigate whether the view on historic amenities has an impact on the housing price. In this specification, we replace the independent variable of interest with a dummy

indicating whether the property has a view on historic amenities. After running the regressions, we test whether the coefficients derived from those regressions are equal by conducting  $z$ -tests (Clogg et al., 1995). The results of the regressions together with the  $p$ -values of the  $z$ -tests are shown in Table 7.

Our findings based on sales prices are consistent with those reported in the original paper. Across all three specifications using the matched sample, we find that the estimated coefficients for both list prices and assessed values are similar in magnitude to those obtained using sales prices. All coefficients are statistically significant at the 1% level. Notably, the coefficient based on list prices is slightly larger, while that based on assessed values is somewhat smaller relative to the coefficient from sales price models. For example, in Specification C, which comes closest to delivering a pure view effect from properties just outside historic districts, the estimated effects from sales and list prices are almost exactly 3%. The effect is 2.5% when estimated from assessed values, but it is not statistically distinguishable from the capitalization on sales prices. Even when increasing the sample size by a factor of more than 50 by using all assessed values, the estimated view effect, at 3.2%, remains remarkably stable. In keeping with expectations, list prices and assessed values thus prove to be good substitutes for sales prices in a cross-sectional valuation study.

## 4.2 Wind turbines and housing values in the Netherlands

We continue using our data from the Netherlands to replicate Dröes and Koster (2021) who estimated the effect of wind turbines on house prices. The dataset is extended from up to mid-2019 to 2022. From 2019 onwards, we observe a boom of renewable energy installations, with substantial construction of wind turbines. Compared to the original paper, our dataset comprises 975 additional wind turbines.

Our model to analyze the effect of wind turbines on housing values is then given by:

$$\log p_{it} = \beta w_{it-1} + \gamma z_{it} + \lambda_j + \psi_{mt} + \epsilon_{it} \quad (8)$$

where  $p_{it} \in \{p_{it}^s, p_{it}^\ell, p_{it}^a\}$  is the price per m<sup>2</sup> (sales prices, list prices and assessed values respectively) of property  $i$  observed in year  $t$ ,  $w_{it-1}$  is an indicator that is 1 if a property is sold, listed or assessed, corresponding to the dependent variable, within 2km in any year following the installation of a wind turbine,  $z_{it}$  are housing characteristics,  $\lambda_j$  are location fixed effects at the postcode 6-digit level (PC6),  $\psi_{mt}$  are municipality by year fixed effects, and  $\epsilon_{it}$  is the error term capturing unobserved characteristics.

We use two separated matched samples: one matching sales prices and list prices, covering the full period from 1985 to 2022, and another matching sales prices and assessed values, covering 2014 to 2022. We first begin by estimating a baseline model using the full

TABLE 8 – WIND TURBINES AND HOUSE PRICES

	(1)	(2)	(3)	(4)	(5)
<i>Sample:</i>	Matched sales-list		Matched sales-assessed		Full sample
<i>Dependent variable:</i>	Log sales price/m <sup>2</sup>	Log list price/m <sup>2</sup>	Log sales price/m <sup>2</sup>	Log assessed value/m <sup>2</sup>	Log assessed value/m <sup>2</sup>
<b>Specification A: Baseline</b>					
Wind turbines placed <2km	-0.0122*** (0.00143)	-0.0111*** (0.00145)	-0.00497** (0.00226)	-0.00430** (0.00216)	-0.00108*** (0.000408)
Number of observations	3,954,215	3,953,995	1,171,090	1,171,090	63,445,005
R <sup>2</sup>	0.922	0.918	0.893	0.885	0.883
<i>p</i> -value <i>z</i> -test	—	0.611	—	0.830	0.090
<b>Specification B: Properties with 1 wind turbine &lt;5km in 2022</b>					
Wind turbines placed <2km	-0.0263*** (0.00378)	-0.0162*** (0.00425)	-0.0308*** (0.00459)	-0.0242*** (0.00416)	-0.000770 (0.000793)
Number of observations	452,734	452,645	130,843	130,843	7,514,955
R <sup>2</sup>	0.916	0.911	0.889	0.885	0.881
<i>p</i> -value <i>z</i> -test	—	0.075	—	0.286	0.000
<b>Specification C: Properties with 1 wind turbine &lt;3km in 2022</b>					
Wind turbines placed <2km	-0.0169*** (0.00306)	-0.0131*** (0.00323)	-0.0123*** (0.00418)	-0.0123*** (0.00388)	-0.00116 (0.000835)
Number of observations	337,872	337,845	100,885	100,885	5,399,714
R <sup>2</sup>	0.924	0.919	0.892	0.887	0.885
<i>p</i> -value <i>z</i> -test	—	0.392	—	0.998	0.009
<b>Specification D: Repeated sales with 1 wind turbine &lt;3km in 2022</b>					
Wind turbines placed <2km	-0.0169*** (0.00338)	-0.0134*** (0.00341)	-0.0182 (0.0126)	-0.0195* (0.0113)	0.000051 (0.000980)
Number of observations	156,977	156,900	11,499	11,499	5,398,363
R <sup>2</sup>	0.976	0.976	0.971	0.978	0.974
<i>p</i> -value <i>z</i> -test	—	0.465	—	0.940	0.149

*Notes:* The matched sales-list sample covers transactions between 1985-2022, the matched sales-assessed sample covers transactions between 2014-2022, the full sample of assessed value covers the period 2014-2022. In all specifications, the treatment group comprises houses located within 2km of a wind turbine installed in preceding years, while the control group consists of houses outside this 2km range. The control variables used in every specification include housing characteristics, such as size, type of housing, dummy for garden, dummy for maintenance condition, and construction period. The control variables for the full sample of assessed values, however, do not include dummy for garden and dummy for maintenance condition. Municipality×Year fixed effects are used in every models. *z*-tests are conducted to test the equality of coefficients by comparing column (2) to (1), and columns (4) and (5) to (3). Standard errors are clustered at 6-digit postcode level and in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

matched samples. As Dröes and Koster (2016) points out, it is primarily the first wind turbine that has a significant impact on nearby housing prices. We therefore analyze subsamples restricted to observations with only one wind turbine within specific distances (5 km and 3 km) in 2022, which also helps to avoid complications arising from multiple treatments. In the final specification (D), we use property fixed effects instead of postcode fixed effect, restricting the sample to properties that sell multiple times. Again, we conduct post-estimation *z*-tests to assess significant differences in capitalization effects between models with different outcome variables. The regression results are reported in Table 8.

Notwithstanding the changes to the data set and the specification, our estimates of sales price capitalization effects are qualitatively similar and quantitatively within a close range of the original study. In the matched sample of sales prices and list prices, we find almost comparable capitalization effects in every specifications, ranging around 1.2 to 2.6%. The corresponding capitalization effect estimates from list prices are, on average, about 20% smaller.

When using the matched sample of sales prices and assessed values, the capitalization effects at the baseline model become smaller, likely due to the shorter observation period (Panel A). However, when restricting to properties with only one wind turbine within a specific range in 2022, the coefficients for sales prices the 2014-2022 period are broadly comparable to those obtained using the full 1985-2022 sample (Panels B-D). The estimated capitalization effects based on assessed values in the matched sample are very similar to those based on sales price. For properties that are actually transacted, assessed values tend to be quite accurate, and differences relative to sales prices likely only comes from different timing of transaction and assessment.

However, extending the analysis based on assessed values to the full sample reduces the magnitude of the point estimates. In the more credible specifications, we find precisely estimated zero effects. This is, perhaps, not surprising as assessed values are generally derived from previous transaction prices of nearby properties. When no recent market transactions exist in the area, these assessed values are much more prone to measurement error. Specifically, areas near wind turbines often have 30% lower population densities and few comparable sales exist (only around 1% of properties in the full sample are transacted).

In conclusion, across all specifications and outcomes, the overall impression is that the capitalization effects from list prices and especially assessed values are smaller in magnitude. The coefficients from list price models are generally not significantly different from those based on sales prices, whereas estimated using assessed values differ significantly in most cases. This supports our expectation, outlined in Section 3, that these measures—particularly assessed values—are less reliable substitutes when the identifying variation is spatiotemporal rather than cross-sectional.

### 4.3 Transit accessibility and house prices in New York City

We now turn to our New York City data set to replicate the analysis in [Gupta et al. \(2022\)](#), who examined the impact of the Second Avenue Subway (Q-line) extension. In this study, we focus on observations within the Upper East Side, defined as the area between 5<sup>th</sup> and 100<sup>th</sup> streets between Fifth Avenue and the East River. We extend the 2003-2019 study period from the original study to include all years through 2023. We continue to use two matched samples: sales prices matched to list prices, sales prices matched to assessed

values. Tax class 1 properties are excluded from the analysis with matched sample due to the low number of observations, yet we still conduct the analysis using the full sample of assessed values.<sup>16</sup> All sales prices, list prices, and assessed values are adjusted for inflation using the Consumer Price Index, with December 2017 as the base period. Outliers are then excluded (see Section 2).

This application uses a difference-in-difference approach to estimate the impact of subway extension. The treatment area is defined as the “2<sup>nd</sup> Avenue Corridor”, covering properties located between 59<sup>th</sup> and 100<sup>th</sup> streets and between First and Third Avenues. The control area consists of the remainder of the Upper East Side. The post-treatment period is defined as after 2013. Table E3 provides descriptive statistics by treatment and control groups. Our preferred specification is given by:

$$\log p_{it} = \alpha + \beta_1 a_i \mu_t + \gamma_1 a_i + \delta_1 \mu_t + \theta z_{it} + \gamma_2 c_t + \beta_2 a_i c_t + \epsilon_{it} \quad (9)$$

where  $p_{it} \in \{p_{it}^s, p_{it}^l, p_{it}^a\}$  denote, respectively, the per m<sup>2</sup> sales price, list price, or assessed values of property  $i$  in year  $t$ ,  $a_i$  is dummy taking the value of one if property  $i$  is located within the 2<sup>nd</sup> Avenue corridor,  $\mu_t$  is a dummy indicating the post-treatment period, with  $c_t$  similarly indicating the 2007-2012 construction period. The coefficient of interest,  $\beta_1$ , measures the average post-treatment impact of the subway extension on treated properties, controlling for property characteristics captured in the vector  $z_{it}$  and anticipation effects ( $\beta_2 a_i c_t$ ).

We present the regression results in Table 9, starting with the simpler model that excludes the construction period control (Specification A), followed by the extended model that includes this control (Specification B) for comparison. Again, we report post-estimation  $z$ -tests for equality of coefficients of the coefficient of interest ( $\beta_1$ ). At 5.6%, the estimated impact of the 2<sup>nd</sup> Avenue Subway is close to the more demanding models controlling for anticipation effects in the original study. It is worth pointing out that controlling for anticipation effects increases the treatment effect (compared to Specification A). This is the expected result for positive anticipation effects, which—if unaccounted for—enter the pre-period, confounding the difference-in-differences comparison. While we cannot statistically reject that the difference in coefficients is zero, it is noteworthy that the magnitude of the reduction is similar to what we have found in our applications in the Netherlands.

When using the list prices in our preferred Specification B, the treatment effect drops

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<sup>16</sup>Condominiums and cooperatives in Upper East Side area are typically classified under tax class 2 and 4, as they are multi-unit residential buildings. Some tax class 1 properties may also be categorized as condominiums and cooperatives (e.g., tax class 1A including 1-3 story condominiums originally a condo). There are 15 observations of tax class 1 properties in the matched sample, and 254 observations in the full sample of assessed values.

TABLE 9 – SUBWAY EXTENSIONS AND HOUSE PRICES IN NYC

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Sample:</i>	Matched sales-list		Matched sales-assessed		Full sample (tax class 2 & 4)	Full sample (tax class 1)
<i>Dependent variable:</i>	Log sales price/m <sup>2</sup>	Log list price/m <sup>2</sup>	Log sales price/m <sup>2</sup>	Log assessed value/m <sup>2</sup>	Log assessed value/m <sup>2</sup>	Log assessed value/m <sup>2</sup>
<b>Specification A: Baseline</b>						
Post×On 2 <sup>nd</sup> Ave	0.0954*** (0.0137)	0.0637*** (0.0103)	0.0397*** (0.0127)	-0.0988*** (0.0182)	-0.0618*** (0.0209)	0.139 (0.0852)
Post	-0.00496 (0.00809)	0.0168*** (0.00543)	0.0411*** (0.00813)	0.253*** (0.0151)	0.241*** (0.0117)	0.295** (0.0934)
Number of obs.	21,772	21,772	34,738	34,738	222,262	254
R <sup>2</sup>	0.707	0.723	0.625	0.615	0.751	0.708
<i>p</i> -value of <i>z</i> -test	—	0.0646	—	0.586	0.000	0.251
<b>Specification B: With anticipation effect</b>						
Post×On 2 <sup>nd</sup> Ave	0.0663** (0.0288)	0.0457* (0.0243)	0.0564*** (0.0138)	-0.149*** (0.0325)	-0.157*** (0.0293)	0.127 (0.0890)
Post	0.0501*** (0.0135)	0.0612*** (0.0112)	0.0606*** (0.00762)	0.330*** (0.0218)	0.356*** (0.0181)	-0.242* (0.107)
Constr. Period ×On 2 <sup>nd</sup> Ave	-0.0458 (0.0324)	-0.0283 (0.0287)	0.0227 (0.0145)	-0.0907*** (0.0314)	-0.130*** (0.0383)	
Constr. Period	0.0820*** (0.0174)	0.0633*** (0.0138)	0.0288*** (0.00979)	0.137*** (0.0203)	0.165*** (0.0153)	-0.545*** (0.0358)
Number of obs.	21,772	21,772	34,738	34,738	222,267	254
R <sup>2</sup>	0.708	0.724	0.626	0.635	0.760	0.713
<i>p</i> -value of <i>z</i> -test	—	0.000	—	0.000	0.000	0.434

*Notes:* The data covers period 2003-2023. “Post” is an indicator for the period after 2013. “Construction Period” is an indicator for the period 2007-2012. Control variables include an indicator for a condo, number of bedrooms, number of bathrooms, the floor of the building, an indicator variable for built before 1942, an indicator variable for built within 10 years of sale, indicator variables for building amenities (exterior space, view, landmark, parking, fireplace, laundry room, pool), and indicators if control variables are missing. Building fixed effects are used in every model. *z*-tests are conducted to test the equality of “Post×On 2<sup>nd</sup> Ave” interaction coefficients by comparing column (2) to (1), and columns (4) to (6) to (3). Standard error is clustered at census block level and in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

by 20%. The significance level decreases from 1% to 10%. For the model excluding anticipation effects, we find that the treatment effect decreases when estimated from list prices. This is consistent with a negative capitalization effect during the construction period. This negative anticipation effect for positive changes is unusual in the context of real estate markets and also inconsistent with the results in the original study. It may itself result from failure to capture short-run changes due to sluggish adjustment in list prices.

These results hold regardless of whether we control for anticipation effects or restrict the analysis to the matched sample or the full sample. This result goes against a large literature on positive rail capitalization effects and is economically implausible. When we run the analysis with the full sample of tax class 1 properties (Column 6), which are proved

in previous sections to track more closely the market prices than tax classes 2 and 4, we obtain a positive average treatment effect. This findings align in sign with the estimates based on sales prices and is more reasonable from an economic perspective. However, it remains statistically insignificant, which is likely due to the small number of observations in tax class 1. Overall, it should be seen as a warning sign reinforcing our notion that assessed values should be avoided in research designs that exploit spatiotemporal variation.

## 5 Conclusion

In this paper, we aim to provide guidance on when list prices and assessed values are suitable alternatives to sales prices in spatial economics research. One key conclusion is that all three measures perform well in capturing cross-sectional variation. This makes assessed values a useful ingredient in the quantification of quantitative spatial models. Since assessed values often provide more comprehensive coverage, they may even be seen as an attractive alternative in micro-geographic applications, even when sales prices are available. However, for research designs that exploit spatiotemporal variation—such as difference-in-differences or event studies—assessed values are less well suited due to their sluggish adjustment to market changes. This limitation is particularly pronounced when assessments are based on income-based appraisal methods rather than transaction data. While list prices also do not perfectly capture transaction prices, they do so to a much lesser extent and are the more obvious alternative when researchers wish to estimate hedonic implicit prices from variation over time, although caution is still needed due to likely attenuation bias.

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# ONLINE APPENDIX

## A Housing studies with alternative price indicators

Table A1 presents a selection of housing studies that use alternative price measures, namely list prices or assessed values instead of sales prices. In rental market, list prices are also commonly used when actual rent is not available.

TABLE A1 – STUDIES WITH ALTERNATIVE HOUSING PRICE MEASURE

Paper	Study area	Price measure	Design
<i>Sales market</i>			
Darling (1973)	Oakland, San Diego, USA	Sales prices and Assessed values	Cross-sectional
Doss and Taff (1996)	Ramsey County, Minnesota, USA	Assessed values	Cross-sectional
Zabel and Kiel (2000)	Chicago, Denver, Philadelphia, Washington D.C, USA	Owner-estimated value	Cross-sectional
Tajima (2003)	City of Boston, USA	Assessed values	Cross-sectional
Hess and Almeida (2007)	City of Buffalo, USA	Assessed values	Cross-sectional
Nicholls and Crompton (2007)	Pebble Creek, Texas, USA	Sales prices and Assessed values	Cross-sectional
Lee et al. (2008)	Tarrant County, USA	Assessed values	Cross-sectional
Cervero and Kang (2011)	Seoul, Korea	Assessed values	Cross-sectional
Mejia-Dorantes et al. (2011)	Madrid, Spain	List prices	Cross-sectional
Bauer et al. (2017)	Germany	List prices	Temporal variation
Kholodilin et al. (2017)	Berlin, Germany	List prices	Cross-sectional
Winkey (2017)	Frankfurt, Germany	List prices	Temporal variation
Cunha and Lobao (2021)	Lisbon and Porto, Portugal	Assessed values	Temporal variation
<i>Rental market</i>			
Thomschke (2015)	Berlin, Germany	List prices	Cross-sectional
Breidenbach et al. (2022)	Berlin, Germany	List prices	Temporal variation
Duso et al. (2024)	Berlin, Germany	List prices	Temporal variation

## B New York City sales prices versus assessed values with only residential buildings

We present the analysis restricted to residential-only buildings in New York City. Descriptive statistics are provided in Table B1. Compared to the main analysis (Table 2), the sample size decreases by 10.6%, primarily due to the exclusion of tax class 2 and 4 properties.

The analysis follows the same approach as in the full sample. First, we decompose the assessed values into cross-sectional and temporal components. Next, we estimate bivariate regressions, where the dependent variable is the postcode fixed effects of sales prices, and the independent variable is the corresponding postcode fixed effect of assessed values. The results, presented in Table B2, are broadly comparable to those from the main analysis (Table 6).

Finally, the results from the bivariate regressions using year fixed effects are presented in Table B3. These findings are consistent with the main analysis (Table 6). All together, it is suggested that restricting the sample to residential-only buildings does not substantially alter the key relationships across space and over time.

TABLE B1 – DESCRIPTIVE STATISTICS FOR NEW YORK CITY (RESIDENTIAL-ONLY BUILDINGS)

	N	Mean	S.d.	Min	Max
<b>Panel A: Full sample</b>					
Sales price/m <sup>2</sup> (in \$)	626,254	8,475	8,691	1,000	99,958
Tax class 1 assessed value/m <sup>2</sup> (in \$)	13,637,581	3,955.65	2,205.98	500	99,959
Tax class 2 assessed value/m <sup>2</sup> (in \$)	3,086,111	2,304.41	1,450.97	500	93,791
<b>Panel B: Matched sample</b>					
<i>Panel B.1: Tax class 1 sales price and assessed value matched sample</i>					
Sales price/m <sup>2</sup> (in \$)	191,000	4,239.32	2,812.82	1,000	96,464
Assessed value/m <sup>2</sup> (in \$)	191,000	4,247.08	2,407.77	500	92,296
<i>Panel B.2: Tax class 2 and 4 sales price and assessed value matched sample</i>					
Sales price/m <sup>2</sup> (in \$)	79,873	7,160.75	6,507.47	1,000	99,908
Assessed value/m <sup>2</sup> (in \$)	79,873	1,447.42	1,099.73	500	14,350

*Notes:* The dataset covers the period 2003 to mid-2023. The data for assessed values only include residential-only buildings. Tax class 1 includes up to three-unit residential properties. Tax class 2 and 4 includes residential properties with more than three units, including coop and condo.

TABLE B2 – CROSS-SECTIONAL CORRELATIONS (NYC, RESIDENTIAL-ONLY BUILDINGS)

	(1)	(2)	(3)	(4)
<i>Panel:</i>	<i>Full sample</i>		<i>Matched samples</i>	
Sub-sample	Tax class 1	Tax class 2 & 4	Tax class 1	Tax class 2 & 4
Postcode FE assessed value	0.974*** (0.0242)	1.180*** (0.0490)	1.107*** (0.0249)	0.912*** (0.0670)
Constant	0.618*** (0.195)	0.138 (0.374)	-0.456** (0.206)	2.144*** (0.481)
Number of observations	167	167	167	141
R <sup>2</sup>	0.959	0.728	0.925	0.588
<i>p</i> -value <i>t</i> -test coef. = 1	0.2881	0.0003	0.0002	0.1890

*Notes:* Dependent variable is postcode FE related to sales prices. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE B3 – TEMPORAL CORRELATIONS, (NYC, RESIDENTIAL-ONLY BUILDINGS)

	(1)	(2)	(3)	(4)
<i>Panel:</i>	<i>Full sample</i>		<i>Matched sample</i>	
Sub-sample	Tax class 1	Tax class 2 & 4	Tax class 1	Tax class 2 & 4
Year FE assessed value	0.903*** (0.0525)	1.335*** (0.218)	1.040*** (0.0628)	1.087*** (0.141)
Constant	0.186*** (0.0205)	0.473*** (0.0642)	0.186*** (0.0220)	0.236*** (0.0633)
Number of observations	21	21	21	21
R <sup>2</sup>	0.928	0.553	0.915	0.834
<i>p</i> -value <i>t</i> -test coef. = 1	0.0801	0.1416	0.5289	0.5432

*Notes:* Dependent variable is year FE related to sales prices. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

## C Distributed lag models

### C.1 The response of sales prices to list prices.

In the main analysis, we have discovered that list prices do not fully capture past sales prices. This can be due to the forward-looking strategy in listing. Earlier studies based on Granger causality tests show that lagged list prices help predict sales prices more than the reverse direction (Knight et al., 1994; Lyons, 2019). To further examine this, we therefore run a reversed specification of the distributed lag model which goes as:

$$\hat{\theta}_t^s - \hat{\theta}_{t-1}^s = \alpha + \sum_{k=0}^K \beta_k (\hat{\theta}_{t-k}^\ell - \hat{\theta}_{t-k-1}^\ell) + \epsilon_t \quad (\text{C1})$$

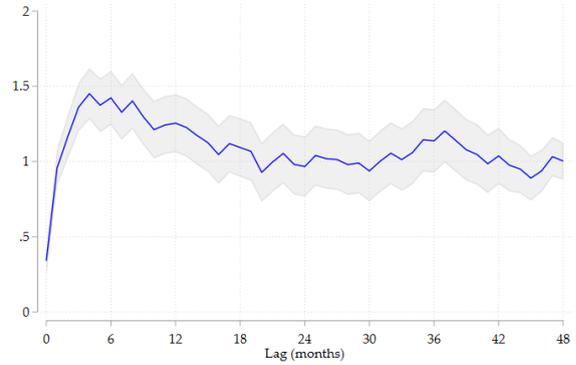
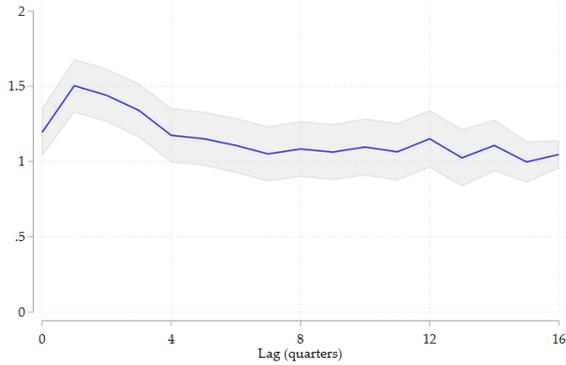
in which the dependent variable is the quarterly or monthly change in the fixed effects for sales prices, while the explanatory variables are lagged quarterly or monthly changes in the fixed effects for list prices. The estimated cumulative impulse response is illustrated in Figure C.1.

In both markets, sales prices respond positively to changes in list prices, yet the speed and completeness of adjustment differ across temporal frequency. At the quarterly frequency, the response in the Netherlands is strong and immediate, exceeding 1 at the contemporaneous response. The New York City case displays a slower, more moderate adjustment with a wider confidence interval. At the monthly frequency, the same pattern holds. In the Netherlands, there is an immediate response of sales prices to changes in list prices after 1 month. The response of sales prices in New York City also rises steadily, yet at a slower pace. Overall, these results show that list prices exhibit signals to lead sales prices, consistent with a forward-looking pricing strategy in which sellers incorporate expectations about future market conditions.

### C.2 Adjustment during market cycle phases.

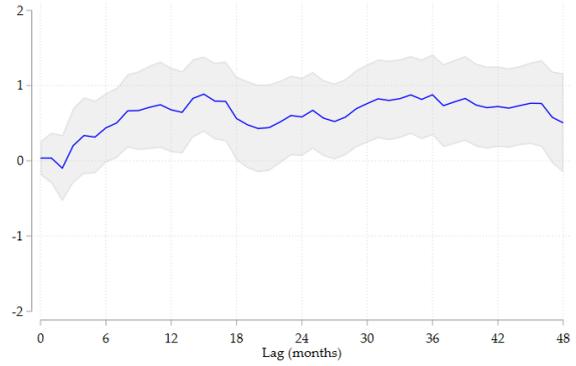
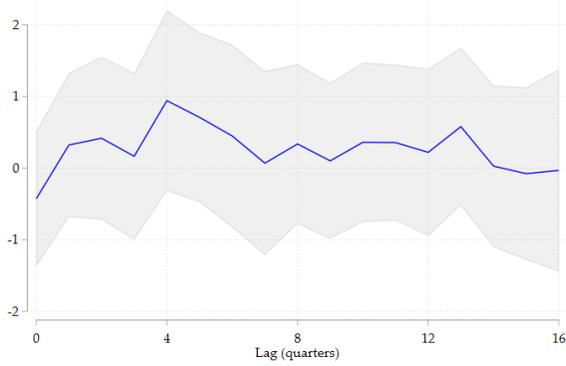
To assess differences in adjustment dynamics across market cycle phases, we separately estimate the cumulative impulse response of list price for the recession and the recovery periods at quarterly frequency. For the Netherlands, the downturn spans 2010-2012, followed by a recovery in 2013-2015. The cumulative impulse response graphs for each phase are presented in Figure C.2. In the case of New York City, the downturn covers 2008-2011 and the recovery is 2012-2015.

We find that during the downturn in the Netherlands (2010-2012), list prices exhibit a relatively strong contemporaneous response, reaching one after three quarters before



(A) CIR OF SALES PRICES AT QUARTERLY FREQUENCY (NL)

(B) CIR OF SALES PRICES AT MONTHLY FREQUENCY (NL)



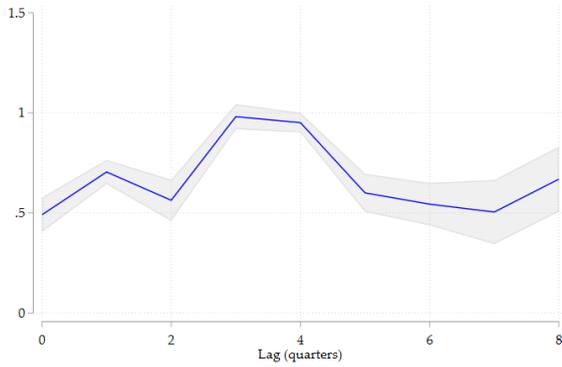
(C) CIR OF SALES PRICES AT QUARTERLY FREQUENCY (NYC)

(D) CIR OF SALES PRICES AT MONTHLY FREQUENCY (NYC)

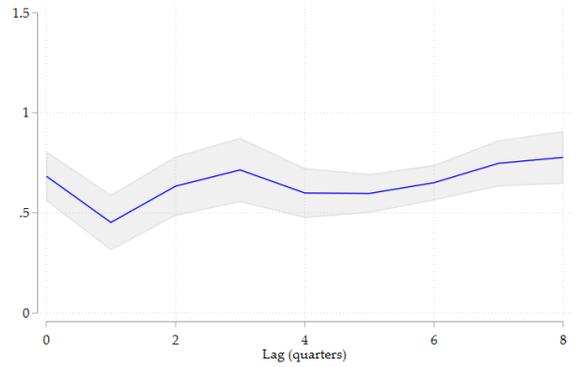
*Notes:* The figure displays cumulative impulse response of changes in sales price to changes in list price at quarterly frequency (Figures C.1a and C.1c) and yearly frequency (Figures C.1b and C.1d). Estimates are obtained from Equation C1 using the time fixed effects derived from prior price decomposition. The standard errors are Newey-West standard errors with automatic bandwidth selection.

FIGURE C.1 – CUMULATIVE IMPULSE RESPONSE OF SALES PRICES AT QUARTERLY AND MONTHLY FREQUENCY

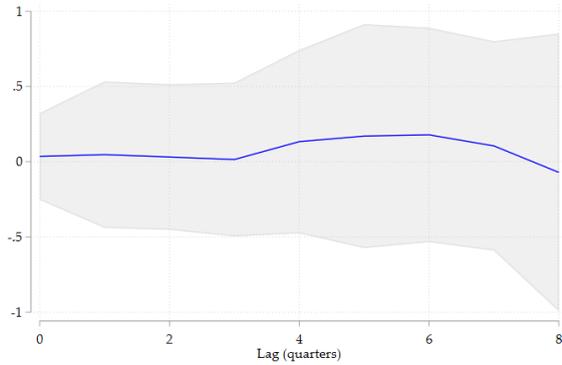
declining thereafter. During the recovery (2013-2015), the cumulative impulse response becomes smoother but less pronounced, stabilizing around 0.7. This pattern suggests that list prices adjust with a short lag during market turning points, and that this lag is more noticeable during recoveries. In New York City, we find the cumulative impulse response fluctuates around zero and remain statistically insignificant in both phases. This indicates a weak short-run adjustment of list prices to changes in sales prices, suggesting that sellers rely more on expectations or comparable listings than on recent sales prices when setting asking prices.



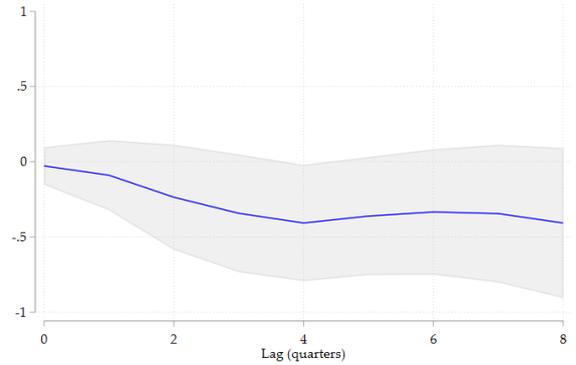
(A) CIR OF LIST PRICES IN 2010-2012 (NL)



(B) CIR OF LIST PRICES IN 2013-2015 (NL)



(C) CIR OF LIST PRICES IN 2008-2011 (NYC)



(D) CIR OF LIST PRICES IN 2012-2015 (NYC)

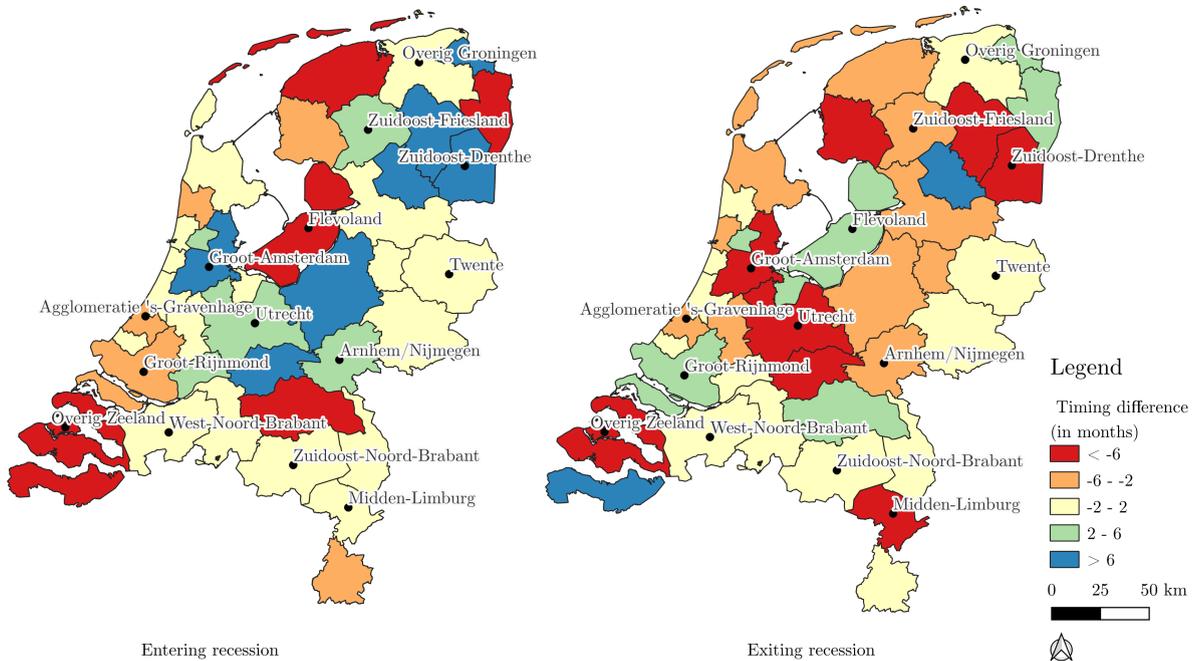
*Notes:* The figure displays cumulative impulse response of changes in list prices to changes in sales prices at quarterly frequency during the recession (Figure C.2a) and recovery (Figure C.2b) phases in the Netherlands, as well as the recession (Figure C.2c) and recovery (Figure C.2d) phases in New York City. Estimates are obtained from Equation 4 using the time fixed effects derived from prior price decomposition. The standard errors are Newey-West standard errors with automatic bandwidth selection.

FIGURE C.2 – CUMULATIVE IMPULSE RESPONSE OF LIST PRICE AT QUARTERLY FREQUENCY DURING MARKET CYCLE PHASES

## D Spatial distribution of timing difference in turning points in list and sales prices

Figure D.1 illustrates the spatial variation in the timing differences between turning points in list prices and sales prices across Travel-To-Work-Areas in the Netherlands. The left panel shows differences when entering the recession, and the right panel shows recession exiting period.

It is revealed that regional patterns do not always persist across downturn and recovery phases. Some areas where list prices initially led during the downturn (e.g., Groot Amsterdam, Utrecht) show a lag during the recovery. This suggests asymmetry in list price adjustment in different market phases.



*Notes:* The figure displays timing differences between turning points in list and sales prices per Travel-To-Work-Area. This is derived from piecewise linear spline model (Equation 5). Positive values indicate that list prices lead sales prices. Negative values indicate that list prices lag sales prices.

FIGURE D.1 – SPATIAL DISTRIBUTION OF TIMING DIFFERENCES BETWEEN TURNING POINTS IN LIST AND SALES PRICES (THE NETHERLANDS)

## E Descriptive statistics for replication

This appendix provides descriptive statistics of the datasets used in the replication analyses.

### E.1 Historic amenities and sorting of households

TABLE E1 – DESCRIPTIVE STATISTICS - HISTORIC AMENITIES AND HOUSE PRICES

	(1) Mean	(2) S.d.	(3) Min	(4) Max
<b>Panel A: Matched sample</b>				
Sales price/m <sup>2</sup> (in €)	2,863	937.5	514.7	5,000
List price/m <sup>2</sup> (in €)	2,902	897.1	551.5	6,910
Assessed value/m <sup>2</sup> (in €)	2,655	843.0	379.7	8,526
In historic district	0.316	0.465	0	1
View on historic district	0.183	0.387	0	1
<b>Panel B: Full sample</b>				
Assessed value/m <sup>2</sup> (in €)	2,718	1,400	50.12	85,108
In historic district	0.298	0.458	0	1
View on historic district	0.175	0.380	0	1

*Notes:* We only keep properties that are within 200 meters of a historic district boundary. The number of observations in matched sample is 34,639. The number of observation in full sample is 2,217,758. The data is from 2014 to 2022. The list prices are from 2004 to 2022.

### E.2 Wind turbines and housing values

TABLE E2 – DESCRIPTIVE STATISTICS - WIND TURBINES AND HOUSE PRICES

	(1) N	(2) Mean	(3) S.d.	(4) Min	(5) Max
<b>Panel A: Matched sample</b>					
<i>Sample A.1: Sales price and list price</i>					
Sales price/m <sup>2</sup> (in €)	4,029,284	2,014	1,112	115	27,109
List price/m <sup>2</sup> (in €)	4,029,284	2,079	1,083	119	24,875
Sold with wind turbines placed <2km	4,029,284	0.0538	0.2257	0	1
Listed with Wind turbines placed <2km	4,029,284	0.0534	0.2249	0	1
<i>Sample A.2: Sales price and assessed value</i>					
Sales price/m <sup>2</sup> (in €)	1,192,873	2,648	1,209	227	16,300
Assessed price/m <sup>2</sup> (in €)	1,192,873	2,442	1,067	65	53,273
Sold and assessed with wind turbines placed <2km	1,192,873	0.0774	0.2672	0	1
<b>Panel B: Full sample</b>					
Assessed value/m <sup>2</sup> (in €)	63,800,824	2,312	1,199	50	98,707
Assessed with wind turbines placed <2km	63,800,824	0.0827	0.2754	0	1

*Notes:* The matched sample of sales price and list price includes transaction between 1985-2022, list price is from 1980 onwards. The matched sample of sales price and assessed value includes transaction between 2014-2019. The full sample of assessed value covers the period 2014-2019.

### E.3 Transit accessibility and house prices

TABLE E3 – DESCRIPTIVE STATISTICS - TRANSIT ACCESSIBILITY AND HOUSE PRICES

	N	Mean	S.d.	Min	Max
<b>Sample A: Matched sample</b>					
<i>Sample A.1: Sales price and list price</i>					
Sales price/m <sup>2</sup> (in \$)	21,829	13,403	6,455	2,154	190,746
List price/m <sup>2</sup> (in \$)	21,832	14,449	7,461	864.2	169,210
On 2 <sup>nd</sup> Ave.	21,829	0.37	0.48	0	1
Condominium	21,829	0.35	0.48	0	1
Coop	21,829	0.65	0.48	0	1
<i>Sample A.2: Sales price and assessed value</i>					
Sales price/m <sup>2</sup> (in \$)	34,769	13,442	7,799	1,020	190,746
Assessed value/m <sup>2</sup> (in \$)	34,769	3,301	3,794	505	133,053
On 2 <sup>nd</sup> Ave.	34,769	0.36	0.48	0	1
Condominium	34,769	0.33	0.47	0	1
Coop	34,769	0.67	0.47	0	1
<b>Sample B: Full sample</b>					
<i>Sample B.1: Tax class 1</i>					
Assessed value/m <sup>2</sup> (in \$)	254	15,417	10,160	2,216	91,889
On 2 <sup>nd</sup> Ave.	254	0.276	0.448	0	1
Condominium	254	0.764	0.426	0	1
Coop	254	0.0276	0.164	0	1
<i>Sample B.2: Tax class 2 and 4</i>					
Assessed value/m <sup>2</sup> (in \$)	222,273	2,308	2,071	213.7	132,755
On 2 <sup>nd</sup> Ave.	222,273	0.349	0.477	0	1
Condominium	222,273	0.239	0.427	0	1
Coop	222,273	0.756	0.429	0	1

Notes: The dataset covers transactions during the period 2003-2023. The list prices are from 2000 to 2023.

## F Replication

This appendix presents replication results for some findings in the three studies to assess the suitability of our dataset. Each replication follows the specification and time span of the original paper as closely as possible, using our processed data. Section F.1 replicates the analysis of [Koster et al. \(2016\)](#), which studies the effect of historic district on house prices in the Netherlands between 2002 and 2011. Section F.2 replicates the design of [Dröes and Koster \(2016\)](#), which examine the impact of wind turbines and solar farms on Dutch residential property prices using data from 1985 to 2019. For both cases, the estimates obtained using our dataset are consistent in sign, magnitude, and statistical significance with the original studies. Section F.3 replicates the findings of [Gupta et al. \(2022\)](#), which analyze the impact of the subway expansion on house prices in New York City. While our replication relies on a different data source, the estimated treatment effects remain aligned in terms of sign and are of comparable magnitude to those reported in the original study.

### F.1 Historic amenities and sorting of households

TABLE F1 – HISTORIC AMENITIES AND HOUSE PRICES—REPLICATION

	(1)	(2)
	Full sample	$\leq 500\text{m}$ to boundary
In historic district	0.145*** (0.014)	0.034*** (0.011)
Municipality fixed effects	Yes	No
Historic district boundary fixed effects	No	Yes
Year fixed effects	Yes	Yes
Number of observations	595,127	68,162
R <sup>2</sup>	0.600	0.656

*Notes:* Dependent variable is Log of sales price per m<sup>2</sup>. We use hedonic pricing model to measure how being located in historic district impacts on housing prices in the Netherlands. The treatment group consists of properties inside the historic district, and the control group consists of properties outside the district. Housing characteristics include size, type of housing, dummy for garden, dummy for maintenance condition, and construction period. The data is from 2002 to 2011. We only include houses that are constructed after 1970 and are therefore not listed. In column (1), we use the full sample. In column (2), we only include houses located within 500 meters of a historic district boundary. Standard errors are clustered at the postcode four-digit level and in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## F.2 Wind turbines, solar farms and housing values

TABLE F2 – WIND TURBINES, AND HOUSE PRICES—REPLICATION

	(1)	(2)
	Full sample	≤ 5km in 2019
Wind turbines placed <2km	-0.0197*** (0.00346)	-0.0297*** (0.00377)
PC6 fixed effects	Yes	Yes
Year and month fixed effects	Yes	Yes
Number of observations	3,516,070	1,542,107
R <sup>2</sup>	0.918	0.919

*Notes:* Dependent variable is Log of sales price per m<sup>2</sup>. We use difference-in-differences models to estimate the impact of wind turbines on housing prices in the Netherlands. The data is from 1985 to mid-2019. Column (1) use the full sample as the baseline analysis, column (2) use the subsample of observations with wind turbine within 5km in 2019. The treatment group includes houses within 2km of a wind turbine installed in preceding years, the control group includes houses outside this range. Controls include housing size, type of housing, dummy for garden, dummy for maintenance condition, and construction period. Controls, postcode six-digit fixed effects, year and month fixed effects are used in every model. Standard errors are clustered at the neighborhood level and in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

## F.3 Transit accessibility and house prices

TABLE F3 – SUBWAY EXTENSIONS AND HOUSE PRICES IN NYC—REPLICATION

	(1)	(2)
	Baseline	With anticipation
Post×On 2 <sup>nd</sup> Ave	0.0228 (0.0175)	0.0349 (0.0312)
Post	0.118*** (0.00857)	0.156*** (0.0120)
Constr. Period × On 2 <sup>nd</sup> Ave		0.0204 (0.0350)
Constr. Period		0.0651*** (0.0131)
Building fixed effects	Yes	Yes
Number of observations	33,746	33,746
R <sup>2</sup>	0.835	0.836

*Notes:* Dependent variable is Log of sales price. We use difference-in-differences models to estimate the impact of the Q-train subway extension along the 2<sup>nd</sup> Avenue corridor on house prices. The data cover January 2003 to March 2019. “Post” is an indicator for the period after 2013. “Construction Period” is an indicator for the period 2007-2012. Controls include a dummy for a condo, number of bedrooms, number of bathrooms, the floor of the building, a dummy for built before 1942, a dummy for built within 10 years of sale, dummies for building amenities (exterior space, view, landmark, parking, fireplace, laundry room, pool), and dummies if controls are missing. Standard errors are clustered at the census block level and in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1