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Property Renovations and Their Impact on House Price Index Construction

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March 2017

Working Paper 17-02

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Please address correspondences to William Doerner (william.doerner@fhfa.gov). Many thanks to Andy Leventis and Fred Graham for their comments. No outside funding was received for this research.

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Abstract

This paper provides the first wide-scale analysis of property renovation bias in repeat-sales house price indices across a multitude of U.S. geographies. Property improvements frequently lead to positive quality drift. In local markets, omitting information on property improvements can bias index estimates upwards. Bias often varies in a predictable manner and can distort valuations by as much as 15 percent in the central districts of large cities. This systematic variation in bias is partially a function of the disparate concentration of renovation activity with property improvements occurring more frequently in denser areas. The distortionary effect of not accounting for property renovations tends to decline outside of downtown areas and is generally negligible in smaller cities (populations below 500,000).

Keywords: property renovations · house price indices · repeat-sales approach

JEL Classification: C43 · C55 · C58 · R30

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1. Introduction

House price indices (HPIs) are an important market monitoring tool for assessing the health and efficiency of real estate markets. Both historical and projected HPIs are key model inputs for estimating the credit risk of a portfolio of loans and pricing mortgage backed securities. Given the important and highly influential nature of these indices, it is essential to understand potential biases that may arise during index construction. In what follows, we focus on a single, but potentially impactful source of bias: unobserved property renovations.^{1,2}

A popular and well-established method for index construction is the repeat-sales technique. The basic notion is to calculate average appreciation by selecting individual houses that sell more than once and computing the price change between transactions. While widely used because of its straightforward interpretation and limited data requirements, the repeat-sales estimator relies on a potentially strong assumption that a house's quality or underlying characteristics remain unchanged between successive transactions. This constant-quality assumption allows us to interpret appreciation between transactions as a clear signal of general house price movements.³ If this assumption is violated, the repeat-sales approach can lead to biased index estimates which misstate actual price movements. We examine this concern by identifying property renovations between transactions and quantifying the impact on a standard repeat-sales index.⁴

Existing empirical evidence of systematic renovation bias is currently mixed and largely confined to single city analyses. Palmquist (1980) uses data from King County, WA to compare a hedonic index constructed using housing characteristics contemporaneous to each

¹Property improvements can range from minor renovations like new windows or energy efficient heating to extensive modifications such as additions which substantively increase a finished living space.

²In several countries, home improvement expenditures can exceed overall spending on new residential construction (Fenwick, 2013).

³Constant-quality is an important distinction. Markets are often described by the temporal patterns of mean or median housing values. For that to reflect a reliable measure of market price movements, however, the sample of transacted homes (and the homes themselves) must remain perfectly the same across each period. The repeat-sales approach attempts to address this issue.

⁴In estimating the inflationary impact of home improvements, we focus our attention on quality change gross of any offsetting depreciation. Harding et al. (2007) find that, net of maintenance, housing depreciates at approximately 2 percent per year. To be upfront about a data limitation, we are unable to distinguish renovations by quality, extent, or cost. The magnitude of renovation or depreciation (especially that due to lack of maintenance) could be correlated to local economic cycles and controlling for depreciation would be ideal but is not possible with our data. Our renovation results suggest that distortionary effects are generally modest but largest in downtown areas of large cities.

sale (which implicitly controls for major renovations) with a repeat-sales index estimated from 1962 to 1976. The two indices are statistically equivalent at the 5 percent level in all but one year. Leventis (2007) uses building permit data to identify renovated properties and studies their impact on house price index construction for the city of San Francisco, California from 1991 to 2006. HPIs constructed with and without identified renovations are nearly identical with no evidence of systematic bias.⁵ In contrast, McMillen and Thorsnes (2006) and Bourassa et al. (2013) both find that unobserved property renovations can potentially engender significant bias.⁶ McMillen and Thorsnes (2006) study home sales in the city of Chicago, Illinois from 1993 to 2002 and show that a standard repeat-sales approach overstates cumulative appreciation by almost 9 percent relative to a median-based quantile estimator (constructed to be less sensitive to unobserved property improvements). Bourassa et al. (2013) examine home sales in Louisville, Kentucky from 1998 to 2010 and conclude that a standard repeat-sales index can bias values by up to approximately 14 percent relative to a benchmark index which explicitly controls for flips and real estate owned transactions.⁷ Unfortunately, it is difficult to pinpoint the cause of the residual error—whether it is due to a transaction having a physical improvement, some kind of investment time sensitivity, or a pricing arbitrage opportunity due to the mortgage’s disposition.

We offer two additions to the literature on property renovations and house price measurement. First, we identify property renovations across a multitude of U.S. geographies. Second, we link renovations to a property transactions database to identify houses that have physically changed since their last sale. As a result, we do not have to rely on statistical techniques for inferring what leads to residual errors; we can actually measure whether omitting reno-

⁵Due to data constraints, we are unable to replicate the Palmquist (1980) results, but we do find qualitatively similar results to Leventis (2007) when examining adjusted versus unadjusted indices for the San Francisco-Oakland-Hayward, CA CBSA. Not accounting for renovations leads to an average upward bias of 0.08 percent in cumulative appreciation for the period 2000 through 2015. The largest observed distortion is 0.16 percent and occurs in the first quarter of 2015.

⁶We are unable to completely replicate the McMillen and Thorsnes (2006) results, although this may be a function of a different level of geographic aggregation. Our indices are constructed for single family homes sales across the Chicago-Naperville-Elgin, IL-IN-WI CBSA while the McMillen and Thorsnes (2006) indices are constructed for a narrower market area of single family home sales in only the city of Chicago, not the additional surrounding counties. When we explore individual ZIP code level indices within the city of Chicago, we do observe a similar upward bias of as much as 12 percent at those geographic levels. Unfortunately, our MLS data coverage does not include Louisville, Kentucky so we cannot verify the Bourassa et al. (2013) results.

⁷We calculate the 14 percent as the largest gap between their conventional and benchmark indices. The magnitude is the percentage difference between them, which reflects the difference in cumulative appreciation since 1998. This is comparable to the measures we offer later.

vations results in a positive bias in housing market indices.⁸

To accomplish these steps, we utilize real estate listings data with property-level attributes from 110 Multiple Listing Services (MLS) and merge the renovation flags onto a database of 100 million property transactions.⁹ Whereas prior studies have focused on one or a handful of local areas because of data limitations, we are able to make broader generalizations through a wide-scale analysis of property renovation bias across the United States. Figure 1 illustrates the extent of the MLS geographic coverage. Each circle represents a unique real estate market listing dataset and its size corresponds to the number of observed transactions (a larger circle denotes a greater number of listings). The majority of the listings data is confined to transactions occurring after 2000. Markets range from only several thousand total listings to more than several million. When we combine these listings data with property transactions, we can construct the incidence and share of transactions associated with a property renovation. Both numbers have (with few exceptions) steadily increased from 2000 to 2015. Figure 2 compares renovated property sales across two dimensions. Panel (a) shows the considerable seasonality in the sale of renovated properties, with the frequency generally peaking mid-year.¹⁰ Panel (b) illustrates this pattern is qualitatively similar across different types of projects, with changes in the number of bedrooms and baths being the most commonly identified renovations.

Consistent with McMillen and Thorsnes (2006) and Bourassa et al. (2013), we find strong and localized evidence of renovation bias. The persistence and magnitude of this bias often varies in a predictable manner within cities. Failing to account for property renovations can upwardly bias ZIP code-level HPIs by as much as 15 percent in terms of cumulative appreciation in central areas.¹¹ As distance from downtown increases, the distortions

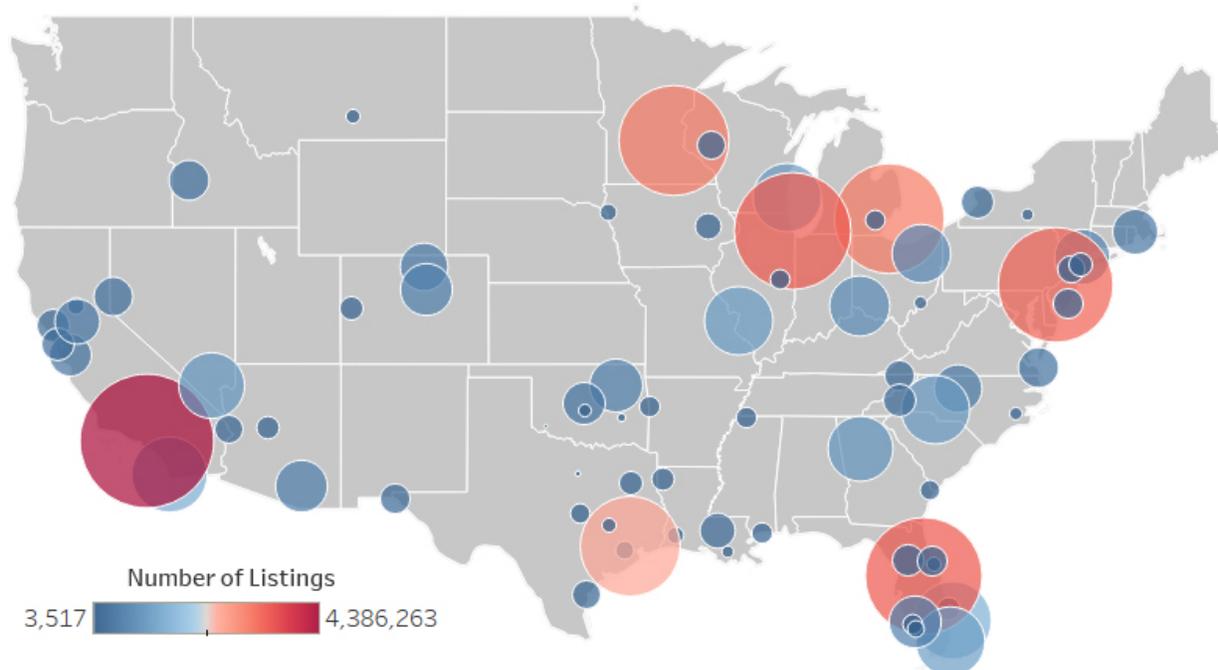
⁸To be clear, we do not intend to compute the implicit price of each type of renovation in a hedonic format nor determine the return on investment of certain improvements. We focus on a specific assumption used in repeat-sales house price index construction to determine if regularly used indices might be biased by not accounting for physical improvements.

⁹The MLS data are licensed from CoreLogic. Coverage is extensive but not complete across the United States. Some major metropolitan areas are either missing or incomplete (e.g., Boston, Memphis, Nashville, New York, Phoenix, Seattle, and Washington, D.C.). We are still able to produce HPIs for more than 4,000 ZIP codes across the country.

¹⁰This is at least partially a function of our identification methodology, which involves pairing sets of listings. Since there are generally more listings during summer months, we are also more likely to identify a renovation mid-year.

¹¹This level of significant upward bias is constrained to select ZIP codes in California, Florida, and Ohio (e.g., Columbus, OH; Canyon Lake, CA; Riviera Beach, FL).

Figure 1: Geographic coverage of MLS data



effect of omitting a property renovation when constructing a house price index for the overall market area declines. This location-based dynamic is particularly pronounced in large cities. In contrast to more localized indices, CBSA level indices are generally robust to the presence of positive outliers or not accounting for renovations, suggesting that more aggregate price measures are relatively immune from the distortionary effects of renovation bias.¹²

The remainder of the paper is structured as follows. Section 2 discusses house price index construction and examines the theoretical consequences of omitting information on property renovations. Section 3 discusses our data sources. Section 4 provides several estimation strategies to measure the bias and concentration of renovations as they relate to a property's location and other demographic effects. Section 5 provides concluding remarks.

¹²Figure 3 provides an example of the differences in adjusted and unadjusted HPI levels when the measures are constructed at the CBSA and ZIP code level in Tampa, FL. A greater renovation bias, or difference between the indices, arises with more localized measures. To emphasize how much variation can be contained in localized indices, Figure 4 contrasts the distribution of renovation bias associated with all ZIP code indices and CBSA indices across the United States. As illustrated, the medians for the renovation bias are visually close and stable for CBSAs and ZIP codes (when comparing adjusted to unadjusted HPIs) but the tails of the distribution become much wider for the ZIP codes at time passes.

2. House price index construction and renovation bias

There are several approaches to constructing HPIs. The choice of a particular methodology is often a function of data availability and whether it is possible to control for differences in property characteristics. Several common techniques are described below.

One of the simplest approaches to index construction is to compute a time series of distributional statistics such as median sales price, which is relatively easy to create and does not require data on housing characteristics. Public organizations, like the United States Census Bureau, release regional indices constructed using median sales prices.¹³ Unfortunately, those indices can provide imprecise estimates of local price movements because median-based indices capture changes in value, which reflect both local price movements and changes in the quality mix or composition of homes in the marketplace. Bourassa et al. (2008) study a median based approach by the Swiss National Bank and find that regional indices, characterized by more pronounced property cycles, are particularly sensitive to changes in composition. Further, during bear markets, lesser quality properties are more likely to sell, which leads value based indices to overstate the extent of local house price declines. These changes in quality can be partially controlled through stratification or mix adjustment, where different types of properties are tracked separately. Each stratum can then be used to proxy for a constant-quality index applicable to a particular property type. If desired, a single mix-adjusted index can be created as a weighted average of each stratum (Wood, 2005). The coarser the stratification, the more likely the final index will be unduly influenced by changes in the quality mix.

Alternatively, changes in the quality mix can be explicitly controlled through a hedonic regression. In a hedonic regression, sales price is modeled as a function of property characteristics and a series of time dummy variables. A renovation might be identified when those characteristics change in quantity or quality. A downside to this type of model is that it is data intensive and requires specifying the functional form and trends in the implicit value of the included property characteristics. A potential for omitted variable bias may arise from unobserved heterogeneity which, in real estate, commonly happens when location-based attributes are not fully captured.¹⁴

¹³Because house price distributions are often positively skewed, a median measure is generally preferred over a mean.

¹⁴Some of this location-specific heterogeneity can be accounted for by introducing location dependence in the hedonic regression (Hill, 2011).

A relatively simple approach exists that can both control for renovations and the unobserved heterogeneity. The repeat-sales methodology resolves time invariant unobserved heterogeneity through chained matching or grouping properties that transact more than once and then examining the price difference between sales. This approach was first introduced by Bailey et al. (1963) and later refined by Case and Shiller (1987) to account for heterogeneity between sales. A decade later, public indices were released for the nation and census divisions by the U.S. government’s Office of the Federal Housing Enterprise Oversight, which was replaced by the Federal Housing Finance Agency (FHFA).¹⁵ We focus on FHFA’s variant of the repeat-sales approach for replicability with existing localized house price indices.¹⁶

Under this framework, house prices are defined by an index value which represents a common price level, a Gaussian random walk which accounts for drifts in individual home values, and a random error or white noise component,

$$\ln(P_{it}) = I_t + R_{it} + N_{it} \quad (1)$$

where the percentage change in value between two periods, $\Delta V \equiv \ln(P_{it}) - \ln(P_{is})$, allows us to ignore in expectations the latter two influences that are purely attributable to idiosyncratic differences across time and space. When house i sells repeatedly its change can be written

$$\Delta V_i = \sum_{\tau=0}^T I_{\tau} D_{i\tau} + \epsilon_i \quad (2)$$

where D is a vector of dummy variables set equal to -1, 0, and 1 to denote the period when a prior or current sale occurs. The distribution of the residuals may suffer from heteroscedasticity as time between sales or other locational characteristics lead to non-constant variance. One empirical solution is, if the dispersion is well-specified, to estimate a second stage with the residuals and then correct with a weighted estimate of the squared deviations.¹⁷

¹⁵Indices can be found online at <https://www.fhfa.gov/hpi>, along with reports, frequently asked questions, and a technical description by Calhoun (1996).

¹⁶Data improvements have been made to include indices made from different data sources and representing other geographies, like states and metropolitan areas. Over the last year, researchers at the FHFA have provided a suite of annual HPIs back to the mid-1970s that cover finer areas, like ZIP codes and census tracts (Bogin et al., 2016). Their data and several related working papers can be found at <https://www.fhfa.gov/papers/wp1601.aspx>. To date, no other source provides HPIs for so many years or local areas.

¹⁷To the extent that extended time between sales is associated with a home renovation, this weighting scheme will decrease the influence of home improvements between sales.

Given this conceptual framework, we express an observed home's value in location j as a linear combination of unit-specific characteristics X with implicit prices β and a price level δ where

$$y_{ijt} = X'_{ijt}\beta_j + D'_{ijt}\delta_{jt} + \epsilon_{ijt} \quad (3)$$

The repeat-sales methodology assumes $E[X_{ijt}] = X_{ijt}$ and gives a differenced equation where

$$\Delta y_{ijt\tau} = \Delta D'_{ijt\tau}\delta_{jt} + \Delta \epsilon_{ijt\tau} \quad (4)$$

but when a property renovation occurs between sales, $E[X_{ijt}] \neq X_{ijt}$, and $\Delta X'_{ijt\tau}\beta_j$ is absorbed into the error term, $\Delta u_{ijt\tau} = \Delta X'_{ijt\tau}\beta_j + \Delta \epsilon_{ijt\tau}$. If the covariance between $\Delta X_{ijt\tau}$ and $\Delta D_{ijt\tau}$ is non-zero, then our estimate of δ will be biased.¹⁸

$$\hat{\delta}_{jt} = \delta_{jt} + \beta_j \frac{Cov(\Delta X_{ijt\tau}, \Delta D_{ijt\tau})}{V(\Delta D_{ijt\tau})} \quad (5)$$

A net increase in quality between sales, $Cov(\Delta X_{ijt\tau}, \Delta D_{ijt\tau}) > 0$, will bias index estimates upwards. We attempt to quantify the extent of this bias across different geographic regions and levels of aggregation by estimating a set of conventional indices and then comparing them to adjusted indices which explicitly account for property renovations between sales.

3. Data for house transactions and property renovations

We begin by constructing a set of conventional repeat-sales indices at a quarterly frequency with house prices drawn from FHFA's property transactions database of purchases and refinance appraisals of single-family homes.¹⁹ The data contain nearly 100 million property transactions drawn from conforming and conventional loans sold or securitized by Fannie

¹⁸This model also assumes the value ascribed to different property characteristics remains constant over time. To the extent that certain attributes become more or less desirable over time, our index estimates will suffer from an additional source of bias that, unfortunately, we cannot control with our data.

¹⁹The HPIs are constructed at a quarterly frequency. For comparative purposes, we construct indices at both the CBSA and ZIP code levels.

Mae and Freddie Mac since 1975.²⁰ These transactions are stacked and sorted by unique address to form over 50 million sales transaction pairs with broad coverage across the United States.

These paired transactions are then linked with a variety of property renovation flags to construct renovation-adjusted indices.²¹ We identify likely property renovations with real estate announcements, which provide information on property-level attributes each time a home is listed for sale. Our MLS data cover over 60 million listings in 36 states and provide a way to track the changes in repeat listings of the same house. We select five key features that are reliably described across listing services: number of bedrooms, number of bathrooms, square footage, lot size, and change in effective year built.²²

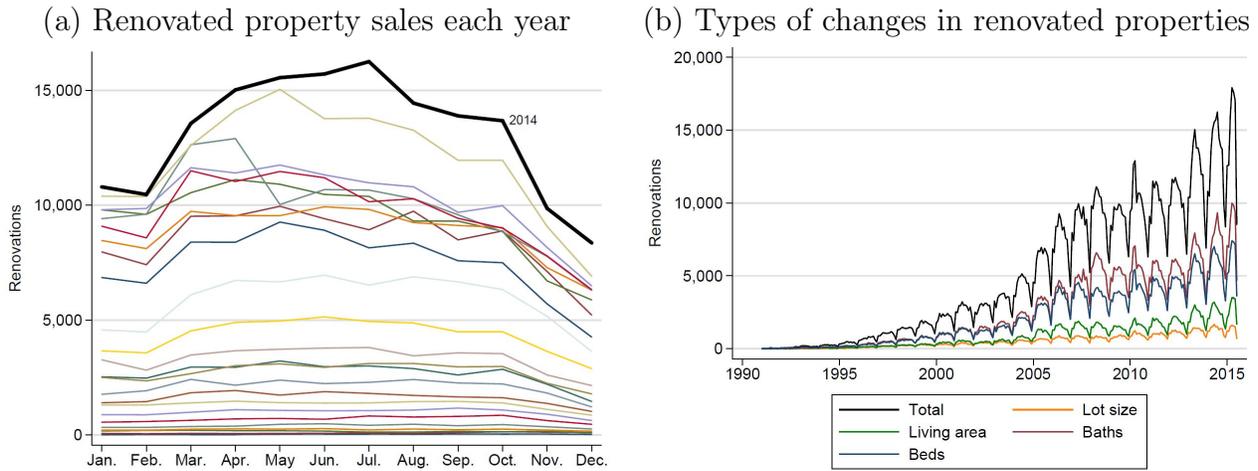
A property renovation is defined as a change in at least one of these five attributes across successive listings. For instance, imagine we observe two listings for 121 Main Street. The first time the house comes up for sale, it is described as having 2 bedrooms, 1.5 bathrooms, and 1500 square feet of living space. The second time the house is advertised as having 3 bedrooms, 2.5 bathrooms, and 2000 square feet of living space. Based upon the physical changes between these listings of the same house, we would flag it as having a renovation

²⁰The data include both sales from market transactions and appraisals from refinancings. The appraisals allow us to increase the sample size and pick up significantly more pairs, like happens with the FHFA all-transactions index. A data challenge, though, is the imperfect timing of the renovation. We identify renovations based on listings in the MLS data, which typically end in a sale but not always. As a result, we are confident a renovation happens between pairings of two sales (because the date falls on or before the most recent sale) but the appraisal and sale pairings are more difficult. For example, when we observe a sale, an appraisal, and another sale we cannot always establish that the renovation occurred before or after the appraisal. In some cases, MLS listings result in a sale that happens before or at the same time of the appraisal and we can successfully assign the renovation to the appraisal. When we do not have a MLS listing between the first and third transaction, we are forced to assign the renovation to the third transaction (the second sale) and not the second transaction (the appraisal).

²¹We pair renovations identified in the MLS data with market values from the FHFA data because of the increased sample size. We could have relied solely on the MLS data to extract sales prices but that would eliminate many of the early period pairings because of limited historical MLS coverage, homes sold directly by owners, and any other transactions not listed with a realtor service that we licensed. Furthermore, a goal of this present study was to determine whether the publicly released FHFA HPIs were susceptible to bias by not accounting for property renovations.

²²A possible critique is that, between listings, a realtor may report certain characteristics (like another bedroom instead of an office) to increase the likelihood of sale. We cover against this possibility by deleting renovations that change back in a later listing and remain fixed.

Figure 2: Trends in renovated properties by year and type



Note: The volumes reflect sales of properties with certain types of renovations in each quarter. The dates do not reflect the dates of the renovations.

between sales.²³ To be clear, this allows us to track *whether* a renovation has occurred but not the *extent* of a renovation.²⁴

Figure 2 shows the number of identified renovated property sales. As illustrated in panel (a), there is a positive summer seasonality in the timing of the renovations and the total number has generally been trending upward since 2000. The most common improvements are increases in the number of bedrooms or bathrooms, as shown in panel (b). Sometimes those changes involve an increase in property size, which is shown in the living area. Lot size changes seldom occur.²⁵ After identifying likely renovations in the MLS data, we merge this information back into our housing transaction pair data using the property location. In the

²³For the purposes of this paper, a change in any of the five attributes counts as a property renovation. In the above example, several attributes changed but they are collectively counted together as a single renovation. The extent and type of renovation could impact the amount of unobserved bias. We are focused, though, on determining whether any effect even exists and reserve later work for exploring the mechanism of the bias. To avoid misidentification, we eliminate anomalies where a property attribute changes between the first two listings but then reverts back in the third paired listing. We also discard changes of less than 250 square feet because there could be a recording difference between realtors or appraisal data being used to populate the field.

²⁴Some listing services provide fields for the quality of particular characteristics or the entire house. We find these fields to be rather subjective and not widely used. Building permits could be used to deduce some of the costs or extent or renovations but that information is not included in the MLS data.

²⁵This is not surprising because a property can be improved internally by submitting any necessary permits and simply conducting the work. A change in lot size, however, necessitates a regulatory appeal to a local planning board to combine or split lots. Such changes occur rather infrequently in practice for single-family lots and that is reflected in the data.

next section, we investigate differences between conventional and adjusted indices to better understand the implications of not controlling for physical renovations.

4. Results

The repeat-sales regression approach can be modified to account for renovations in several ways. For instance, Goetzmann and Spiegel (1995) argue that adding an intercept term to the repeat-sales regression effectively removes the non-temporal component of house price appreciation and mitigates any systematic bias introduced by unobserved renovations. Effectively, this constant term captures the average change in quality across all housing characteristics over the average holding period. While the Goetzmann and Spiegel (1995) approach allows for a broad approximation as to the effect of renovations, accuracy can be further increased by adding a time-varying renovation indicator to our standard specification.²⁶

$$\Delta y_{ijt\tau} = \Delta D'_{ijt\tau} \delta_{jt} + \text{Renovation}'_{ijt} \gamma_t + \Delta \epsilon_{ijt\tau} \quad (6)$$

where the renovation dummy turns on for the period of the second sale. It thus absorbs the “extra” appreciation that occurred as a result of the renovation, irrespective of the duration of the “holding” period. To measure the effect of not accounting for renovations during index construction, we compare index estimates generated using equation (6) with index estimates generated using equation (4). The difference between these two sets of indices represents a cumulative measure of renovation bias.²⁷ Not accounting for renovations typically biases index estimates upwards. This is because, for a given home, the entire change in sales price between periods is interpreted as area-specific house price appreciation. If a renovation occurs between these two sales, a portion of this appreciation is likely due to a change in quality. When we net out the effect of a change in quality, our adjusted index estimates tend to decrease in magnitude.

²⁶To simultaneously price both renovated and non-renovated properties, a hybrid estimator could measure the shadow price or implicit market value of various types of home improvements. Clapp and Giaccotto (1998) suggest an estimator akin to this, where changes in hedonic characteristics are proxied for by the assessed value of a home at the time of each sale. A potential problem with this specification is there might not be sufficient variation in ΔX_{ij} to reliably estimate region-specific hedonic values. This issue can be solved by running region-specific models simultaneously and constraining the hedonic coefficients to remain constant across equations, $\beta_j = \beta_k$. Since we are interested in index bias and not implicit prices, we continue with the more appropriate repeat-sales approach.

²⁷The impact at a particular point is shown in the estimated effects with $\hat{\gamma}_t$. Those differences can accumulate over time and become more pronounced with subsequent periods of differences. That cumulative effect is shown by comparing the adjusted and unadjusted HPI levels since a base period.

Figure 3: Comparing adjusted and unadjusted indices for select geographies

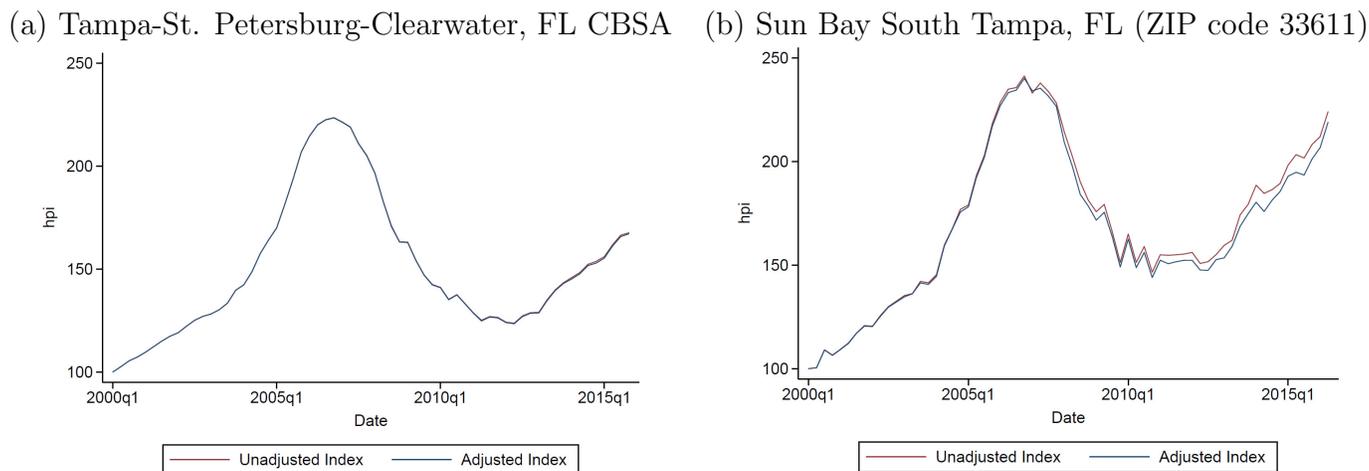


Figure 3 shows an example of adjusted versus unadjusted indices for the Tampa-St. Petersburg-Clearwater, FL CBSA. As illustrated, the adjustment has little effect on HPIs constructed at the CBSA level of aggregation in panel (a), but the bias is amplified by the more granular HPI in panel (b). For purposes of exposition, we focus our attention on a ZIP code in South Tampa (33611). With the ZIP code level HPI, the unadjusted index increases faster than the adjusted index as house prices begin to rise in the mid-2000s and the difference grows again when prices appreciated quickly between 2012 and 2014. To put this bias in perspective, the unadjusted South Tampa index is approximately 5 percent higher in Q1 and Q2 2014 which is equivalent to an \$11,500 overestimate for the average home in that ZIP code. This amount is not an insignificant sum—for local residents, \$11,500 represents approximately 20 percent of median household income.²⁸

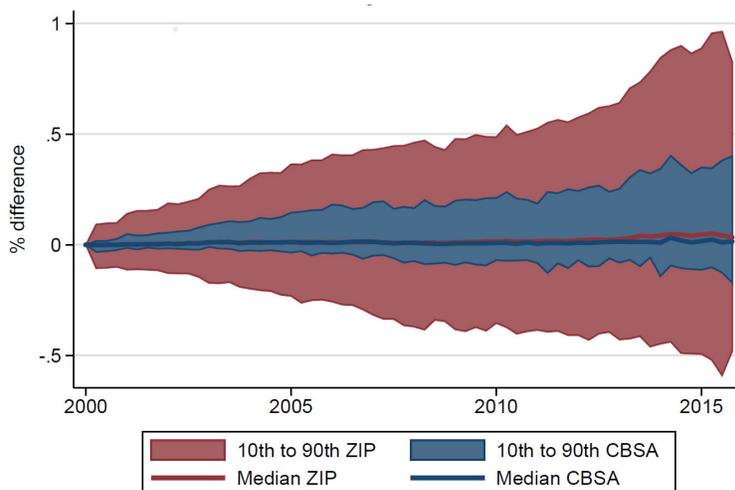
Figure 4 contrasts the distribution of CBSA level renovation bias against ZIP code level renovation bias across the entire country. As shown, bias is relatively muted in CBSAs but can vary considerably across ZIP code HPIs. In the next section, we investigate ZIP code level variation and explore its link to city size, demographics, and location within a city.

4.1. Renovations and distance to the central business district

Location might explain the greater extent of renovation bias found in a more local level HPI. Standard urban theory suggests land use tradeoffs increase as a property gets closer

²⁸Based upon census estimates.

Figure 4: Distributional spreads of renovation bias (average differences between HPIs)



to the center of the city, or the central business district (CBD).²⁹ To measure the urban effect, we limit our sample to ZIP codes within 25 miles of the CBD.³⁰ In order to fully examine the effects of renovations on index construction, we focus on two separate but related dependent variables: the average percentage difference between unadjusted and adjusted index levels (hereafter referred to as renovation bias) and the percentage of transactions associated with a renovation (hereafter referred to as renovation concentration).³¹ *Ex ante*, we posit a positive covariance between these two dependent variables. The distortionary effect of not accounting for property renovations is likely to increase as the value added of property improvements rises. In addition, as it becomes more profitable to pursue a property renovation, the concentration or percentage of transactions associated with a renovation is likely to increase, amplifying any omitted variable bias.

Table 1 examines how location affects both of the renovation variables. The upper panel measures distance in categorical bins to show non-linear effects while the lower panel treats

²⁹The CBD is calculated as the maximum value within the CBSA of the inverse of the standardized land area plus the standardized share of housing units in 20+ unit structures. Land area data are from the Census' TIGER line shapefiles, and structure type is from the 1990 Decennial Census, the earliest census for which ZIP code data are available. Distance to the CBD is calculated with the "as the crow flies" or straight-line distance.

³⁰Distance is computed based on the centroid of ZIP codes.

³¹Renovation bias does have a time element to it. Since we are comparing the differences in the index levels, the measure tracks the cumulative percent difference since a base year (2000 in this paper). This could also be expressed as the difference in the cumulative appreciations.

Table 1: Estimating the effect of renovations based on residential location

	Categorical bins for distance to CBD					
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean percent difference in HPIs			Mean percent of renovations		
	All	Large cities	Small cities	All	Large cities	Small cities
0-5 miles	0.197*** (0.035)	0.504*** (0.069)	0.028 (0.030)	0.169*** (0.034)	0.352*** (0.054)	0.129*** (0.044)
5-15 miles	0.042*** (0.025)	0.085*** (0.042)	0.001 (0.023)	0.071*** (0.024)	0.144*** (0.033)	0.022 (0.034)
Number of ZIP codes	4,220	2,147	2,073	4,315	2,167	2,148
Log-likelihood	-4,701	-2,850	-1,391	-4,768	-2,379	-2,286
R^2	0.007	0.024	0.001	0.006	0.022	0.004
	Quadratic expression for distance to CBD					
	(7)	(8)	(9)	(10)	(11)	(12)
	Mean percent difference in HPIs			Mean percent of renovations		
	All	Large cities	Small cities	All	Large cities	Small cities
Distance to CBD	-0.020*** (0.007)	-0.068*** (0.014)	-0.006 (0.005)	-0.017*** (0.006)	-0.048*** (0.011)	-0.017*** (0.008)
(Distance to CBD) ²	0.0005* (0.0003)	0.002*** (0.000)	0.000 (0.000)	0.000 (0.000)	0.001*** (0.000)	0.000 (0.000)
Number of ZIP codes	4,220	2,147	2,073	4,315	2,167	2,148
Log-likelihood	-4,704	-2,852	-1,391	-4,763	-2,371	-2,285
R^2	0.006	0.023	0.001	0.008	0.029	0.006

Note: Significance levels are denoted with * as 0.10, ** as 0.05, and *** as 0.01. **Bolded** text indicates significance at the 0.10 level. Standard errors are in parentheses. The percent differences compare standard and adjusted HPI levels and are calculated at the ZIP code level. The percent of renovations compare the number of renovations of single-family house transactions. Mean values are calculated over our entire sample from 2000 to 2015. Some ZIP code coverage begins after 2000. The omitted reference group in Eqns (1)-(6) is 15-25 miles.

distance continuously using a quadratic expression. Full sample results are shown in columns (1), (4), (7), and (10). As detailed in column (1), ZIP codes 0-5 miles from the CBD are associated with an average renovation bias of 0.197 percent (relative to the omitted category of being 15-25 miles away). This estimate is statistically significant at the 1 percent level and indicates that, on average, unadjusted indices are larger than adjusted indices. For a \$350,000 house, this coefficient estimate translates into a bias of approximately \$700.³² As we move farther from the city, estimated bias decreases. ZIP codes that are 5-15 miles from the CBD are associated with an average renovation bias of 0.042 percent (relative to the omitted category), which is statistically significant at the 10 percent level. When we split the sample by city size, the effect amplifies in large cities (column 2) and vanishes for small cities (column 3).

Regression results for our second dependent variable, the average frequency or concentration of renovations, are shown in columns (4) to (6). In the full sample, in column (4), we estimate that ZIP codes 0-5 miles from the CBD have 0.169 percent more renovations per transaction than ZIP codes 15-25 miles from the CBD. This effect appears to decrease with distance from the CBD, suggesting a negative concentration gradient. We estimate that ZIP codes 5-15 miles from the CBD have 0.071 percent more renovations per transaction than the excluded category. Both estimates are statistically significant at the 1 percent level. When splitting by sample size, the effect does not completely vanish in centers of small cities like we had seen with the renovation bias estimations.

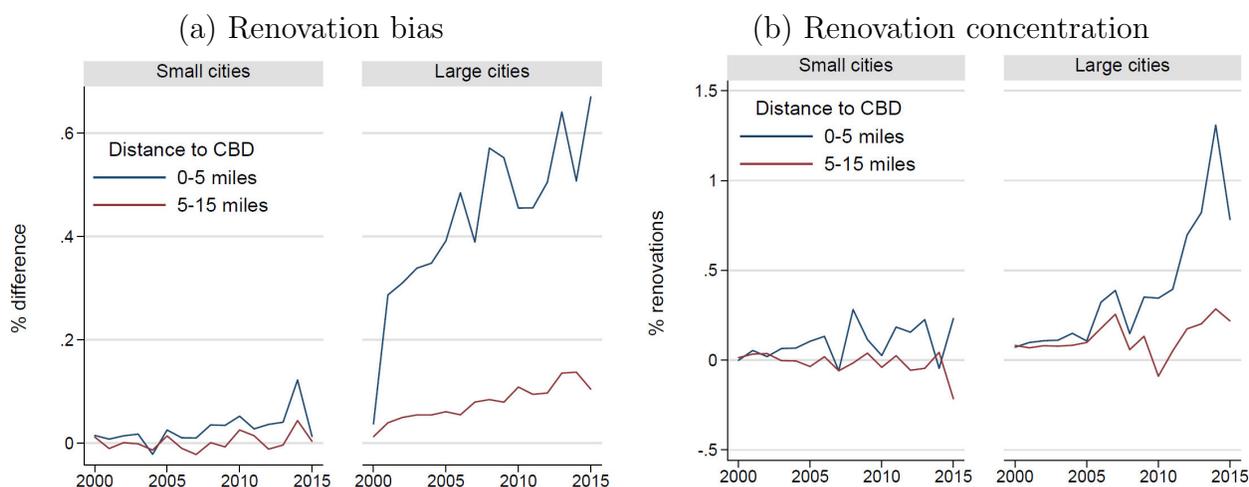
To understand how these effects vary across cities, we split our sample into two groups: ZIP codes in large cities and ZIP codes in small cities where large cities are defined as cities with more than 500,000 housing units. As detailed in Column (2), the relationship between distance from the CBD and renovation bias is significantly magnified in large cities. The coefficient estimate attached to the 0-5 mile indicator variable is equal to 0.504 percent and statistically significant at the 1 percent level. For the same \$350,000 home evaluated earlier, this effect would translate into a bias of approximately \$1,750 dollars. For small cities, the coefficient estimate attached to the 0-5 mile indicator is statistically insignificant. We observe a similar dynamic when we turn to the average concentration of renovations. In large cities, ZIP codes 0-5 miles from the CBD have an estimated 0.352 percent higher

³²In other words, if an unadjusted HPI were used to estimate the growth in the value of a \$350,000 house, the magnitude of the bias would amount to about \$700. This estimate is relative to the omitted category of being 15-25 miles away from the CBD.

concentration of renovations. Again, there is evidence of a negative concentration gradient. As we move to the second distance bin, 5-15 miles from the CBD, the estimated coefficient drops in size to 0.144 percent. Both estimates are statistically significant at the 1 percent level. These results suggest that both renovation bias and renovation concentration exhibit an agglomeration effect such that they are bigger in larger cities and the magnitude decreases as the distance grows from the CBD.

In order to examine how these relationships have evolved over time, we move to a series of yearly regressions estimated with data from 2000 to 2015. For each year, renovation bias and renovation concentration are regressed on our 0-5 mile and 5-15 mile distance bins and the results show the percent difference in the cumulative price growth since 2000.³³ Figure 5 illustrates the resulting coefficient estimates. In large cities, panel (a) demonstrates the relationship between renovation bias and distance from the CBD grows significantly in magnitude from 2000 to 2015. We see a similar dynamic in panel (b), which depicts the relationship between renovation concentration and distance from the CBD. Again, the estimated effect attached to the 0-5 mile indicator increases significantly between 2000 and 2015. These two panels suggest the current relationships may be understated between bias, concentration, and distance from the CBD when taking a simple 15-year average or performing estimations with a sample limited to data prior to the 2007 financial crisis.

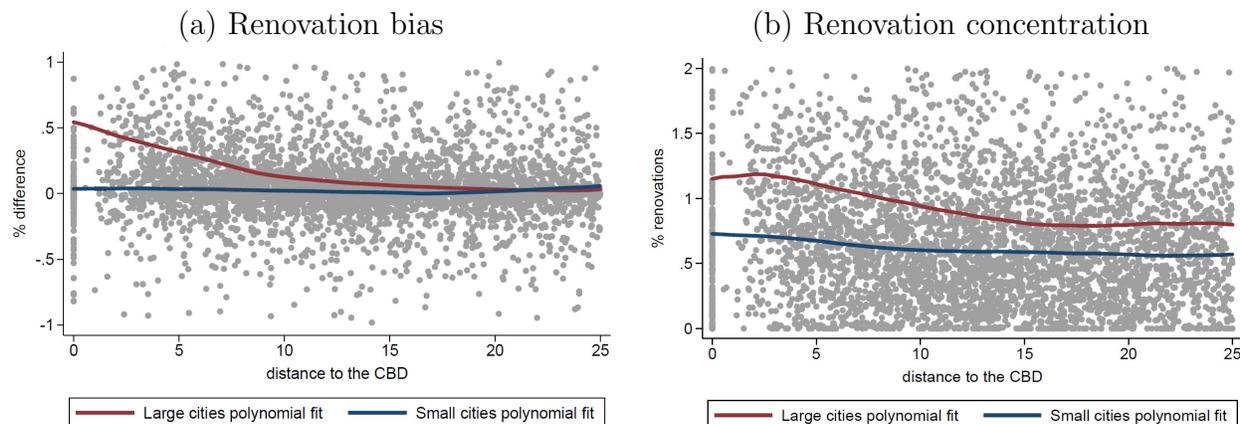
Figure 5: Annual estimated magnitudes for renovation measures



Note: Estimates are recovered from annual regressions run separately by city size.

³³The gap between the lines could be measuring a combination of compounding differences in cumulative appreciation rates as well as a growth in the renovation effect. Looking between years shows us the latter.

Figure 6: The relationship between renovations and residential locations within metropolitan areas



Note: Renovation bias and concentrations are averaged over a 15-year period (2000-2015) for each ZIP code to provide a long-run comparison. More specifically, the measures are calculated by each period and then averaged together. We perform similar analyses for different period lengths and the results are qualitatively similar.

Thus far, our analysis has been constrained to a series of relatively broadly defined distance bins. To add additional nuance, we move to a second specification, where both dependent variables are modeled as a quadratic function of distance from the CBD. Regression results for the full sample show evidence of a negatively sloped gradient. For every mile we move from the CBD, renovation bias decreases by 0.02 percent.³⁴ These model results suggest that a \$350,000 property within 1 mile of the CBD would be upwardly biased by approximately \$1,400 relative to a similar property 20 miles from the CBD. When we move to the large cities sample, this effect increases by almost a factor of four (or a bias of approximately \$4,750). We observe a similar relationship when we turn to model results for average renovation concentration. To assess the appropriateness of our quadratic specification, we run local polynomial regressions on our two dependent variables and distance to the CBD. Figure 6 depicts the results of these non-parametric estimations. In both instances, we see evidence of a negatively sloped gradient for large cities, but no discernable relationship between distance and renovations for small cities.

4.2. Renovations, demographics, and simultaneity

Based upon our analysis, renovation bias and renovation concentration decrease with distance from the CBD, but what drives this location-based relationship? We investigate the

³⁴The associated quadratic term is statistically significant but negligible in magnitude.

Table 2: Estimating the effect of renovations based on demographics within cities

	(1)	(2)	(3)	(4)
	Mean percent difference in HPIs		Mean percent of renovations	
	Large cities	Small cities	Large cities	Small cities
Distance to CBD	-0.053*** (0.015)	-0.002 (0.007)	-0.021** (0.009)	-0.017*** (0.006)
(Distance to CBD) ²	0.002*** (0.001)	0.000 (0.000)	0.001* (0.000)	0.0005* (0.0003)
City density	0.003 (0.003)	-0.009 (0.021)	-0.001 (0.003)	-0.015 (0.021)
Percent single-family detached	-0.168 (0.130)	-0.025 (0.104)	-0.259*** (0.074)	-0.173* (0.099)
Percent mobile home	-1.048*** (0.374)	-0.325** (0.143)	-1.172*** (0.204)	-0.561*** (0.136)
Percent in labor force	-1.378*** (0.375)	-0.302 (0.232)	-1.038*** (0.213)	-0.131 (0.220)
Median income	0.002 (0.003)	-0.001 (0.003)	-0.002 (0.001)	0.003 (0.002)
Unemployment rate	0.185 (0.932)	-0.003 (0.695)	-0.159 (0.520)	0.148 (0.653)
Number of ZIP codes	2,008	1,932	2,027	1,999
Number of CBSA fixed-effects	28	291	29	296
Log-likelihood	-2,689	-1,178	-1,584	-1,149
R^2	0.059	0.114	0.493	0.642

Note: Significance levels are denoted with * as 0.10, ** as 0.05, and *** as 0.01. **Bolded** text indicates significance at the 0.10 level. Standard errors are in parentheses. The percent differences compare standard and adjusted HPI levels and are calculated at the ZIP code level. The percent of renovations compare the number of renovations of single-family house transactions. Mean values are calculated over our entire sample from 2000 to 2015. Some ZIP code coverage begins after 2000.

correlation between our two dependent variables and several pre-period demographic variables to better understand the root cause of this relationship. As detailed in Table 2, the relationship between our two dependent variables and distance to the CBD remains qualitatively unchanged with a negative and convex distance gradient. We find a common and statistically significant negative effect on both our dependent variables for the percentage of mobile homes and the percentage of the population in the labor force.³⁵

Estimations are initially performed without CBSA fixed-effects. Based upon these results, we find a statistically significant effect for city density, the percentage of single-family detached housing and other demographic variables. When we introduce controls for the unobserved heterogeneity at the CBSA level, statistical significance only remains on the percentage of single-family detached for the renovation concentration equation. The controls for median income and unemployment are also no longer statistically significant for either renovation measure.³⁶

Because of the interrelationship between renovation bias and renovation concentration, it is difficult to attribute an estimated increase in bias to a single cause. For instance, a ZIP code near the center city may derive a relatively large benefit from renovations, which leads to a greater distortionary effect when omitting information on property improvements. At the same time, knowledge of this additional value added may direct a disproportionate amount of renovation activity to these areas which, in and of itself, will amplify bias.

To address simultaneity, we turn to a series of instrumental variable regressions informed by our demographic analysis in Table 2. Specifically, we account for the potential endogeneity of renovation concentrations by instrumenting for it using two plausibly exogenous pre-period variables: the percentage of a ZIP code's housing stock made up of single family residential properties and the density of housing stock. Both variables potentially impact renovation concentrations, but are unlikely to have a direct impact on the degree of renovation bias

³⁵This holds for all variables in large and small cities with the one exception being a negative but insignificant for the percentage of labor force in small cities.

³⁶We report results with CBSA fixed-effects but the results associated with distance to CBD are qualitatively similar when we do not control for unexplained heterogeneity at the CBSA level and instead use a variety of density, structural, labor force, and earnings controls from the 1990 census. The controls that are statistically significant are still included in Table 2, even if they are no longer statistically significant.

Table 3: 2SLS regression estimations for the mean cumulative percent difference (2nd stage) and percentage of renovations (1st stage)

	(1)		(2)		(3)		(4)	
	Categorical bins		Categorical bins		Large cities		Small cities	
	Large cities	Small cities	Large cities	Small cities	Large cities	Small cities	Large cities	Small cities
0-5 miles	0.346*** (0.110)	0.061 (0.150)						
5-15 miles	0.044 (0.053)	0.001 (0.038)						
Distance to CBD			-0.058** (0.025)				-0.010 (0.023)	
(Distance to CBD) ²			0.002** (0.001)				0.000 (0.001)	
Mean percent of renovations	0.491** (0.242)			-0.246 (0.887)			0.240 (0.449)	-0.122 (0.889)
Number of ZIP codes	2,008			1,932			2,008	1,932
Number of CBSA fixed-effects	26			289			27	289
Excluded instruments (Cragg-Donald Wald F -stat)	26.12			0.59			14.70	0.57
Underidentification tests (Anderson LM -stat)	51.67			1.39			14.81	1.34
Weak instrument (Anderson-Rubin Wald F -stat)	2.33			0.07			0.29	0.04
Overidentification tests (Sargan-Hansen χ^2 -stat)	0.38			0.03			0.00	0.03

Note: Significance levels are denoted with * as 0.10, ** as 0.05, and *** as 0.01. **Bolded** text indicates significance at the 0.10 level. Standard errors are in parentheses. The omitted reference group in Eqns (1)-(2) is 15-25 miles. Results only reflect the second stage estimation. The first stage utilizes instrumental variables for the percent of single-family detached homes and city density from the 1990 census.

itself.³⁷ As the percentage of single family units decreases, it becomes harder for families seeking more square footage or additional beds and baths to successfully move. Instead, they are expected to remain in their current property and renovate. Further, the density of an area’s housing stock should affect the types of renovations that can be easily pursued. In a dense area, space is more likely to be held at a premium, which may preclude increases in lot size or living area. Instead, homeowners should pursue interior modifications such as adding an additional bed or bath.

Using these two instrumental variables, we run 2-stage least squares (2SLS) on two measures of distance: a series of categorical bins and distance from the CBD specified as a quadratic. Table 3 provides our regression results.³⁸ Our results suggest that approximately half of the observed variation in ZIP code-level renovation bias is a function of differing concentration in renovation activity. The remainder of the bias may be a function of differences in the additional increase in property value associated with a renovation in an urbanized ZIP code. All of these results hold for large cities. Small cities do not exhibit a consistent relationship between renovation bias and location relative to the CBD. Further, there is a general lack of evidence of an endogenous relationship between renovation bias and the concentration of renovation activity. This phenomenon seems isolated to central areas of large cities.

4.3. Robustness checks of the renovation bias

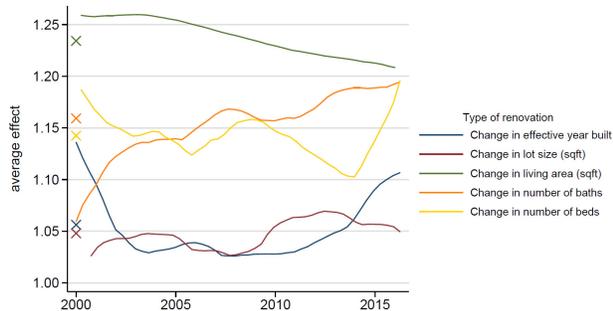
Although we explore several alternative specifications of the estimation results (like with functional form and measurement of renovation bias), we also offer two forms of robustness checks in Figure 7. Panel (a) examines the different types of renovations and their additive effect on the estimated house price growth rate. Panel (b) tests the sensitivity of our location variables on different conditional city size cut-offs.

³⁷When we introduce controls for unobserved heterogeneity at the CBSA level, neither of these variables have a statistically significant direct relationship with the renovation bias variable as shown in Table 2.

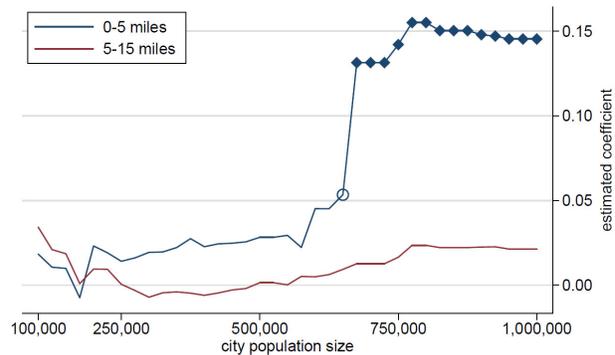
³⁸Among the regression diagnostics, the Cragg-Donald Wald F statistic tests the null hypothesis that any of the endogenous regressors are underidentified. In large cities, we are able to reject the null hypothesis of underidentification at the 1 percent level. Based upon Stock-Yogo weak ID test critical values, we reject the null hypothesis of more than 10 percent maximal bias relative to OLS for our large cities sample. The Anderson-Rubin Wald statistics tests the null hypothesis that (a) the coefficients of the endogenous regressors in the structural equation are both equal to zero, and (b) the overidentifying restrictions are valid. We fail to reject the null hypothesis at the 10 percent level for our quadratic specification “large cities” sample and fail to reject the null at the 5 percent level for our distance bins “large cities” sample. Finally, the Sargan-Hansen Chi-squared statistic tests the null hypothesis that (a) our instruments are valid, and (b) correctly excluded from the estimated equation. We fail to reject the null hypothesis at the 10 percent level for all four specifications.

Figure 7: Sensitivity testes for renovation and city size

(a) Capturing different types of renovations



(b) Altering the city size



Note: Estimates in panel (a) are recovered from local polynomial regressions and average effects are shown with an “X”. Estimates in panel (b) are obtained by running separate regressions with conditional filters that only include observations in cities below a particular population size. Significance levels are denoted with a solid diamond for 0.01, open square for 0.05, and open circle for 0.10.

In panel (a), we investigate the relative value added of individual renovation types using changes in five different housing attributes in five major CBSAs.³⁹ Recall, in our earlier estimates, we identify a renovation when there is a change in any of the main property attributes (i.e., effective year built, lot size, living area, number of bedrooms, and number of bathrooms). The average additive effect associated with each renovation type is detailed with the colored “X” marks in panel (a) of Figure 7. A change in living area square footage is associated with an estimate of 1.23, which indicates that, on average, that particular renovation increases the appreciation by approximately 23 percent. Based upon our estimates, adding beds or baths increases the growth rate by approximately 15 percent. The average gain associated with an increase in effective year built is approximately 6 percent, although this may understate the full value of a property renovation.⁴⁰ Finally, a change in lot size increases appreciation by approximately 5 percent.⁴¹

³⁹To increase the precision of our point estimates, we focus our analysis on the five CBSAs with the highest concentration of renovations in our MLS data.

⁴⁰Significant collinearity across certain renovation indicators can lead to higher standard errors and more uncertainty regarding true point estimates. For instance, a change in the effective year built is often coincident with a major increase in living area square footage and/or the number of beds or baths.

⁴¹Again, this point estimate is potentially inaccurate because of a relative paucity of data points on associated transactions. In the MLS data, identified changes in lot size are relatively rare, which makes it difficult to accurately estimate the price effect of this specific type of upgrade. We are not suggesting that a change in lot size has no positive premium. In fact, recent evidence by Brooks and Lutz (2016) suggests combined properties have premiums of 15 to 40 percent in the Los Angeles market.

Panel (a) also depicts how each of these renovation specific effects has changed dynamically over time (as graphed with solid lines).⁴² As shown, the effects attached to a change in beds and baths have oscillated between 1.1 and 1.2, although in recent years, both effects are again trending upwards. The additive effect attached to a change in lot size has remained relatively constant at approximately 1.05, while the effect attached to a change in effective year built has varied from a high of approximately 1.15 in 2000 to a low of approximately 1.05 in 2007 and 2008. The additive effect attached to a change in living area square footage has slowly decreased in magnitude from a high of approximately 1.27 in 2000 to a low of approximately 1.2 in 2015. While trends may convey a signal, because of limited sample size, some of the variation is likely noise and not useful.

In panel (b), we examine how the significance of the location results vary when we alter the sample based upon city size. We run separate regressions in an incremental fashion that regress the renovation bias variable on the categorical distance bins conditional upon city size. We begin with ZIP codes belonging to cities with populations less than or equal to 100,000 units per the 1990 census. We continue to repeat this exercise by increasing the population cut-off in increments of 25,000 and the estimated coefficients are shown in panel (b). Earlier in the paper, we distinguish between small and large cities using a population cut-off of 500,000. This robustness check supports our earlier conclusion that statistically significant effects are confined to larger cities, and the results would have been even stronger if we had used a higher population cut-off.

5. Conclusion

This paper provides the first wide-scale analysis of property renovation bias across the United States. We find strong but localized evidence of positive quality drift and upwardly biased index estimates in large metropolitan areas. These distortionary effects appear to be confined to more granular housing market indices; the bias effectively vanishes when constructing more aggregate metro-area price measures. This suggests that repeat-sales indices, which

⁴²Due to the way we measure renovation bias, the size of the parameters are, in part, a function of the timing of the prior renovations. Because of depreciation, across-time differences in the relative volumes of renovations will play a role in determining the size of the measured renovation bias in any given year. For example, assume there is increase in bathroom renovations in 2001, but none thereafter. In this hypothetical, if one assumes that 2001 bathroom improvements depreciate in value (say, because they become out of date), then the measured impact of bathroom renovations will necessarily decay over time. A transaction pair with a first and second second in 2000 and 2002 respectively will show a relatively robust renovation bias, whereas the 2000 to 2015 pair will show a much-diluted bias. The differences in relative renovation activity over time will impact both the relative results shown in this section, as well as our earlier results.

are often used in public discourse to describe the state of housing markets, are not unduly influenced by positive quality drift when they represent a more aggregate area (e.g., CBSA, state, region, or nation).⁴³ The localized indices, though, can be affected in centralized areas of large cities, and that could be a concern when index data are being used for model valuations or policy analysis. For instance, mark-to-market loan-to-value ratios or loan-level appraisals could be affected by not accounting for the renovation effect. In market areas where renovations exist, current indices may overstate the extent of house price appreciation in systematic and non-trivial ways.

Because of the localized nature of renovation bias, we focus the majority of our analysis on within-city variation. We find that the persistence and magnitude of this bias varies in a predictable manner. As distance from the center city increases, the distortionary effect of not accounting for a property renovation tends to decline. This negative gradient is partially a function of more renovations occurring near city centers and differences in the value added through property improvements throughout the city, especially in larger cities.

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⁴³Note, these conclusions mainly reflect results for HPIs constructed after 2000 in areas where we have MLS coverage. Densely populated areas may not always be more likely to have greater statistical bias when renovation activity is unaccounted.

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