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# The hedonic model and the housing cycle☆

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ABSTRACT

The hedonic house price model is a significant workhorse when it comes to estimating the value of local public goods such as school quality and crime, and locational amenities such as job accessibility. Given Rosen's (1974) result that hedonic coefficients can be interpreted as the marginal willingness to pay (MWTP) for the good, the hedonic model can be used to calculate the benefits of policies based on improving school performance or public safety. One of the key assumptions for this interpretation of the hedonic coefficients as MWTP is that the market is in equilibrium. The recent turbulence in the U.S. housing market has led many researchers to question the interpretation of the hedonic coefficients. Putting periods of significant market instability aside, housing markets go through cycles just as the economy does. One might expect, then, that hedonic coefficients will also vary over the housing cycle.

A house price hedonic for the Greater Boston Area is estimated using transactions data over a long time period, 1987–2012, that covers multiple cycles with peaks in 1988 and 2005. The impacts of standardized test scores, crime rates, and job accessibility on house prices are estimated on an annual basis. Surprisingly, there is evidence that these estimates exhibit a counter-cyclical variation with the largest impacts occurring during the recent downturn. This can be explained by changes in the composition of buyers over the housing cycle.



## 1. Introduction

The hedonic house price model has become a significant workhorse when it comes to estimating the coefficients for local amenities such as public goods (school quality and crime) and locational amenities such as job accessibility (collectively referred to as local public goods). When it comes to evaluating policies based on improving school performance or improving public safety, researchers have often relied on hedonic estimates to calculate the benefits of such policies.

An important motivation for using the hedonic model to evaluate policies that involve local public goods is Rosen's (1974) result that hedonic coefficients can be interpreted as the marginal willingness to pay (MWTP) for the good. This means that coefficient estimates for local public goods can be used to measure the benefits of these goods. One of the key assumptions underlying Rosen's result is that the market is in equilibrium. The recent turbulence in the U.S. housing market has led many researchers to question the interpretation of the hedonic coefficients.[1]

Putting periods of significant market instability aside, housing markets go through cycles just as the economy does. One might expect, then, that hedonic coefficients will also vary over the housing cycle, potentially being largest in magnitude at the peak when demand and hence willingness to pay (WTP) is high and smallest in magnitude at the trough when demand and hence WTP is low.

To get an idea of how much variation in hedonic coefficients one might expect over the housing cycle, a house price hedonic for the Greater Boston Area is estimated in this study using transactions data over a long time period: 1987–2012. This time period covers multiple cycles with peaks in 1988 and 2005 and troughs in 1991 and 2012.

The local public goods that are included in the model are state-administered standardized test scores, crime rates, and an index of job accessibility. The hedonic coefficients for these local amenities are estimated on an annual basis to investigate how they vary over the housing cycle. Contrary to the hypothesis that the hedonic coefficients will be

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[1] For example, this was the topic at a panel at the Association of Environmental and Resource Economists Summer Conference in 2012 entitled "Valuation in a Bubble: Hedonic Modeling Pre- and Post-Housing Market Collapse." There was a general belief that hedonic coefficients should be referred to as "implicit prices" rather than as MWTP, particularly when the market experiences significant instability (Boyle et al, 2012).

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largest in magnitude at the peak of the housing cycle, the results show that the coefficient estimates are largest during the recent downturn.

As a comparison, similar results for three structural characteristics are provided: lot size, living space, and bathrooms. The price impacts for these variables exhibit consistent trends and do not appear to be affected by the housing cycle.

The annual price impacts for the local public goods are significantly related to housing transaction volume. This is an indication that the number of buyers and hence the type of buyer in the market can change over the housing cycle. As such, the marginal buyer will vary and hence the hedonic coefficient estimates, which reflect MWTP, will also change over time. This can explain the counter-intuitive result that the hedonic coefficient estimates for the local public goods are actually largest during the recent recession.

One recommendation from this study is that results for the hedonic coefficients using data over a complete housing cycle are likely to give estimates of the MWTP for local public goods that are most representative of the population as a whole. This is particularly true for policymakers who are using the hedonic results to measure the benefits associated with policies related to local public goods. The evidence shows that using data from the recent housing market downturn can be problematic and the results from the 1987–2005 housing cycle are preferred (versus the 1996–2012 period). Furthermore, applying data over the housing cycle allows for the effective use of fixed effects that mitigate omitted variables bias due to unobserved neighborhood quality.

Section 2 surveys the relevant literature. Section 3 provides details about the data. Section 4 develops the standard hedonic model and discusses the use of fixed effects to control for unobserved neighborhood quality. The results are given in Section 5 and the results are discussed and conclusions are drawn in Section 6.

## 2. Literature survey

There is little prior evidence on the impact of the housing cycle on hedonic coefficients. One prominent study is conducted by Smith and Huang (1995) who carry out a meta-analysis of 86 hedonic estimates of implicit prices for reductions in total suspended particulates (TSP). They regress these estimates on a number of city, hedonic model, and data characteristics. They include as city indicators, TSP level, real income, and the housing vacancy rate. Real income has a positive effect and the vacancy rate has a negative impact. These results are consistent with the conjecture that MWTP is larger in magnitude when the market is booming and smaller in a down market (vacancies are higher when the market is down). Finally the TSP level has a negative coefficient estimate. While one would expect that the MWTP to reduce TSP would be higher at higher levels of TSP (lower levels of air quality), the negative coefficient could indicate that higher TSP levels are proxying for other city-wide factors that affect the demand for air quality (other than real income). It could also signify residential sorting whereby households with greater preferences for clean air sort into cities with lower air pollution levels.[2]

Another factor that can affect coefficient estimates is the composition of buyers/sellers in the market. Krainer (2001) develops a model to explain hot and cold markets where the former are characterized by high prices and volume and the latter are characterized by low prices and volume. Krainer shows that in a hot market, sellers are able to (and want to) sell houses quickly, thus prices and volume are high. Novy-Marx (2009) notes that buyer entry is induced when markets are hot because the value of entry increases. This raises prices and volume even more, exacerbating the values of these fundamentals. Of course, the opposite happens in cold markets. To the extent that hedonic coefficients depend on house prices, one would expect to see significant variation across hot and cold markets

Chernobai and Chernobai (2013) categorize buyers into long- and short-term buyers, and housing units into low- and high-quality units. They note that short-term buyers are more likely to buy low-quality units than long-term buyers because the costs of attaching themselves to the low quality unit are lower. This generates a form of selection bias in that lower quality units are more likely to transact since they are more likely to be sold and purchased by short-term buyers.

Combining the results from the Krainer (2001) and Novy-Marx (2009) papers on the one hand and those from Chernobai and Chernobai (2013) on the other hand allows for a conjecture about selection bias in hot and cold markets. First, one can think of high-/low-quality units as those with higher/lower levels of local public goods. Then it follows that since long-term buyers prefer high-quality units, they have a higher MWTP for local public goods. Second, it follows that hot markets, with higher levels of transactions, have a relatively higher proportion of short-term buyers as sellers are able to sell houses quickly which appeals to these buyers. Then cold markets, with lower levels of transactions, have a relatively higher proportion of long-term buyers as houses are sold at a slower pace. This reflects sellers' reluctance to lower prices in down markets (e.g. sellers are subject to “loss aversion” as they are not (psychologically) willing to sell their houses for less than they paid for them (Genesove and Mayer, 2001)). Then the hedonic coefficients can actually be larger (in magnitude) in cold markets (than in hot markets) since the marginal buyer is more reflective of long-term buyers with higher MWTP. The result is a countercyclical relationship between market conditions and the estimated MWTP for local public goods.

## 3. Data

The transaction data include single-family home sales in the Greater Boston Area for 1987–2012. The data are from the Warren Group for 1987–1994 and CoreLogic for 1995–2012 and cover towns in Bristol, Essex, Middlesex, Norfolk, Plymouth, and Suffolk Counties.[3]

Sales that were not standard market transactions such as foreclosures, bankruptcies, land court sales, and intra-family sales are excluded. Furthermore, for each year, the bottom and top 1% of sales prices are excluded to guard against non-arms-length sales and transcription errors. The data include typical house characteristics: age, living space, lot size, the number of bathrooms, bedrooms, and total rooms. The sample is limited to units with at least one bedroom and bathroom, 3 total rooms and 500 square feet of living space and no more than 10 bedrooms and 10 bathrooms, 25 total rooms, 8000 square feet of living space, and 10 acres.

The second transaction is excluded for properties that sold twice within 6 months (similar to Case/Shiller) and for properties with two sales in the same calendar year with the same transaction price (likely duplicate records). Properties for which consecutive transactions occurred in the same year or in consecutive years and where the transaction price changed (in absolute value) by more than 100% are excluded. Similarly, properties where consecutive transactions were in year $t$ and $t + j$ and where the transaction price changed (in absolute value) by more than j00% were excluded for $j = 2,\ldots,12$.

32 towns with less than 100 total observations are dropped and 36 census tracts with less than 10 observations are excluded leaving a total of 145 towns, 630 census tracts, and 369,859 observations in the data set.

The test score data used for this analysis come from the Massachusetts Department of Education (MADOE). Starting in 1988, the Massachusetts Educational Assessment Program (MEAP) was administered every other year until 1996. Mathematics and reading exams were given

[2] See Chay and Greenstone (2005) for a test of residential sorting by preference for air quality.

[3] The city of Boston is not included since it is not in the data that was provided by the Warren Group.

statewide to 4th, 8th, and 12th grade students in 1988, 1990 and 1992 and to 4th, 8th, and 10th grade students in 1994 and 1996.

One of the components of the Massachusetts Education Reform Act of 1993 (MERA) was the institution of a new statewide test in 1998; the Massachusetts Comprehensive Assessment System (MCAS). The MCAS is given every year in grades 3–8 and 10 (in math and English Language Arts (ELA)). MEAP and MCAS scores are standardized to make them comparable across years. School quality is measured as the sum of district-level 4th and 8th grade math and reading/ELA exams. The average of the two surrounding years for 1989, 1991, 1993, 1995, and 1997 is used since no state-wide standardized exams were given in these years. Since scores are not comparable across years, the school quality variable is then standardized on an annual basis. It is referred to as TESTS.

About half the towns in Massachusetts have their own schools for all K-12 grades (referred to as K-12 towns). The other half sends their students to regional schools (which receive students from multiple towns) for at least some of these grades (typically 9–12) or to other towns for some or all grades. Hence one might think that school quality is valued differently in the K-12 towns compared to the other towns. To account for this, separate school quality variables for the K-12 towns and the other towns are included. These variables are referred to as TESTS_K12 and TESTS_OTHER. They are standardized such that the mean for the whole sample is 0 and the standard deviation is 1. This makes hedonic estimates for TESTS_K12 and TESTS_OTHER across years comparable.

Crime data comes from the FBI's Uniform Crime Reporting Statistics. Both property and violent crimes are provided. Property crimes include burglary, larceny-theft, and motor vehicle theft. Violent crimes include murder and non-negligent manslaughter, forcible rape, robbery, and aggravated assault. These two measures of crime are highly correlated (correlation = 0.68). Hence, the first principle component of the two crime variables is used. This variable is then standardized over the whole sample since units are not meaningful. This variable is referred to as CRIME. One of the drawbacks of this crime measure is that 36 of the 145 towns did not report crime statistics and many of the remaining towns do not report data for all years. This is dealt with in the model by including a variable that indicates which towns do not report crime information (and by setting the crime measure to zero for the missing values).

The employment accessibility index from Fisher et al (2009) is used to measure job accessibility. This is a gravity index that is a function of commuting time to each job location. The data on employment comes from the Massachusetts Department of Labor's ES-202 database. It provides annual average employment for each of Massachusetts's 351 cities and towns. The data on commuting times is obtained from the Boston Metropolitan Planning Organization. It divides the region into 986 Traffic Analysis Zones. The accessibility index is only measured in 2005 so there is likely to be measurement error in other years though this should be minimal since job accessibility changes very slowly over time.[4] This will imply some attenuation bias in the coefficient estimates and hence differences in coefficients could reflect differences in the measurement error bias. This variable is standardized since units are not meaningful. It is denoted ACCESSIBILITY. It is available for 136 of the 145 towns in this data set. As is the case for CRIME, a variable that indicates the towns for which employment accessibility is not available in included in the regression model (and the missing values are set to zero).

Summary statistics are given in Table 1. The correlation between TESTS and CRIME is −0.73, between TESTS and ACCESSIBILITY is 0.18 and between CRIME and ACCESSIBILITY is −0.04.

[4] Another potential source of measurement error is the Big Dig construction project that rerouted the main route through the city. It began in 1991 and ended in 2007. While there was projected to be considerable time savings for some commutes, demand induced increases in traffic likely mitigated much of this savings (https://en.wikipedia.org/wiki/Big_Dig#Impact_on_traffic cites Murphy (2008)).

**Table 1**
Summary statistics for hedonic variables.

| Variable | Mean | Std. dev. | Minimum | Maximum |
|---|---|---|---|---|
| Sale price | 313.00 | 212.68 | 17.50 | 1925.00 |
| Standardized test | 0.00 | 1.00 | −3.42 | 2.49 |
| Property crime rate (per 100,000 population) | 2160.31 | 1323.11 | 70.20 | 10,885.00 |
| Violent crime rate (per 100,000 population) | 284.33 | 323.01 | 4.60 | 2371.70 |
| Job accessibility index | 0.00 | 1.00 | −1.53 | 4.22 |
| House age | 43.96 | 35.12 | 0.00 | 200.00 |
| Number of bedrooms | 3.31 | 0.82 | 1.00 | 10.00 |
| Number of bathrooms | 1.71 | 0.74 | 1.00 | 10.00 |
| Number of half baths | 0.61 | 0.54 | 0.00 | 8.00 |
| Total number of rooms | 7.01 | 1.61 | 3.00 | 23.00 |
| Living area (square feet) | 1964.95 | 854.14 | 500.00 | 7999.00 |
| Lot size (acres) | 0.62 | 0.78 | 0.10 | 10.00 |
| Median income − 10th percentile | 8.46 | 2.77 | 3.75 | 18.75 |
| Median income − 25th percentile | 15.36 | 4.06 | 6.25 | 32.50 |
| Median income − 50th percentile | 23.54 | 4.85 | 13.75 | 45.00 |
| Median income − 75th percentile | 32.44 | 6.64 | 21.25 | 62.50 |
| Median income − 90th percentile | 43.70 | 10.25 | 28.75 | 85.00 |
| Percent nonwhite | 2.24 | 1.98 | 0.00 | 16.44 |
| Percent renters | 27.40 | 13.53 | 5.70 | 74.54 |
| Percent no high school degree | 21.03 | 9.68 | 4.14 | 48.58 |
| Percent BA | 23.79 | 12.71 | 7.59 | 61.72 |
| House price − 10th percentile | 33.03 | 8.89 | 20.24 | 76.21 |
| House price − 25th percentile | 43.10 | 11.95 | 27.32 | 96.36 |
| House price − 50th percentile | 55.11 | 15.90 | 33.30 | 131.55 |
| House price − 75th percentile | 69.38 | 21.20 | 41.28 | 171.56 |
| House price − 90th percentile | 85.88 | 28.68 | 50.88 | 217.19 |
| Number of observations | 639,859 | | | |

## 4. The hedonic house price model

The following standard hedonic model is specified for house price $P_{injt}$ for house $i$, in neighborhood $n$, in jurisdiction $j$, in period (year) $t$

$$\ln(\mathrm{P_{injt}}) = \beta_{0t} + \mathrm{X_{injt}}\beta_{1t} + \mathrm{N_{njt}}\beta_{2t} + \mathrm{e_{ijnt}} \quad (1)$$

where $X_{injt}$ is a vector of house characteristics, $N_{njt}$ is a vector of neighborhood amenities that includes local public goods such as school quality and safety that are produced by the town and ones that are essentially exogenous to the town, i.e. job accessibility. The intercept is allowed to vary to allow for the market price to change over time. Eq. (1) is specified as a log-linear model but this still allows for the explanatory variables to include logs, higher order polynomials, interactions, and other nonlinear functions of the observables.

The standard interpretation of the coefficients in the hedonic model (Eq. (1)) as the MWTP for an additional unit of a house characteristic or neighborhood amenity is based on Rosen (1974). The basis of his model is the pricing of a heterogeneous good with multiple characteristics. The outcome of optimizing decisions by consumers and producers, under a set of assumptions, will result in an equilibrium hedonic price function that represents a set of tangencies between consumer bid functions and producer offer functions. Rosen shows that under these equilibrium conditions the coefficients corresponding to the characteristics in the hedonic model can be interpreted as consumers' MWTP for these variables.

Assuming that the hedonic function (1) is known, one can take the derivative with respect to a local public good $N_{njt}$ and claim that this derivative represents the MTWP for $N_{njt}$. Of course it is not known and must be estimated from data. One issue is that the hedonic function is based on both the bid and offer functions and the points on the hedonic are due to factors that affect both consumers and producers. This introduces a classic simultaneous equations problem in the estimation of this function. But for many policies related to local public goods, it is only necessary to consider the demand side of the market (Palmquist, 1991). Taking the supply side as fixed is not a problem because housing supply mainly consists of existing stock. Hence the Rosen model of consumer and producer maximization can be replaced by one in which consumers face a given distribution of housing units (and their characteristics) (Epple, 1987).

The hedonic equilibrium can change each period as factors affecting consumers' housing decisions vary over time. Furthermore, the composition of buyers can change over time. This means that the hedonic coefficients, as interpreted as the WTP for the marginal buyer can change from period to period. Hence, in its most general specification, the coefficients in the hedonic model (Eq. 1) are allowed to vary every period. This amounts to estimating a separate equation for each period typically taken as a year.

If one believes that single period estimates will contain too much sampling error to be useful then it is a reasonable strategy to group periods together. Another reason to group the data is because, for reasons just mentioned, estimates for single years can be affected by the housing cycle. When viewed as a panel, it is standard to assume that the coefficients are constant over time (other than the intercept). This should produce a weighted average of the annual coefficient estimates (versus taking the straight average of the coefficient estimates obtained from the annual regressions).

A key consideration when estimating Eq. (1) is controlling for unobserved neighborhood quality. In part, this reflects residential sorting such that neighbors are choosing to live in the same location based on similar tastes for observable and unobservable neighborhood characteristics. Nguyen-Hoang and Yinger (2011) point to three approaches to alleviating this omitted variables bias (OVB).[5] The first solution is to include as many observable neighborhood characteristics as possible as explanatory variables. School quality, crime, and job accessibility are included; three important characteristics that new homeowners care about when deciding where to live. Neighborhood measures of demographic characteristics such as median household income and the percent of residents with a BA degree are often included. The argument is that either individuals care about the income or education of their neighbors (though these characteristics can be difficult to observe) or because they can proxy for unobservable neighborhood characteristics. What is problematic about these variables is that they tend to be endogenous due to residential sorting. Numerous census tract-level variables from the 1980 Decennial census are included in Eq. (1). This should minimize their endogeneity bias since they are measured prior to the start of the period covered in the sample data.

The second approach is to use instrumental variables. The problem is that finding valid instruments is not an easy task. Following Downes and Zabel (2002), the percent of the tax base that is residential (PCT_TAX_RES), per pupil assessed value (PER_PUPIL_ASSESED_VALUE), the percent renting (PCT_RENT), and the percent of the population that is school aged (PCT_LE17) are considered as instruments. They assume that these variables are valid instruments because they represent demand-side factors that influence the level of public goods but do not directly affect house prices.

The third approach is the addition of some form of fixed effects (FE) to Eq. (1). Potential candidates include border fixed effects and neighborhood effects in the form of census tract or town fixed effects.[6] The border fixed effects model as first employed by Black (1999) was used to obtain more accurate estimates of school quality. In this case, the borders are school attendance zones. This allows for the comparison of houses on either side of the attendance zone. The argument is that all that varies within the border fixed effects area is school quality due to the location in different school attendance zones. Hence, conditioning on the border fixed effects should control for unobserved town-level factors that would otherwise be correlated with school quality.[7] Black's estimates are significantly reduced when the border fixed effects are added to the model. The argument for this outcome is that school quality is correlated with unobserved town amenities that result in a positive bias.

But it is also important to recognize that the source of variation and hence the interpretation of the coefficient for school quality has changed. Without border fixed effects, identification comes from cross-district variation in school quality. Hence the coefficient is a measure of the value residents place on being in one school district versus another. That is, the value of the right to send your kids to grades K-12 in that district versus another district. With border fixed effects, the identification comes from differences in elementary school quality (since attendance zones at their most disaggregated level are for elementary schools). Hence, the coefficient is a measure of the value residents place on being in one elementary school versus another. That is, the value of the right to send your kids to grades K-4 (or 5 or 6) in that attendance zone versus another attendance zone in the same district. Given that the attendance zones on either side of the border are assigned to the same middle and high schools, this estimate does not include the value that residents place on attending the middle and high school. Hence, it would not be surprising if the unbiased estimate of school quality from the border fixed effects model is smaller than the one from the model without the border fixed effects.

Another consideration is that variation in school quality within a border fixed effect area is likely to be small compared to cross-district variation in school quality. Thus, even if the unbiased estimates with and without border fixed effects are the same, calculating the impact of larger changes in school quality that better represent cross-district variation using the border fixed effects model requires out-of-sample extrapolation which usually assumes a linear impact. This issue also arises with census tract fixed effects since there is no guarantee that residents will attend different elementary schools. Hence census tract fixed effects most certainly require data from multiple periods to obtain time variation in order to identify coefficients for local public goods such as school quality.

For local public goods that do not vary within the jurisdiction in an exogenous way (such as by attendance zones), identification must come from cross-time variation or cross-jurisdiction variation. If there is not much variation across time (which is likely to be the case unless a policy is instituted that significantly changes the level of the public good), then the estimates can be imprecise, and, as mentioned about

[5] Another means for controlling for OVB is to include a town-level house price index. This controls for changes in town-level prices over time. Without it, the changes in the local public goods may pick up general changes in the jurisdiction-level quality and lead to biased estimates of the impact of these local public goods on house prices. But changes in the price index are due, in part, to changes in the local public goods and hence including the price index will attenuate the impacts of the local public goods on house prices. One solution is possible if the price index predates the sample period. One can then estimate a pre-sample town-level price trend and extend this trend into the sample period. This would be the predicted price trend based on pre-sample information and hence would not be affected by in-sample changes in the levels of the local public goods. The Federal Reserve Bank of Boston does generate a town-level price index for Massachusetts. It is a repeat sales index and is constructed in a similar manner as the Case/Shiller house price index. It is an annual index starting in 1987. Unfortunately, it cannot be used for this analysis since it does not pre-date the sample period.

[6] The repeat sales model is a popular approach for generating house price indices (i.e. Case/Shiller). That is, the focus is on the time varying intercept, $\beta_{0t}$. Unit fixed effects control for unobserved unit characteristics that bias the results if they are correlated with the explanatory variables. Typically, the repeat sales model does not include *X* or *N* as explanatory variables so these are assumed to be (relatively) constant over time (any units that show evidence of change such as pulling building permits, are typically excluded). In fact, one advantage of repeat-sales is that one does not need information on structural or neighborhood characteristics. The use of repeat sales also mitigates selection bias that can arise when different types of units are sold in different periods (though this can be controlled for, to a certain extent, by including house characteristics in the hedonic model). Though, it can result in selection bias since houses that sell more than once are not necessarily representative of the population of housing units. Using the repeat sales model (versus the hedonic model) will have a significant impact on the estimates of the house price index if unobserved unit characteristics vary systematically with price within the jurisdiction.

[7] Bayer et al. (2007) point out that households can sort on either side of the attendance boundary which will bias the border fixed effects estimates. They recommend including neighborhood-level demographic variables to control for differences in neighborhood quality on either side of the border.

border fixed effects, estimates of large changes (as one might expect from changes due to policies) require out-of-sample extrapolation. The other strategy is to rely on across jurisdiction variation. To minimize bias, one would like to include as many jurisdiction-level observables in the model as possible. One strategy is to include variables from a decennial census year before the first year of the sample.

## 5. Results

The dependent variable in the hedonic model is the natural log of house price. House characteristics include the natural logs of lot size and living space and their squares, indicator variables for age less than or equal to 10 years, 10 to 30 years and 30 to 50 years (greater than 50 years is the excluded category), indicators for 2, 3, 4, and 5 or more bedrooms (1 bedroom is the excluded category), 2 or 3 or more bathrooms (1 bathroom is the excluded category), the number of half baths, and indicators for 5–9 rooms, 10–14 rooms, and 15 or more rooms (3 or 4 rooms are the excluded category). The average real single family tax bill is also included in the model rather than the property tax (mill rate) which is clearly endogenously determined.

The local public goods are town test scores, crime, and the job accessibility index. Since these variables are standardized, the coefficients are interpreted as the percent change in house prices for a one standard deviation increase in the local public good, on average (given the transformation $100 * (\exp(b) - 1)$).[8]

Eq. (1) will first be estimated separately for each year of the data which gives 25 point estimates for each local public good. It is not possible to use fixed effects with the annual regressions since the local public goods do not vary within the town. To control for unobserved neighborhood quality, a number of variables from the 1980 Decennial Census are included; the 10th, 25th, 50th, 75th, and 90th percentiles of household income and house prices and percent renters, nonwhite, and residents ages 25 or older with no high school degree and with a BA degree. Thus the coefficients for the local public goods are identified by cross-town variation conditional on the 1980 decennial census variables.

Eq. (1) will then be estimated using the whole sample and also for relevant sub-periods; first by OLS and then with town and census tract fixed effects. This will allow one to ascertain the impact that not controlling for unobserved neighborhood quality can have on the hedonic estimates for the local public goods.

### *5.1. Annual regressions*

Initially, a separate hedonic for each year that includes test scores, crime, and the job accessibility index is estimated (a regression for 1987 is not possible since the test score data starts in 1988). The summary statistics for the transformed coefficient estimates ($100 * (\exp(b) - 1)$) for the local public goods from the annual regressions are given in Table 2. The mean for TESTS_K12 is 1.44%; a one standard deviation increase in TESTS_K12 is correlated with a 1.44% increase in house prices (14 of the 25 estimates are significant at the 1% level). This change is on the low end of the 1%–4% range reported in the recent literature (Nguyen-Hoang and Yinger, 2011). For the mean coefficient estimate for TESTS_K12, the difference in house prices at the town with the 95th percentile test score compared to the town with the 5th percentile test score is 3.7%.

The coefficient of variation, the ratio of the standard deviation and the mean of a variable, is a useful measure of the variation in the price impact estimates. The value of the coefficient of variation for TESTS_K12 is 1.07. Generally, it is only meaningful when a variable takes on only non-negative or non-positive values (in which case the coefficient of variation is the ratio of the standard deviation and the absolute value of the mean of a variable). In the case of TESTS_K12, three of the values are negative, though none is significant at the 1% level. The coefficient of variation is 0.89 if these three values are set to zero.

The mean estimated impact for TESTS_OTHER is actually negative. Twelve of the estimates are negative and only ten of the 25 are significant at the 1% level. It does not appear that buyers of houses in towns that do not have K12 schools significantly value school quality, at least in terms of the district-level scores on state administered 4th and 8th grade reading and math tests.

The mean estimated price impact for CRIME is −3.11% (21 of the 24 estimates are significant at the 1% level). For the mean coefficient estimate for CRIME, the difference in house prices at the town with the 95th percentile crime value compared to the town with the 5th percentile crime value is −7.23%. Pope and Pope (2012) found that property values increased 7–19% between 1990 and 2000 at the top decile of zip codes in terms of crime reduction. So their estimates are comparable to the ones obtained in this study.

The coefficient of variation for CRIME is 0.71. In the case of CRIME, two of the values are positive, though both are small and insignificant. The coefficient of variation is still 0.71 if these two values are set to zero. So the variation in crime impacts is smaller than that for TESTS_K12.

The mean of the estimated impacts for ACCESSIBILITY is 11.34; a one standard deviation increase is associated with an 11.34% increase in house prices (all estimates are significant at the 1% level). The coefficient of variation for ACCESSIBILITY is 0.15. So the impact of job accessibility is particularly large and there is relatively little variation in the estimates over time.

To get some idea about the magnitude of the variability in the annual estimates for these local public goods, similar results for three structural characteristics are provided; lot size, LOTSIZE, living space, LIVESIZE, and bathrooms. The log and its square for lot size and living space are included. Since these are continuous variables, the elasticity evaluated as the sample means is calculated. The specification for bathrooms includes binary variables for the presence of two bathrooms, BATHS2, and for three or more bathrooms, BATHS3 (1 bathroom is the left out group). The semi-elasticity for these two variables is provided. The summary statistics are included in Table 2. The mean elasticity for LOTSIZE is quite small (0.04%; though all estimates are significant at the 1% level) and the coefficient of variation is 0.40. The mean elasticity for LIVESIZE is significantly larger; 0.34% and the coefficient of variation is 0.16. The average semi-elasticities for BATHS2 and BATHS3 are 6.51 and 13.73 with coefficients of variation of 0.29 and 0.32, respectively.

The variables with the smallest coefficient of variation; ACCESSIBILITY and LIVESIZE also have large price impacts. Whereas the estimated impacts for TESTS_K12 show the largest variation. Generally, there is much less variation in the annual estimates for the structural characteristics than there is for the local public goods.

Next, time-series graphs of the estimated annual impacts for TESTS_K12, CRIME, and ACCESSIBILITY and their 95% confidence interval estimates are given in Fig. 1a, 2a, and 3a. One thing that is apparent is that the estimates are significantly larger in magnitude at the end of the sample when the market has been fairly volatile.

Fig. 4 displays the annual elasticities and semi-elasticities for the four housing characteristics; living space, lot size, and the presence of two or three or more bathrooms. There is a consistent increase in the elasticity for living space and for the semi-elasticities for the two bathroom variables and a consistent decrease in the elasticity for lot size over time. This could reflect an increase in the demand for larger houses and a decline in demand for lot size over time.

Next, the relationship between these estimates and measures of the housing and business cycles is investigated. Measures of the former include the growth rate in real house prices in the Boston MSA based on the house price index from the FHFA (put in real terms using the

[8] Yinger (2015) shows that to be consistent with sorting, the specification of local public goods in the hedonic is nonlinear.Squares of test scores, crime, and the accessibility index were included. In only half the cases for the annual regressions are these squared terms significant at the 1% level.

**Table 2**
Summary statistics for annual regressions.

| Price effects — percent change | Mean | Median | Std. dev. | CV* | Min | Max | Pct sig at 1% |
|---|---|---|---|---|---|---|---|
| TEST_K12 (1 sd increase) | 1.44 | 1.37 | 1.54 | 1.07 | −0.92 | 5.52 | 56 |
| TEST_OTHER (1 sd increase) | −0.71 | 0.41 | 2.01 | −2.86 | −4.50 | 2.26 | 40 |
| CRIME (1 sd increase) | −3.11 | −2.97 | 2.20 | −0.71 | −7.45 | 1.21 | 88 |
| ACCESSIBILITY (1 sd increase) | 11.34 | 11.26 | 1.72 | 0.15 | 8.55 | 15.95 | 100 |
| LIVESIZE (1% increase) | 0.34 | 0.33 | 0.05 | 0.16 | 0.27 | 0.44 | 100 |
| LOTSIZE (1% increase) | 0.04 | 0.04 | 0.02 | 0.40 | 0.01 | 0.07 | 100 |
| BATHS2 (semi-elasticity) | 6.51 | 6.27 | 1.88 | 0.29 | 3.53 | 12.14 | 100 |
| BATHS3 (semi-elasticity) | 13.73 | 12.01 | 4.39 | 0.32 | 8.34 | 25.24 | 100 |

* — coefficient of variation. Each regression includes the following house characteristics: the natural logs of lot size and living space and their squares, indicator variables for age less than or equal to 10 years, 10 to 30 years and 30 to 50 years, indicators for 2, 3, 4, and 5o r more bedrooms, 2 or 3 or more bathrooms, the number of half baths, and indicators for 5–9 rooms, 10–14 rooms, and 15 or more rooms and the average real single family tax bill and the following variables from the 1980 Decennial Census: the 10th, 25th, 50th, 75th, and 90th percentiles of household income and house prices and percent renters, nonwhite, and residents ages 25 or older with no high school degree and with a BA degree.

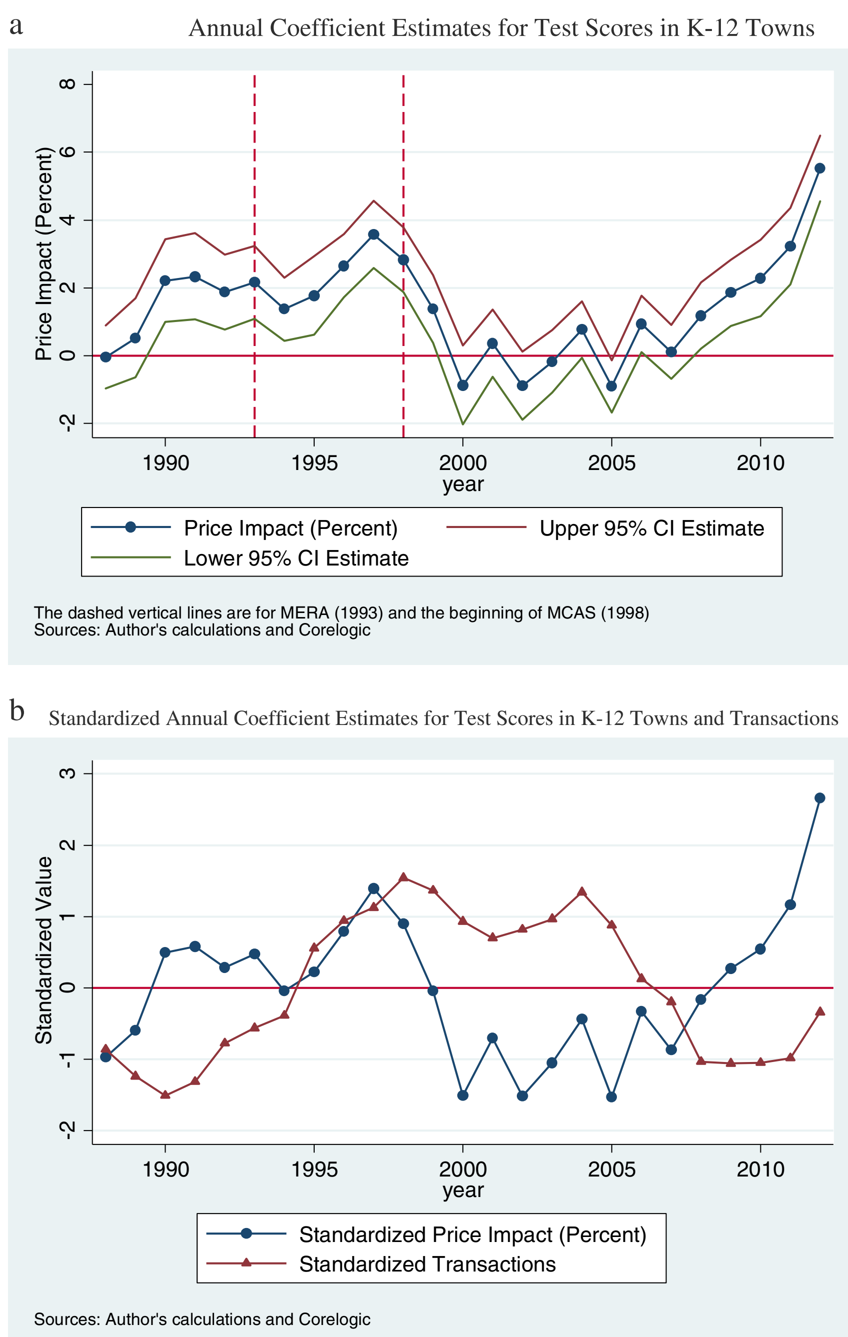



**Fig. 1.** a. Annual coefficient estimates for test scores in K-12 towns. b. Standardized annual coefficient estimates for test scores in K-12 towns and transactions.

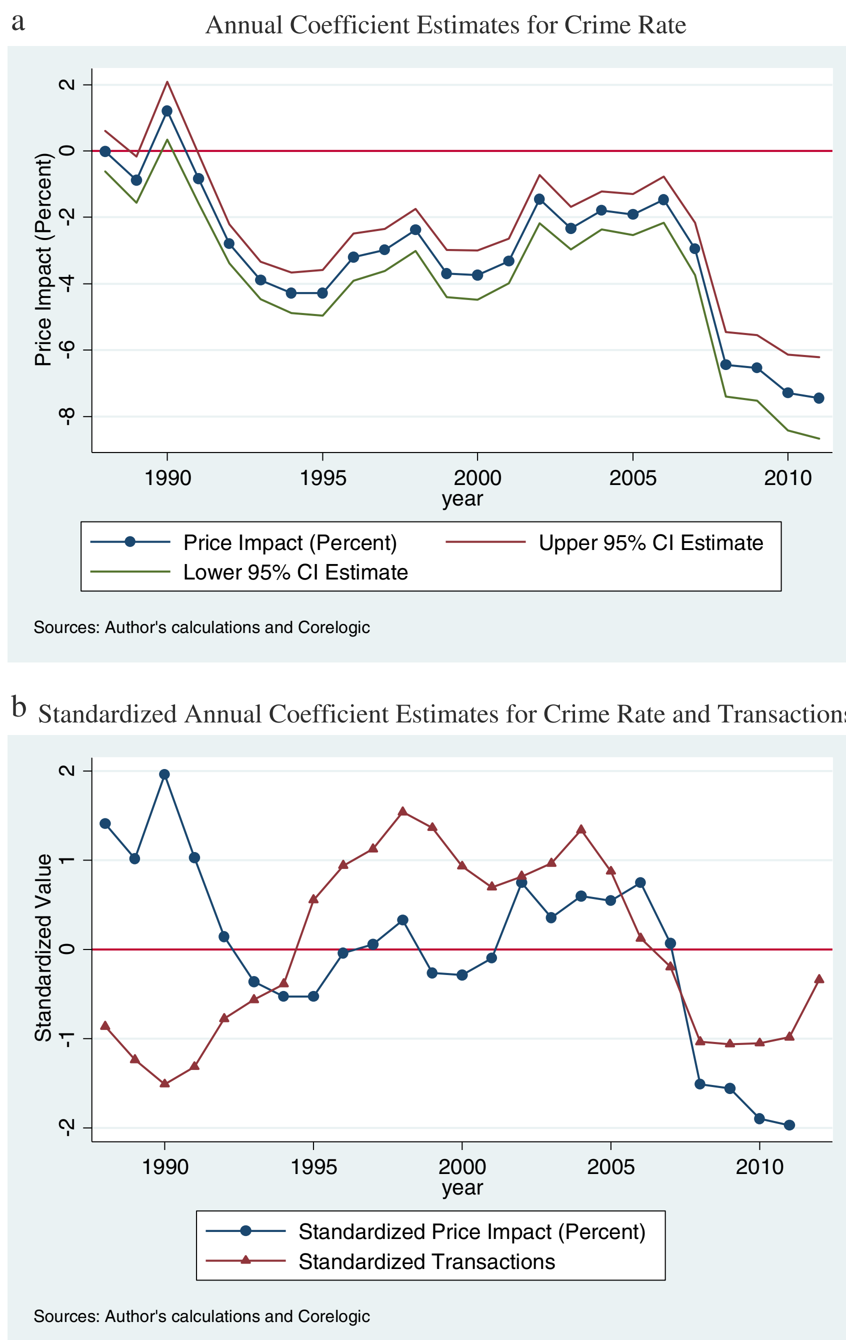



**Fig. 2.** a. Annual coefficient estimates for crime rate. b. Standardized annual coefficient estimates for crime rate and transactions.

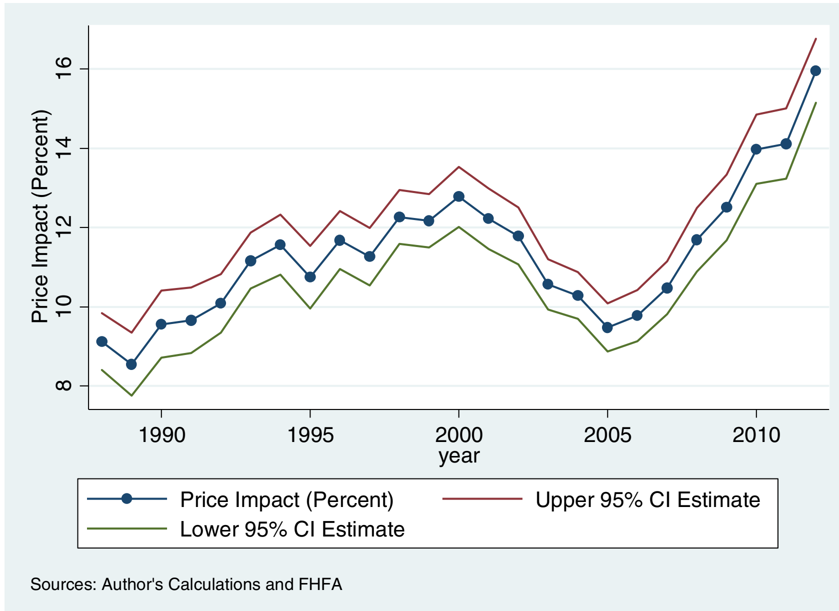
Boston area CPI), HP_GR, the number of transactions based on the sample in this analysis, the owner occupied vacancy rate from the Census Bureau's Vacancy Rate Survey, the growth rate in building permits from the U.S. Census Bureau, and the 30-year fixed mortgage rate.[9] Measures of the business cycle include the growth rate in real Massachusetts GDP, the Boston MSA unemployment rate; both from the Bureau of Economic Analysis and the growth rate in real per capita personal income. Summary statistics for these variables are included in Table 3.

To provide some background, Fig. 5 plots HP_GR. The market was coming off a high in the mid to late 1980s and was in decline in the beginning of the period covered in the data. Prices did not show positive real growth until the mid-1990s at which point there was sustained growth until the recent downturn. The vacancy rate is also graphed in Fig. 5. It shows the expected counter-cyclical relationship with real house price growth. Note that the vacancy rate is quite low for the Boston MSA relative to other MSAs and there has been relatively little variation until the recent downturn when vacancy rates have reached their highest levels over the 25-year period of the data. This indicates that the market has recently been in a period of excess supply not seen in more than 25 years and is a sign that the market has exhibited a significant amount of turbulence.

It appears that transactions lead house prices by two years. Fig. 6 includes the second lag of the growth rate in transactions and HP_GR. One can see that the two series line up quite nicely except for the 1997–2005 period when they are the mirror image of each other — the house price

[9] Data on the foreclosure rate are only available starting in 1999 so it is not included in this analysis.

a Annual Coefficient Estimates for Accessibility Index




b Standardized Annual Coefficient Estimates for Accessibility Index and Transactions

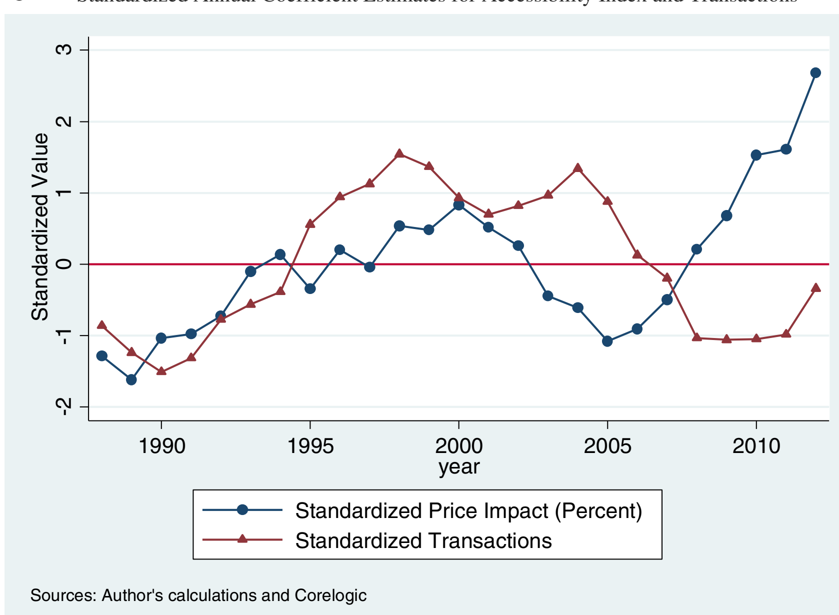



**Fig. 3.** a. Annual coefficient estimates for accessibility index. b. Standardized annual coefficient estimates for accessibility index and transactions.

growth rate is relatively high whereas the transaction growth rate is relatively low. It is apparent that demand is outpacing supply during this time period and this is driving up prices. Fig. 6 also includes the real state GDP growth rate. The GDP and transaction growth rates series track quite well. This is an indication that the business and housing cycles are in sync in Massachusetts.

Pairwise correlations are used to ascertain relationships between the estimated price impacts for each of the local public goods and the housing and business cycle measures. All of these latter measures are not included in one regression since there are only 25 observations and given that some of these variables are reasonably highly correlated, it will be very difficult to get precise coefficient estimates for each regressor. A variable that is significantly negatively correlated with the estimated impacts for TESTS_K12, (the negative of) CRIME, and ACCESSIBILITY is housing transactions. The standardized versions of transaction volume and the estimated price impacts of the local public goods are included in Fig. 1b, 2b, and 3b. The standardization of the variables makes it easier to see the relationship between each pair of variables. The inverse relationship between transactions volume and the impacts for TESTS_K12 is evident from Fig. 1b; transactions are low and the estimated price impacts are larger than average in the negative real growth periods of 1988 to 1995 and 2006 to 2012, and transactions are high and the impact estimates tend to be below average in the positive real growth period of 1996 to 2005.

The relationship between transactions volume and the annual impact of crime on house prices is positive since crime has a negative impact on house prices. This is evident in the positive real house price growth period of 1996–2005 and the ensuing negative real growth

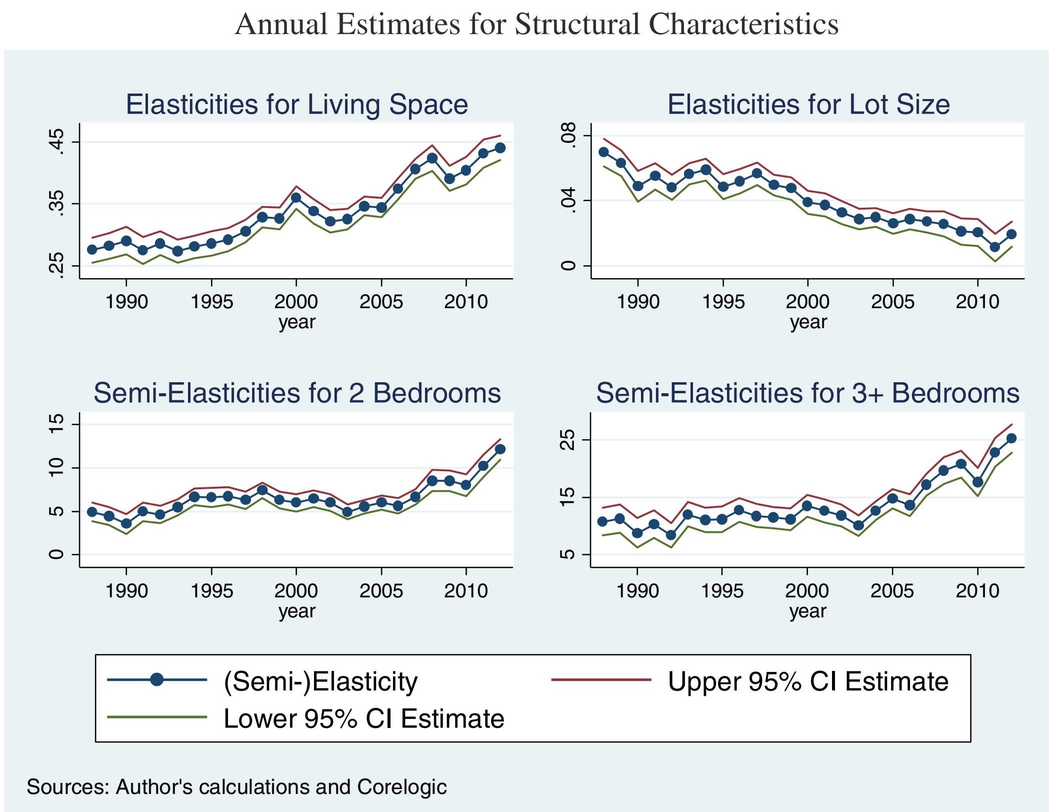



**Fig. 4.** Annual estimates for structural characteristics.

period of 2006–2012. The relationship is negative over the 1987–1995 period though the first three impacts are not significantly different from zero. A similar, though opposite relationship exists between transactions and the annual impact of job accessibility on house prices; the inverse relationship holds starting around 1996.

The inverse relationship between transaction volume and the annual impacts for TEST_K12 and ACCESSIBILITY and the positive relationship between transaction volume and the annual impacts for CRIME is indicative of a non-random selection of buyers in the housing market. As conjectured in Section 2, down markets with low levels of transactions have relatively more buyers with high MWTP for local public goods whereas the opposite is true in up-market periods with high levels of transactions. Hence hedonic coefficients which reflect the MWTP of the marginal buyer can actually be larger (in magnitude) in down markets. This counter-cyclical effect is consistent with this empirical evidence of an inverse relationship between transaction volume and the magnitudes of the hedonic coefficients estimated here.

From the fact that the annual price impacts for the structural characteristics are consistently trending upwards or downwards over the 1987–2012 period, it is apparent that they are not related to the housing cycle. The only measure that is consistently correlated with the house characteristic impacts is the mortgage rate and this is because it was trending downwards over the 1987–2012 period. So it is not clear that the mortgage rate is driving these trends or that this is just a spurious correlation.

**Table 3**
Summary statistics for housing ad business cycle variables.

| Variable | Mean | Median | Std. dev. | Min | Max |
|---|---|---|---|---|---|
| *Housing cycle variables* | | | | | |
| Real house price growth rate | 0.69 | −0.48 | 6.53 | −11.10 | 11.21 |
| Transaction growth rate | 1.14 | 2.47 | 12.42 | −23.86 | 28.39 |
| Owner occupied vacancy rate | 1.03 | 1.00 | 0.45 | 0.30 | 2.00 |
| Building permits growth rate | 2.05 | 0.27 | 17.67 | 2.05 | 0.27 |
| Mortgage rate | 7.1 | 6.97 | 1.79 | 7.1 | 6.97 |
| *Business cycle variables* | | | | | |
| Real state GDP growth rate | 1.76 | 1.34 | 2.60 | −3.13 | 6.95 |
| Unemployment rate | 5.29 | 5.00 | 1.65 | 2.60 | 8.30 |
| Real per capita personal income | 52.16 | 53.43 | 5.97 | 52.16 | 53.43 |

*5.2. Period regressions*

Regressions using the full sample and some selected sub-samples are run assuming that the coefficients (other than the intercept) are constant over time. The price impacts for the local public goods and the selected house characteristics using the full sample are listed in the top panel of Table 4. OLS results as well as those using town and census-tract fixed effects (FE) are also included. These latter estimators control for unobserved time-invariant neighborhood effects at the town and census-tract level, respectively.

The price impact estimate for TESTS_K12 is larger than the mean of the annual estimates and is reasonably similar across estimators: 2.28 for OLS, 2.86 for town FE, and 2.86 for census tract FE. The estimates for TESTS_OTHER are not significant at the 1% level for any of the three estimators. The OLS estimate for the impact of CRIME, −2.47, is smaller in magnitude than the average of the annual estimates and significantly declines in magnitude when fixed effects are included; −1.24 for town FE and −0.54 for census tract FE. It appears that CRIME is positively correlated with other unobservable town- and tract-level disamenities. For example, the towns with the highest crime rates: Lawrence, Lynn, Lowell, and Brockton, have some of the lowest house prices. The impact of ACCESSIBILITY can only be estimated using OLS since it is measured at one point in time. The OLS estimate, 11.09, is very similar to the mean of the annual estimates, 11.34.

The price impacts of the house characteristics are quite similar to the corresponding means of the annual estimates and are mostly unaffected by the use of the three different estimators though the estimates of the impacts for the two bathroom variables decline somewhat when the

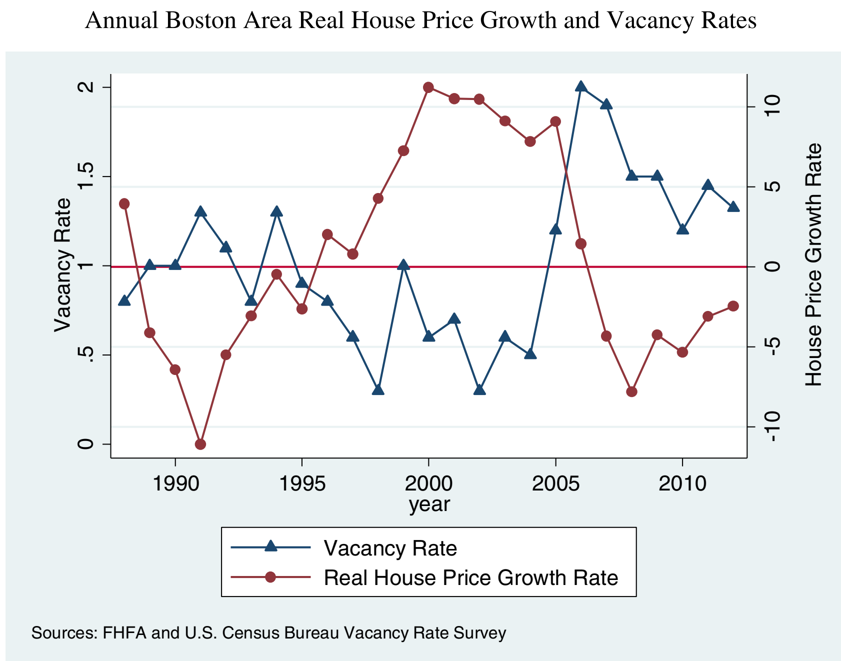



**Fig. 5.** Annual Boston area real house price growth and vacancy rates.

fixed effects estimators are used. This indicates that houses with more bathrooms tend to be located in neighborhoods with relatively high levels of unobserved amenities.

The Boston Area housing cycle over the 1987–2012 sample can be divided into three sub-periods: 1987–1995; a period of negative real growth, 1996–2005; a period of positive real growth; and 2006–2012; another period of negative real growth (see Fig. 5).[10] Separate regressions for each of these three periods are run to see how the coefficient estimates vary when estimated using data from distinct parts of the housing cycle. The results are included in Table 4. For TESTS_K12, the smallest OLS estimate is during the expansionary period, 1996–2005 and largest during the recent downturn. This is consistent with the annual estimates and is contrary to the conjecture that the magnitudes of the impacts would be pro-cyclical in line with higher demand driven by higher household income. As is the case when using the full sample, the town and census tract fixed effect estimates are larger than the OLS estimates during the 1987–1995 and 1996–2005 periods. But the fixed effects estimates are negative when using the data from the recent housing crisis. This is evidence that one should be skeptical of hedonic results based on recent data.

The OLS estimates of the price impact of CRIME are similar during the first two periods (approximately −3) and about half this size in magnitude when using the data from the recent downturn. As is the case for TESTS_K12, the fixed effects estimates are opposite in sign when using the 2006–2012 data. Again, the results from the recent downturn produce estimates that are questionable. The OLS estimates for the impact of ACCESSIBILITY on house prices are relatively constant across the three periods, though it is largest during the 2006–2012 period.

Results using data from a full housing cycle, 1987–2005, are given in the 5th panel in Table 4. These seem to provide the most reasonable estimates of the impact of the local public goods on house prices. A similar pattern emerges whereby the fixed effects estimates for TESTS_K12 are about twice as large as the OLS estimates; the former indicate that a one standard deviation increase in test scores lead to an increase in house prices of around 3.5%. This fits in the 1%–4% range of values found in the literature (Nguyen-Hoang and Yinger, 2011).

In the case of CRIME, the opposite pattern is evident; the fixed effect estimates are smaller in magnitude than the OLS estimates; a one standard deviation in test scores leads to an increase in house prices of around 2%. This evidence indicates that both test scores and crime are positively correlated with local disamenities. It highlights the usefulness of fixed effects for controlling for unobserved neighborhood quality. The result for test scores is somewhat surprising as conventional wisdom would appear to hold that better schools are located in towns with higher amenity levels (though conventional wisdom is often shown to be incorrect!).

Note that TESTS_K12 and CRIME are measured at the town level. Yet there is within-town variation in these variables. This means that the town-level and the census tract-level fixed effects estimators could produce different results if the within-town variation in these local public goods is correlated with within-town unobserved neighborhood quality. The estimates for TESTS_K12 are similar when using the town-level and the census tract-level fixed effects. This likely reflects the fact that there is relatively little within town heterogeneity in school quality (from multiple elementary and middle schools). So any within-town heterogeneity in unobserved neighborhood quality does not appear to result in additional OVB. But the estimates for CRIME are different when using the town-level and the census tract-level fixed effects. This likely reflects the fact that there is significant with-in town heterogeneity in crime that is highest in areas with low levels of unobserved neighborhood quality. Census tract fixed effects will control for this OVB but town-level fixed effects will not. This shows that fixed effects with different levels of aggregation can mitigate different amounts of OVB depending on the relationship between the heterogeneity in the local public good and unobserved neighborhood quality.

The hedonic model is also estimated using data from a second full housing cycle, 1996–2012; 1996 was the start of a period of real positive growth until the recent recession that began a period of negative real growth. The results are given in the bottom panel of Table 4. The OLS estimates for TESTS_K12, CRIME, LOTSIZE, and LIVESIZE are similar to those using the data from the 1987–2005 housing cycle. The estimates

[10] Note that growth is actually positive in 2006 so one might fix the period of the recent great recession as 2007–2012. The period 2006–20012 is used since, despite the positive growth in 2006, it was a real drop from the peak growth in 2005 and was an initial sign that the market was turning down. There is little change in the results if the 2007–2012 versus the 2006–2012 period is used.

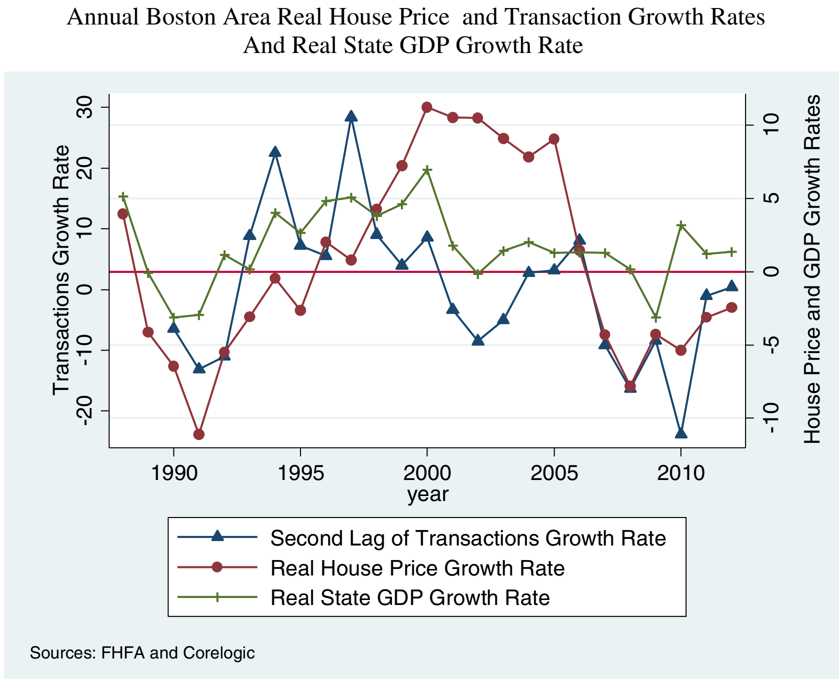



**Fig. 6.** Annual Boston area real house price and transaction growth rates and real state GDP growth rate.

for ACCESSIBILITY, BATHS2, and BATHS3 are larger than the 1987–2005 estimates and the estimate for TESTS_OTHER is negative and significant. Furthermore, the fixed effects estimates for TESTS_K12 are small in magnitude and not significant and the census tract fixed effect estimate for CRIME is actually positive and significant. These estimates seem less believable as compared to those for the 1987–2005 housing cycle and what is found in the previous literature. Hence, the recommendation is to use data for a full "normal" housing cycle (that doesn't include the 2006–2012 period).

As previously discussed, the local public goods are endogenously determined with house prices which can lead to biased estimates. As discussed in the Data section, PCT_TAX_RES, PER_PUPIL_ASSESED_VALUE, PCT_RENT, and PCT_LE17 are considered as instruments for TESTS[11], CRIME, and the single family tax bill (ACCESSIBILITY is assumed to be exogenously determined). There is evidence of the endogeneity of these variables using a Hausman test. Hence the model is re-estimated for the full sample using instrumental variables. The resulting coefficient estimates for TESTS and CRIME are significant but implausibly large and the estimate is actually negative for TESTS. This is symptomatic of the fact that these instruments fail the over-identification test for instrument exogeneity. This leads to instrumenting only for the single family tax bill using PCT_TAX_RES and PER_PUPIL_ASSESED_VALUE as these seem to be the most plausibly exogenous. But this has little effect on the coefficient estimates for the local public goods and generally does not produce the expected negative coefficient estimate for the single family tax bill when applied to the sub-samples and the annual regressions. Hence, the other two methods are relied on to control for omitted variables bias due to unobserved neighborhood quality (including as many neighborhood indicators as possible and using fixed effects).

The question remains as to what is a reasonable hedonic estimate of local public goods that can be used to evaluate WTP for these goods? It is probably wise to exclude the estimates from the recent years when the market was in turmoil. With enough data, one can estimate the hedonic using a full housing cycle to get an average estimate over the cycle. For Boston this period is 1987–2005. Using the fixed effects estimator is preferable to taking the average of the annual estimates as it enables one to control for unobserved neighborhood quality that appears to be significantly correlated with the local public goods school quality (test scores) and the crime rate.

[11] TESTS_K12 and TESTS_OTHER are not included separately so as to simplify the IV estimator.

## 6. Discussion and conclusion

The recent downturn in the housing market has led researchers to question the use of hedonic coefficients as reliable estimates of MWTP. Furthermore, it is likely that estimates of MWTP will vary as the conditions of the housing market change. In this study, data over multiple housing cycles in the greater Boston area are used to investigate the variation in coefficient estimates for local public goods — school quality, crime, and job accessibility. The coefficient estimates do vary over the housing cycle and are largest in magnitude during the recent downturn. This result is contrary to the hypothesis that hedonic coefficients are largest in magnitude at the peak of the cycle (due, in part, to the income effect). Instead, this can be explained by the composition of buyers in the market and hence who the marginal buyer is.

The variation in the annual coefficient estimates for the local public goods is most strongly related to sales volume. The relationship appears to be counter-cyclical and is indicative that there is a non-random selection of buyers in the market over the housing cycle. Based on results in Novy-Marx (2009) on the characteristics of hot and cold markets and those in Chernobai and Chernobai (2013) on ex ante selection bias, it is conjectured that cold markets have relatively more buyers with higher MWTP for local public goods. Hence the hedonic coefficients, which reflect MWTP, are higher in cold markets and vice versa for hot markets (see Section 2 for details).

The annual estimates of the price impacts of the local public goods are clearly not causal for at least two reasons. First, test scores and crime are jointly determined with house prices. That is, local public goods affect house prices but house prices also determine the level of local public goods through the composition of residents in the jurisdiction (and their demands for local public goods). Second, while a large number of explanatory variables are included in the annual regression models, it is likely that unobserved neighborhood characteristics are

**Table 4**
Price impact estimates from pooled period regressions.

| Periods | Price impacts | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | TESTS_K12 (1) | TESTS_OTHER (2) | CRIME (3) | ACCESSIBILITY (4) | LIVESIZE (5) | LOTSIZE (6) | BATHS2 (7) | BATHS3 (8) |
| *1987 to 2012* | | | | | | | | |
| OLS | 2.28 | −0.36 | −2.47 | 11.09 | 0.33 | 0.04 | 6.42 | 13.42 |
| | 0.00 | 0.02 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | 2.86 | −0.09 | −1.24 | 0.00 | 0.33 | 0.06 | 5.47 | 11.42 |
| | 0.00 | 0.91 | 0.04 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | 2.86 | 0.21 | −0.54 | 0.00 | 0.32 | 0.06 | 4.99 | 9.82 |
| | 0.00 | 0.77 | 0.01 | | 0.00 | 0.00 | 0.00 | 0.00 |
| *1987 to 1995* | | | | | | | | |
| OLS | 1.72 | 0.46 | −2.84 | 10.01 | 0.28 | 0.06 | 5.58 | 10.55 |
| | 0.00 | 0.09 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | 2.83 | −1.04 | −2.58 | 0.00 | 0.29 | 0.07 | 4.75 | 9.05 |
| | 0.04 | 0.24 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | 2.79 | −1.07 | −2.50 | 0.00 | 0.27 | 0.07 | 4.38 | 7.64 |
| | 0.00 | 0.14 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |
| *1996 to 2005* | | | | | | | | |
| OLS | 1.14 | 0.01 | −3.08 | 11.31 | 0.33 | 0.04 | 6.21 | 12.30 |
| | 0.00 | 0.98 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | 1.41 | −0.09 | −2.36 | 0.00 | 0.33 | 0.06 | 5.14 | 10.06 |
| | 0.24 | 0.93 | 0.06 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | 1.52 | −0.17 | −1.08 | 0.00 | 0.31 | 0.06 | 4.60 | 8.43 |
| | 0.02 | 0.85 | 0.01 | | 0.00 | 0.00 | 0.00 | 0.00 |
| *2006 to 2012* | | | | | | | | |
| OLS | 2.82 | −3.21 | −1.49 | 12.44 | 0.41 | 0.02 | 8.42 | 19.08 |
| | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | −2.10 | −1.06 | 3.34 | 0.00 | 0.41 | 0.04 | 7.41 | 16.57 |
| | 0.09 | 0.49 | 0.15 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | −1.72 | −1.08 | 5.67 | 0.00 | 0.39 | 0.05 | 6.83 | 14.83 |
| | 0.02 | 0.37 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |
| *1987 to 2005* | | | | | | | | |
| OLS | 1.91 | 0.19 | −2.67 | 10.74 | 0.31 | 0.05 | 5.92 | 11.73 |
| | 0.00 | 0.30 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | 3.53 | −0.31 | −2.20 | 0.00 | 0.31 | 0.06 | 4.98 | 9.86 |
| | 0.00 | 0.70 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | 3.48 | −0.01 | −1.64 | 0.00 | 0.30 | 0.06 | 4.51 | 8.30 |
| | 0.00 | 0.98 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |
| *1996 to 2012* | | | | | | | | |
| OLS | 1.94 | −0.74 | −2.66 | 11.62 | 0.35 | 0.03 | 6.84 | 14.39 |
| | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Town FE | 0.55 | −0.29 | −0.23 | 0.00 | 0.35 | 0.05 | 5.80 | 12.04 |
| | 0.54 | 0.77 | 0.87 | | 0.00 | 0.00 | 0.00 | 0.00 |
| Tract FE | 0.72 | −0.29 | 1.29 | 0.00 | 0.34 | 0.06 | 5.25 | 10.38 |
| | 0.15 | 0.72 | 0.00 | | 0.00 | 0.00 | 0.00 | 0.00 |

p-values in parentheses.

correlated with the local public goods variables and hence generate omitted variables bias (OVB). For this reason, the focus is more on the variation in the annual estimates over time rather than the level values.

Thus, while it cannot be ruled out that the variation in the hedonic coefficients over the housing cycle is only due to OVB, the explanation that has been provided, the non-random composition of buyers in the market, is at least as plausible for the following reasons. First, the likely OVB that is relevant here is due to unobserved neighborhood characteristics that are correlated with the local public goods. For this to generate the observed fluctuations in the hedonic coefficients, the bias generated by the unobserved neighborhood characteristics would have to vary over the housing cycle. In particular, if the underlying coefficients in the true data generation process are constant over time, then the variation in the OVB must be due to variation in the correlation between the local public goods and the unobserved neighborhood characteristics. And given the countercyclical pattern in the coefficient estimates for all three local public goods, this correlation between the local public goods and the unobserved neighborhood characteristics would have to exhibit a similar pattern. While this is possible, it seems more likely that the changes in the coefficient estimates are being driven directly by changes in the housing market conditions versus indirectly due to changes in the OVB.

Second, this countercyclical pattern is also present in the pooled regressions that include census tract fixed effects which mitigate the OVB due to unobserved neighborhood characteristics (though still are not causal). For example, the results for the 1987–1995 period (down market) are larger in magnitude than for the 1996–2005 period (up market) (see Table 4). These latter results are less likely to be affected by variation in OVB than the annual estimates since they control for unobserved neighborhood characteristics at the census tract level.

Third, the coefficient estimates for the local public goods that are obtained in this study, both from the annual regressions (particularly the average) and from many of the pooled samples are quite reasonable and in line with those from the existing literature (TESTS_K12 and CRIME specifically). So it seems likely that the variation across the housing cycle is not being driven solely by OVB.

When using the data for the full housing cycle, the town and census tract fixed effects estimates of the price impacts for TESTS_K12 are larger than the OLS estimates. This indicates that conditional on the other public goods, TESTS_K12 is negatively correlated with the unobserved town-level neighborhood quality. It could be that, all else equal, households are willing to trade off higher school quality for lower levels of other types of neighborhood quality. Furthermore, the town and census tract fixed effects estimates are similar. It appears that using town-level fixed effects mitigates the OVB arising from unobserved time-invariant neighborhood quality so that using the more disaggregated census-tract fixed effects does not lead to further reduction in this bias.

On the other hand, census tract fixed effects estimates of the price impacts for CRIME are smaller in magnitude than those obtained using town fixed effects which are, themselves, smaller in magnitude than the OLS estimates. This implies that crime rates are higher in otherwise lower quality neighborhoods within the town. So if one does not control for the unobserved neighborhood amenities at the census tract level, the price impact of CRIME appears to be larger (in magnitude) than its true value.

Based on the results in this analysis, there is reason to be skeptical of hedonic estimates using data from the recent downturn as the market has been in a state of excess supply that has not been seen in at least 25 years. Based on the annual estimates of the price impacts for the local public goods, one might be inclined to believe that the relatively large (in magnitude) estimates are a result of a particularly non-random selection of buyers during the recent years where the marginal buyer has a high MWTP for local public goods. But other results using these data are not believable. For example, the fixed effects estimates for the impacts of TESTS_K12 are negative and significant and those for CRIME are positive and significant when using data that are confined to the 2006–2012 period. Of course, this is only for one housing market. More studies are needed to see if these results hold up for other housing markets.

An outcome of this research is the recommendation that, when possible, one use data for a full housing cycle when estimating the house price hedonic as this is likely to give estimates of the MWTP for local public goods that are most representative of the population as a whole. This particularly applies to policymakers who are using the hedonic results to measure the benefits associated with policies related to local public goods. This is justified for the following reasons. First, given that the conjecture of the non-random selection of buyer-types in any given year holds, using data over a full housing cycle will provide a sample that is more representative of the population and hence an estimate of MWTP that it more applicable to the population as a whole. This is particularly relevant when estimating hedonic coefficients for local public goods such as school quality and crime as there is significantly more variation in the annual estimates of the price impacts of these goods (as measured by the coefficient of variation) as compared to the variation in the price impacts of structural characteristics which do not appear to be driven by the housing cycle.

Second, using data for multiple time periods allows for the use of neighborhood fixed effects that can mitigate the OVB that arises from unobserved neighborhood quality. With fixed effects, the hedonic coefficients are identified by within-neighborhood variation and because the measures of local public goods are often constant within the neighborhood, identifying variation is only observed over time. The use of longer time periods allows for more temporal variation that increases the precision of the hedonic estimates.

Third, while the results are not causal, they still match up well with what has been found in the literature. This is particularly true for the fixed effects results based on the 1987–2005 housing cycle.

In the case of a large scale policy change, using the hedonic approach with neighborhood fixed effects with coefficients (other than the intercept) that are constant over time is probably not appropriate since general equilibrium effects due to residential re-sorting will shift the hedonic function. In this case, one is better off estimating separate hedonics before and after the change. The data that cover these two periods should be of reasonable length otherwise one can confound housing market cycle effects with the policy effects. When the impacts of changes in public goods are very local, such as in the discovery and cleanup of hazardous waste sites, the general equilibrium effects will be relatively small and hence it is more reasonable to use one hedonic model with constant coefficients over the whole time period (and neighborhood fixed effects).

The impacts of these major policy changes may be better measured using recently-developed sorting models that capture the general equilibrium effects that these changes can engender (see Kuminoff et al. (2013) for an excellent review of these models). For example, Smith et al. (2004) show that the general equilibrium effects can be quite different than the partial equilibrium effects from a market-wide change in air quality in Los Angeles. Of course, these models assume market equilibrium so they may not be useful using recent data. Furthermore, the recommendation of using data that cover long time periods is appropriate when using these models as the general equilibrium effects will likely vary with the housing cycle just as the hedonic estimates do.

One caveat of this study is that these results are only for one housing market. Furthermore, the results for the Boston metro area may not be generalizable to other U.S. metropolitan areas. Given the higher price levels and higher price volatility in costal metropolitan areas, it would be beneficial to conduct this analysis for an inland metropolitan area. Given the difficulty in collecting the full set of data on transactions and local public goods over a long time period, this is left for future research.

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