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# Making – or Picking – Winners: Evidence of Internal and External Price Effects in Historic Preservation Policies

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**Abstract**:
This article measures the impacts of historic preservation regulations on property values inside and outside of officially designated historic districts. The analysis relies on a model of historic designation to control for the tendency to designate higher quality properties. An instrumental variables model using rich data on historic significance corrects for this bias. The results for Chicago during the 1990s indicate that price impacts from designation inside a landmark district vary considerably across homes inside the districts. Controlling for extant historic quality, which the market values positively, restrictions apparently have negative price effects on average both within and outside districts.

## Introduction

The preservation of historic properties and districts is an increasingly popular form of land-market intervention, especially for local governments. Federally, the National Register of Historic Places (NRHP) comprises over 1.4 million buildings and objects on over 80,000 listed properties.[1] More locally, over 2,000 local historic district commissions exist in the U.S., many arising after the National Historic Preservation Act of 1966. Such policies are typically justified on the grounds of preserving beneficial external effects of historical properties. Other benefits often cited include stabilizing local land markets and conferring subsidies to historic property owners.

Attempts to quantify the costs of such policies have centered on the price effects of historical protection on the "protected" properties. A handful of studies have attempted to estimate spatial spillovers. These studies, however, have all been plagued by the endogeneity of the protection, where higher quality or exemplar historic properties (and districts) tend to receive special regulatory protection. The problem of endogenous policy treatment poses a challenge to those using hedonic price methods in other contexts as well. The sizable literature on the economics of historic preservation has generally found a positive price effect for officially designated historic buildings, often quite large, while lacking a substantive theory to explain why that might be the case. Historic preservation advocates often cite these studies despite the absence of a formal model of the regulators in this context and the inability of these price effects to be interpreted causally. The present analysis addresses both of these shortcomings.

We first outline a general case of land use regulation in the presence of externalities. Then, we demonstrate the usefulness of the model in an instrumental-variables estimation of a hedonic price equation – with an application to historic preservation in Chicago. The model of restrictions on development, owing to concerns about externalities caused by existing structures, identifies theoretical conditions leading to a regulatory takings. The analysis employs these determinants as instruments for endogenous regulatory "treatment" in a hedonic price analysis. This approach demonstrates the importance of addressing endogenous policy variables in standard hedonic approaches.

The innovation of this analysis is twofold. First the explicit consideration of the designation process allows for more consistent estimates of the implicit prices of the hedonic price function. Unlike previous research that typically assumes that landmark designations are exogenous, the estimator employed here corrects for regulators' tendencies to "pick winners." Second, it distinguishes between two conceptually distinct (yet easily conflated) property attributes: historic quality and status as an officially designated landmark property. By directly controlling for historical quality with the novel Chicago Historical Resources Survey, we can differentiate between the effects of the historic preservation policy and the effects of historic quality itself. A building's historic quality may affect its own property value as well as the values of its neighbors. Similarly, official designation of a property may affect its own price as well as the prices of its neighbors. This analysis identifies the separate effects across all four combinations of these two dimensions: own vs. external, historic quality vs. policy status. The results enable us to separate the external (dis)amenity effect of historic properties from the policy effect of merely being designated (where designation confers property use restrictions, some subsidy, possibly some information effects).

**Background**

Governments at the local, state, and federal level implement a variety of policy tools in the name of historic preservation. These policies typically target historic objects, buildings, or districts and reflect multiple objectives (preservation of cultural resources, local economic development, and compensation of private preservation). Officially designated landmarks can enjoy a form of "certification of quality" by government authorities, various forms of marketing by public agencies, subsidized capital and operating expenses, special public services, and other advantages designed to promote tourism. Some preservation policies also involve government caretaking or ownership of resources, as with public museums, monuments, or parks.

The imposition of binding restrictions on private property owners is largely a local affair. Federal regulations (e.g., National Historic Preservation Act of 1966, Antiquities Act of 1906) are either voluntary, subsidy-based, or primarily deployed in the context of federally owned property. Many state and local preservation policies give teeth where the federal rules have few. This paper considers the city of Chicago where, since 1968, the Commission on Chicago Landmarks has recommended landmark designations to the City Council. The stated purposes of the ordinance cover a wide range of topics like preservation, urban renewal, tourism, welfare, and character. By April 2008, 259 individual landmarks and 50 historic districts (comprising over 9,000 properties) had been designated (42 individual landmarks, and 7 districts with roughly 4,500 included properties were added between 2005 and 2008). It is important to note that designation is not voluntary and property owners can appeal designation on hardship grounds. Once designated, alterations or construction affecting the landmark must be approved by the Commission (part of the city's Department of Planning and Development). The City also offers financial incentives for landmark property owners, depending on the type of landmark property. For instance, owner-occupied residences can receive waivers of building permit fees and a 12-year freeze on property taxes.

With such numerous objectives, evaluating the effectiveness of the policy poses a serious challenge. Establishing the counterfactual (i.e., what would happen in the absence of the policy) is complicated by the heterogeneous implementation and the possibility that policy treatments depend on outcome variables. For example, evaluating the effect of designation on a property's sale price becomes complicated when designations "follow the market" (Schaeffer and Millerick 1991) and merely reflect the additional value already recognized in the market, when designations promote urban renewal by targeting areas already in decline, or perhaps both at different times. Making causal inferences about the effects of preservation policies by merely observing how outcomes correlate with designation is thus problematic.

Insofar as preservation policies restrict property use, property values can be expected to decline and sales prices would reflect this. A negative marginal price in a hedonic analysis would reflect this kind of takings. On the other hand, access to financial assistance, such as subsidized loans or fee exemptions, should increase property values and hence prices. The effect of merely honorific designation on prices is less straightforward, possibly having no effect or perhaps conferring cachet or generating higher visibility to designated properties.

Other, more external impacts may be expected as well. If preservation policies or landmark designation bring more stability to a neighborhood by restricting changes in buildings or neighborhood character, property prices may rise because such stability mitigates investment risk for property owners. Landmark designation and historic preservation may also yield rather intangible benefits to the community, which might be reflected in property prices. Ahlfeldt and Maennig (2010) argue that preservation policies conserve the “fabric of built heritage” and add value by producing variety. Asabere and Huffman (1994) suggest that historic preservation policies may solve a market failure in “providing a sense of unity with the past,” a public good. Landmark designation may strengthen the “social fabric” of a community, enhancing local property values and prices (New York Landmarks Conservancy 1977). Schaeffer and Millerick (1991) noted how historic designation can indicate “public commitment” to the neighborhood. Numerous other observers cite landmark designation policies as “catalyzing” rehabilitation of neighboring communities (Listokin et al. 1998, Coulson and Leichenko 2001, Rypkema 1994). Policies can affect housing turnover and thus shift the neighborhood’s equilibrium per Bond and Coulson’s (1989) filtering and neighborhood externality model.

Mason (2005) reviews a sizable literature estimating impacts of historic preservation policies – usually focusing on price impacts.[2] Many studies find evidence to support strong and positive impacts of these policies and fuel local efforts for still more. Recent studies have measured price premiums using a repeat-sales (Noonan 2007) or repeat-assessment (Coulson and Lahr 2005) hedonics. Unfortunately, designations are hardly exogenous to property values or appreciation rates, especially when improving or protecting land values is ostensibly one of the goals of the policy intervention. Ideally, the empirical approach would correct the impact estimates for the bias resulting when the expected impacts affect the likelihood and type of policy intervention. This analysis uses a simple model to explain which areas receive the intervention and integrates several data sources to instrument for the endogenous policy variable in the hedonic price equation. This formal economic model of the determinants of historic preservation policymaking will be the first of its kind as far as we know. The first-stage estimates of determinants of designations offer valuable insights into the economics of local real estate regulation. Accordingly, the 2SLS estimator offers more robust evidence of causal (price) impacts of historic preservation policies than most previous studies' methods and data permit.

**Theoretical Model**

In order to get consistent estimates of the policy effect of preservation policy, we attempt an instrumental variables estimation. In order to motivate this approach, we develop a simple model of the designation process. We imagine a historic preservation regulator maximizing his administrative utility function with respect to the restrictiveness of his preservation interventions, *r*, which we treat as continuous.[3] The regulator has direct preferences over *r* and the utilities of the property owner (*P*) and of other affected stakeholders or neighbors (*N*). Thus, the regulator will choose the restrictiveness of his intervention into the property market (his preservation policy) in order to maximize:

$$u(r;z) = U(P(r;z), N(r;z), r;z) \tag{1}$$

where $U_N$, $U_P > 0$ and $U_r < 0$. The regulator's utility is rising in the welfare of the (regulated) property owner and in the welfare of the neighbors because he cares about property values or because residents apply political and administrative pressure on the regulator to increase their own utility. The regulator balances the property owner's and neighbors' opposing interests in restrictiveness. Property owners prefer to have fewer restrictions ($P_r < 0$) in order to preserve the option of redevelopment.[4] Neighbors favor more restrictions ($N_r > 0$), assuming those restrictions do not restrict *their own* options. Neighbors are expected to value restrictions on nearby properties because this reduces the risk of attractive properties being redeveloped in undesirable ways.[5] The regulator's direct preferences over the restrictiveness arise from administrative costs of the program such as the costs of monitoring and enforcing compliance, as well as the administrator's internalization of the subsidies or tax breaks that local regulation stipulates be provided as compensation to owners of restricted properties. The direct preferences over the exogenous factors *z* will not be important to the model because they are not choice variables for the administrator.[6] The role of exogenous *z* will become important below.

The regulator optimizes by setting the marginal utility of restrictiveness to zero:

$$F(r;z) = U_P P_r + U_N N_r + U_r = 0. \qquad (2)$$

For interior solutions, equation (2) holds at the optimum and thus implicitly defines $r^*$ as a function of *z*. We assume that the second-order condition (that $F_r < 0$) is satisfied so that equation (2) implicitly defines $r^*(z)$ as the utility-maximizing level of restrictiveness. The partial effects of any of these exogenous characteristics on the optimal level of restriction, $r^*$, are thus:

$$\frac{\partial r^*}{\partial z} = -\left(U_P P_{rz} + U_N N_{rz} + U_{rz}\right) / F_r \qquad (3)$$

where $F_r = U_P P_{rr} + U_N N_{rr} + U_{rr} < 0$ is implied by the assumption that the second-order condition is satisfied.[7]

Equation (3) helps us understand how independent variation in any exogenous neighborhood or property characteristic will affect the optimal level of restrictiveness. In general, such a factor will increase the level of restrictiveness whenever $U_P P_{rz} + U_N N_{rz} + U_{rz} > 0$, which means that it increases the marginal utility of restrictiveness. The term is basically the sum of the effect of $z$ on the marginal utilities of each stakeholder, weighted by the regulator's weight of each stakeholder's utility. For a variable $z$ to be a valid instrument in a hedonic price model, it must have a non-zero effect in equation (3), but not affect property values.[8] This implies that $P_z = P_{rz} = 0$, while $N_{rz}$ and $U_{rz}$ do not both equal zero. That is, neighbors and regulators might "care" about the factor, but owners and potential buyers will not care.

The historic nature of the property will obviously not serve as such an instrument. While neighbors might like the external effects of the historic quality ($N_z > 0$), owners might either find the outdated structure onerous ($P_z < 0$) or quaint ($P_z > 0$). More restrictions on historic properties will increase neighbors' utility ($N_{rz} \geq 0$, because reduced uncertainty about the persistence of the positive externalities will benefit risk-averse neighbors) but could make living in a historic property worse ($P_{rz} < 0$, because modifications will be more difficult). From the above, it is apparent that historic quality serves as a poor instrument for preservation: it has an indeterminate effect on preservation, and likely has a direct effect on housing prices. Similarly, neighborhood demographic characteristics will likely affect both preservation decisions *and* housing prices in the neighborhood.

While neither historic quality nor neighborhood demographics will likely serve as valid instruments for historic designation, the interaction of them likely will. Neighborhood demographics that increase the benefits from designation *when historic externalities are present* will increase the likelihood that a property is formally designated. For instance, more owner-occupied units in the neighborhood (recipients of district tax breaks) will increase the likelihood that historic neighborhoods will be preserved, while having little effect on the probability that newer neighborhoods are so preserved. Conversely, having more owner-occupants in the area in 1970 will not affect the utility or disutility for owners *in the 1990s* of living near outdated properties. The identification assumption is thus that $z$ is some vector of interactions between the historic quality of the property being sold or its environs and key neighborhood demographics circa 1970. Such interactions are assumed to have no effect on owner utility ($P_z = P_{rz} = 0$), but important effects on neighbor utility ($N_{rz} \neq 0$). The reasonableness of assuming these interactions (i.e., supply and demand "shifters" for designation) do not belong in a hedonic price equation is further supported by including the uninteracted historical quality, historic density, and various current neighborhood demographics in the hedonic and in using lagged neighborhood demographics measured decades before the property sale. Nonetheless, relevant diagnostic tests for instrument validity are still warranted.

A feature of historic preservation policy in Chicago and elsewhere is that protected properties can be bundled together into landmark districts. This is a detail away from which we have abstracted. Landmark districts offer regulators another policy instrument for making preservation decisions. The decision to include a property in a landmark district, however, follows a similar structure as that outlined above. Administrative costs associated with a building in a district, along with the attendant internal and external effects, are likely to differ from individual designation decisions. Despite these differences, the same factors, $z$, discussed above can serve as instruments for either type of designation. Due to data limitations, this analysis emphasizes the district designations.

**Data and Method**

The following analysis combines data from many sources. First, property data come from actual sales data recorded in the Multiple Listing Service (MLS) for all single-family attached residential property sales in the city of Chicago during the 1990s. This type of property composed the bulk of property sales (and roughly 75% of the housing units) in the city at the end of our data period, and the MLS serves as the information clearinghouse for most arms-length housing sales in the city. The data include 71,275 attached home sales in Chicago from 1990 – 1999. MLS tracks many property attributes such as the address, numbers and types of rooms, and parking availability. For the square footage and year built variables, which are missing in many observations, several fixes are considered. Their missing values are imputed using several auxiliary regressions, following Noonan (2007). List-wise deletion takes care of missing values for other variables in the regressions that follow. The final sample is slightly less than 60,000.

Second, the City of Chicago's Landmarks Division in the Department of Planning and Development provide information on the landmarks (City of Chicago 2004). Added to the official landmarks data is the Chicago Historical Resources Survey (CHRS). Starting in 1983, historians from the Landmarks Commission inventoried the half million properties in Chicago's city limits. The survey report describes the methodology in greater detail (Commission on Chicago Landmarks 1996). Fieldwork obtained detailed information from a final sample of 17,366 historically significant properties. The CHRS property data contain information on addresses, architects, significance, maintenance, and construction dates (http://www.cityofchicago.org/Landmarks/CHRS.html). The CHRS surveyors assessed each CHRS property for its historic value or integrity. Very significant and well-preserved properties were given codes of either red or orange. Sold properties thus fall into one of three categories depending on whether the sold property is a CHRS red or orange property (*R/O*), is given lesser historical quality codes (*OTHER*) but is still significant enough to be in the CHRS, or it was overlooked by the CHRS. Table 1 shows the overlap between the MLS attached-home sales in the CHRS database and the landmark properties. Obviously, Chicago's landmark designating process involves more than just historical quality, especially for district designation.

[TABLE 1 ABOUT HERE]

Third, the analysis also uses a variety of other geographic data for the city including Chicago's Community Areas and Census TIGER files. To link properties to their block-group level Census variables, the Geolytics™ dataset is employed to produce boundary-constant neighborhood demographics for 1990.[9] The Geolytics™ Neighborhood Change Database is used to get tract-level measures of demographics and housing in 1970 for use as instruments.

The empirical model common to hedonic analyses (Rosen 1974) takes the semi-log form:

$$\ln P = \alpha + \beta X + \delta DISTRICT + \varepsilon_{it} \qquad (4)$$

where $X$ is a vector of property attributes and neighborhood quality measures, and *DISTRICT* represents designation status, the discrete analogue to the index $r$ from the theoretical model. The problem of endogeneity arises if, for example, designation status tends to be conferred on properties with unexpectedly high or low sale prices. Given the explicit concern about impacts on designees' sale prices apparent in Chicago's landmark ordinance (Commission on Chicago Landmarks 2006), the possibility of endogenous designation is worth exploring. As shown above, valid instruments will be those variables in $z$ that predict $r$ but do not belong in $X$. The first-stage of the two-stage least squares regression essentially estimates $r^*$ using $X$ and instruments $z$ that are excluded from $X$ in estimating the second-stage, equation (4). This provides consistent estimates of $\delta$ and permits a better understanding of why landmark designations occur where they do. Table 2 lists the variables used in estimating equation (4), their definitions, and descriptive statistics.

Identifying price effects of landmark preservation policies in this setting poses noteworthy challenges and opportunities. First, due to the rarity of individual building designations (even in a landmarks program as vibrant as Chicago's), the data only permit robust estimations of district designations' price effects. The final sample has 284 sales in individually designated landmark buildings, 272 of which are in five different buildings. This makes it difficult to distinguish between the effect of building-specific unobservables and the individual designation effect. Fortunately, enough variation exists for district designations. Second, although previous research tends to focus on the effects of designation on a property's own price (Coulson and Leichenko 2001 and Ahlfeldt and Maennig 2010 are notable exceptions), the external effects of historic preservation – effects of designation on nearby property prices – remain arguably the most important effect of the policy. Even if the own-price effect of designation is negative, preservation policies can still be justified on positive externality grounds. Including measures of proximity to landmarks as controls in equation (4) allows the recovery of amenity (or disamenity) effects of designation. This avoids the limited overlap between the home sales data and the landmarks inventory because the nearby landmarks include *all* landmarks (e.g., Douglas Tomb, St. Ignatius High School, Robie House), regardless of their housing a home sale during the 1990s. The external price effects of landmark building designation are thus recoverable, even if its own-price effects are not.

[TABLE 2 ABOUT HERE]

**Results**

First, Table 3 reports the results of simple OLS regressions across a number of specifications of the hedonic price equation. Column 1 of Table 3 presents the results from a fairly naïve regression of landmark district status and property characteristics on log real sales price. These results suggest large price premiums (approximately 25%) for homes in preserved districts.[10] (Unconditional mean prices are 73% higher in districts.) The coefficients on the control variables are either unsurprising or readily interpretable.[11] Although homes in districts sell for a premium over comparable homes, and homes in districts tend to have nicer attributes than non-district homes, districts are inherently spatial and thus geographic controls are vital. Column 2 adds geographic and local neighborhood demographic controls and a set of 77 Community-Area (CA) fixed effects. As a result, the price premium for properties in landmark districts falls to roughly 4%. Controls for geographic position of the structure and census block-group demographics also exhibit generally expected signs.[12] Nicer homes are in districts, and districts are in nicer neighborhoods. In this sense, these historic preservation districts appear to be "picking winners." Failing to control for both housing attributes and geography can severely distort the apparent effects of landmark districts. Controlling for them, the premium falls much closer to zero.

While the effect of designation on the price of the designated home is certainly important, landmark designation usually has other aims. In some senses, the primary rationale for preservation is that the threatened historic properties embody positive externalities overlooked by private interests when considering redevelopment. These historical spillovers should be captured at least partly by neighboring property values. Column 3 includes counts of (individually designated) landmarks in the home's block group and a measure of the share of the home's block group's land area that is occupied by another landmark district. The proportion of the sale's block group that is included in a block group *exclusive of the sale's own district* seeks to identify externalities associated with historic preservation policies on non-designated properties. Column 3 shows sizable external effects of landmark districts and rapidly diminishing returns to nearby landmark buildings. A ten percent increase in the nearby area within a district is associated with a 2.6% rise in property values. Having a landmark nearby increases price, but additional individual landmarks beyond that actually diminish property values. Properties sold inside of districts receive a roughly 5% premium after controlling for proximity to other nearby designations. This suggests that it is better to be near a district than in it (i.e., the *DistShare* coefficient is substantially larger than the *DISTRICT* coefficient).

Even though the results in column 3 of Table 3 show a smaller price effect of districts than the prevailing literature on price effects of historic preservation efforts,[13] it does make use of variables that are typically available in other cities. One important source of bias in the estimator is the possibility that the landmark variables are correlated with unobserved historic quality. If historic quality is valued (either positively or negatively) in the housing market, the correlation of historic quality with district designation will bias the coefficient on *DISTRICT*. The model in column 4 leverages the measures of historic quality available for the roughly 17,000 properties deemed sufficiently historically significant to include in the CHRS. Measures of the property's own historical quality (its color code ranking and whether it has a known name like "The Smith Building") and measures of the nearby density of historical properties (counts of CHRS red/orange and other CHRS properties within three mutually exclusive buffers) are included in the model for column 4. As a result, the own-price effect of *DISTRICT* gets much stronger, the price effect of nearby districts actually grows more positive, and the external price effect of nearby landmark buildings remains unchanged. Controlling for nearby historical properties does little to affect the price effects of proximity to individual landmarks but, like the bias for *DISTRICT*, removes a downward bias on the effect of nearby landmark districts. Interestingly, less significant CHRS properties generally sell for a premium while the premium for more significant (red- or orange-coded) CHRS buildings is insignificant.[14]

[TABLE 3 ABOUT HERE]

The sensitivity of the implicit prices of landmark variables to controls for historical quality marks an important contribution to the previous work, which has generally failed to include objective measures of historical quality distinct from official designation (Noonan 2007, Coulson and Lahr 2005). In column 4, the inclusion of controls for a building's historic quality has a significant impact on the *DISTRICT* coefficient. For landmark districts in Chicago, the omitted-variable bias appears most strong when geographic and neighborhood quality variables are absent. Neighborhood historic quality may predict which properties receive landmark status. There is evidence of external effects of a historic preservation policy, a major justification for the policies, although the nonlinearity complicates matters.

While the results in Table 3 are informative, the discussion in the theoretical section suggests that taking the OLS coefficients as policy effects – even in the face of so many control variables – is not necessarily justified. Many unobserved factors that affect the sales price might also affect the probability that a property is designated. Chief among these would be a poorly observed historic quality or maintenance level in older properties. While Noonan (2007) and Coulson and Lahr (2005) attempt to deal with this problem via repeat-sales or repeat-assessments models, the approach here uses instrumental variables. Table 4 presents estimates of a hedonic price equation similar to the model presented in column 4 of Table 3 estimated using a two-step efficient GMM IV estimator (with standard errors robust to arbitrary heteroskedasticity). Table 4 shows how varying the geographic controls and the instrumental variables can affect the main coefficient of interest, *DISTRICT*, as well as other important historic and policy variables' coefficients. The instruments used in Table 4 constitute a subset of the available IVs. Table 4 presents a set of models with a consistent set of instruments that perform well in the first stage, have good diagnostic statistics, and represent well the results with other IV strategies. Coefficients on the other exogenous variables in the hedonic, as listed in column 4 of Table 3, are suppressed for space considerations.[15]

As described above, the instruments are interactions of a neighborhood demand (for historic preservation) shifter and a measure of historic quality around the property being sold. The neighborhood demand variable is percent of housing that is owner-occupied measured at the tract-level in the 1970 census, which immediately followed the passage of the Landmarks Ordinance but predates sales by at least two decades. The historic quality variables are (a) an indicator for the property's historic significance (either it being a CHRS property or being a red- or orange-coded CHRS-property), and (b) an indicator of the historic density around the home (the number of CHRS or red/orange properties within 100 meters of the home). The (difference-in-Sargan) C statistic confirms that *DISTRICT* is indeed endogenous in the OLS price equation at the 0.01% level.

[TABLE 4 ABOUT HERE]

The instrumental variable approach to dealing with the endogenous district treatment variable performs well overall. The first column of Table 4 shows the instruments performance in the first stage conforms largely to expectations: one has the expected sign and significance, and one is insignificant. The Kleibergen-Paap rk Wald F statistic is sufficiently high to calm worries about weak instruments. The Hansen J statistic, which tests for over-identification, is too low to reject the null hypothesis that the instruments are excludable. As suspected, modeling *DISTRICT* as endogenous has substantive effects on the coefficients of the other historic variables, although these effects are small relative to the change in the *DISTRICT* coefficient itself. The district effect reverses sign (from positive to negative) when endogeneity is dealt with, but the negative effect is significant only at the 10 percent level.

Another interpretation of the endogeneity of *DISTRICT* in the OLS models (Table 3) is that it arises from unobserved neighborhood quality being correlated with designation. A remedy for this is to simply include block-group (BG) fixed effects in the estimation.[16] Column 2 of Table 4 implements this strategy and shows some further surprising results. First, the coefficient on *DISTRICT* collapses to zero. Second, the *DistShare* coefficient switches sign significantly. Controlling for block-group attributes (including unobserved ones) suggests that the effect of a district in a neighborhood on non-designated properties is strong and negative. The effect of individual landmarks within a neighborhood goes to zero: unsurprising given that these block-group-level variables offer little within-block-group variation across the 1990s. The coefficients on the historic quality of the unit change slightly to align with expectations: historically significant buildings (*R/O*) sell for a higher premium than less significant buildings (*OTHER*), which in turn sell for more than buildings not included in the CHRS at all.

One important result in column 2 is that the difference-in-Sargan C-statistic still rejects the exogeneity of *DISTRICT* at better than the 1% level. This suggests that the endogeneity in Table 3 is not coming solely through unobserved neighborhood characteristics. Even within tightly defined geographic areas, inclusion inside historic districts is endogenous. Column 3 of Table 4 uses an IV estimator with the instruments from column 1 to identify the effects of *DISTRICT* designation in the presence of block-group fixed effects. The first-stage results show the expected signs for the instruments and evidence of strong instruments. The instruments fail the over-identification test at the 0.5% level, however. This casts some doubt on the use of the same IVs with different geographic controls. These results are presented primarily to demonstrate, especially in the presence of block-group fixed effects, the sensitivity of results to identification strategy. Both the *DISTRICT* and *DistShare* coefficients are now positive and highly significant. However, a slight change in the instruments in column 4 (redefining them to use only red- and orange-coded CHRS properties instead of any CHRS properties) pushes both coefficients back negative (although the *DISTRICT* coefficient is insignificant). These instruments are strong, consistent with theory, and pass the over-identification test at the 10% level. Altering the IVs between columns 3 and 4 modestly influences the other coefficients of interest.

The inconsistency across columns in Table 4 raises several important points. First, the estimated price effect of a landmark district policy – both endogenous and inherently spatial – is very sensitive to the identification strategy. Alternative instrument sets and alternative geographic controls yield different results. Switching from one set of plausible controls for neighborhood quality to another (e.g., column 1 versus column 3) dramatically influences the coefficients of interest. It also influences the apparent validity of the instrumental variables. The exclusion restrictions for the IV set when there are Community-Area fixed effects (column 1) may not be valid when there are block-group fixed effects (column 3). Although Hansen's J may lack power against the null of valid instruments, the diagnostic statistics for column 4 do lend more confidence to those results with the alternative – and arguably equally plausible – set of instruments.

Second, the sensitivity of the results to a minor change in the instruments invites speculation on the nature of the identification being used. It is possible that the effects of district designation are highly heterogeneous and that different instruments identify different treatment effects. Indeed, looking at the magnitude of the instruments' coefficients in the first-stage equations between columns 3 and 4, we see that the second strategy (column 4) seems to place more weight on the units' own historic significance (*R/O×OwnOcc70*) relative to the historic density (*ROin100×OwnOcc70*). Obviously, the second strategy also puts more weight on the most significant properties than the first strategy does. The properties singled out in the first stage as highly likely to be designated thus differ somewhat across columns. Perhaps properties included in districts because of their historic neighbors benefit through association with the nicer historic area, whereas properties included in the district for their own sake suffer from the restrictions imposed on them as an "anchor" of the historic district.

Column 5 of Table 4 sheds light on another aspect of heterogeneous effects: the vintage of the district. This IV hedonic model includes a second endogenous regressor: the years elapsed since the property was initially designated in a district (taking a value of zero for never-designated properties). Column 5 uses all four instruments from columns 3 and 4 as excluded instruments. The Kleibergen-Paap statistic of 46.98 suggests strong instruments, and the model passes the over-identification test at the 10 percent level. These results show that the immediate impact of designation is an over 40 percent increase in value, but that this premium declines over time. The negative vintage effect is as expected for at least two reasons. First, a major subsidy for district homes is the 12-year property tax freeze, which suggests that the district premium should be dissipating for at least the first dozen years after designation. Second, adverse effects of being designated may grow with time. As time passes the optimal use of the structures may change, and the constraints imposed by historic designation policies become more onerous. Eventually, these negative effects come to dominate the positive effects, leaving a net negative effect of the policy.

The negative coefficient on years since designation implies that the effect of designation will be about zero for a unit sold 14 years after designation, which is quite close to the average for districted units of about 13 years. Thus, the effect of designation on the average unit sold in a district is quite close to zero, and the effects estimated through instrumental variables could change drastically based on subtle changes in the identification strategy. It is possible, for instance, that the set of instruments in column 3 tend to identify effects off of more recent districts while the instruments in column 4 identify effects off of older districts. This makes sense since the column 4 instruments focus on the most historically significant structures, which were more likely to be included in the earliest districts after passing the historic preservation ordinance (Noonan and Krupka 2010). While effect heterogeneity arising from differences in the vintage of the district is demonstrably important, we also suspect that there is heterogeneity of effect across units within any given district as described above. The hedonic price in Table 4 is an average treatment effect of district designation that masks this kind of heterogeneity.

Another interpretation of the endogeneity of *DISTRICT* in column 2 of Table 4 maintains that unobservables correlated with the policy treatment bias the estimator. Even in the presence rich controls for historical quality and local neighborhood quality, *DISTRICT* could still be endogenous. Controlling for block-group-level fixed effects, as in columns 3-5 of Table 4, identifies the districts' price effects from the (within block-group) differences in housing units' probabilities of being designated. These differences arise from two sources: spatial (i.e., whether the unit's building is included inside a district's boundaries) and temporal (i.e., whether the unit sold before or after its building was included in a district). If some valuable and unobserved building characteristic makes a building's inclusion inside a district more likely, the estimated coefficients could be biased. Columns 6 and 7 of Table 4 hold all such characteristics constant through building-specific fixed effects (at the cost of only 3,249 observations that are singletons for their building or address).[17] This approach controls for the within-block-group *spatial* variation in designation likelihood and thus identifies the price effect of *DISTRICT* from the temporal variation. This effectively compares sales prices in buildings that had sales before *and* after that building was designated in a district.

Column 6 shows the building fixed-effect OLS estimator, which drops building-invariant regressors.[18] There, we see a negative, insignificant effect of *DISTRICT* and a significant and large negative coefficient on *DistShare*. Concerns about endogeneity persist, even after fully controlling for spatial unobservables, perhaps because designation follows building-level appreciation so that a before-after comparison presents biased estimates. Using the IVs, discussed next, the null hypothesis that *DISTRICT* is exogenous can be rejected at the 1% level.

In the building fixed-effect context, the instruments need to predict the timing of a sale (before or after designation) rather than whether a building will become designated. The interaction between *R/O*, the change in block-group income from 1980 to 1990, and *saleyear* captures this risk of designation ($R/O \times \Delta income$) and timing of sale. A second instrument uses a different demand-shifter, percent college educated, but is otherwise the same. The instruments are strong, have the expected sign, and easily pass the over-identification test. The *DISTRICT* coefficient is again large, negative, and significant (implying a 37% reduction in sales price due to being designated), as is the *DistShare* coefficient.[19]

**Discussion**

Overall, the results in Table 4 reinforce the sensitivity to modeling assumptions and identification strategies. The more robust results (in terms of model diagnostic tests and controls for policy endogeneity) fail to support positive price effects of the *DISTRICT* policy treatment. That historic districts have nonpositive price impacts on the designated properties appears robust to alternative instruments and to different spatial control variables. That said, simply changing the geographic controls without changing the IVs can lead to very different conclusions. Using various instrument sets (not reported here), the results for the *DISTRICT* coefficient are typically large and negative when the instruments appear strong and valid. Table 4 shows one exception, however, to highlight the sensitivity of the results to identification strategies. We attribute much of this sensitivity to heterogeneous effects of district designation arising through vintage effects of the district itself and through whether the property is an "anchor" for the district or merely "along for the ride." At the extreme – controlling for building-specific unobservables – the own-price effects of districts appear negative.

While the results do not consistently support positive own-price impacts of districts (i.e., within the district), the findings for the external price effects of districts (i.e., outside the district) are more ambiguous. The positive external price effects from districts appear largely explained by omitted local neighborhood quality. Models that control for block-group or building fixed effects show negative external price effects. The model in column 3 again proves an exception to this. The external price effects of the policy are likely quite heterogeneous across properties and districts, like the own-price effects, and the sensitivities with regard to the identification of the *DISTRICT* coefficient extend to other variables in the coefficient vector. With these sensitivities in mind, the abundance of evidence across numerous specifications (not all reported here) nonetheless suggests a large negative effect of historic preservation policy on preserved properties and even on the surrounding area.

The large district effects reflect several forces. On the one hand, inclusion in a historic district restricts redevelopment options of owners (and buyers), which should lower the value of the property. On the other hand, district designation may offer benefits such as tax reductions or a kind of certification of (or signal for) the property's quality. For attached housing in Chicago, the tax benefits are outweighed on average by the long-term restrictions on renovation. The certification effects also appear minimal given the model with excellent controls for historical quality. Furthermore, the stability that district designation brings to the neighborhood's overall character (in terms of the types of land uses and buildings' external appearances) could constitute a *disamenity* to buyers. Districts may relatively lack access to modern urban conveniences. Constraining new construction in districts can tip neighborhoods in a dynamic filtering model and limit their rent and income growth (Bond and Coulson 1989). These effects of the policy are distinct from the effects of the historic characteristics that are preserved. All our results suggest that historic properties sell for premiums in Chicago. The negative effects are for the policy restrictions, not the qualities preserved.

The *external* effects of historic districts appear to be large and negative, with the exception of column 3 of Table 4. As the results from columns 2 and 6 emphasize, this negative effect of *DistShare* is not an artifact of our IV approach. The negative external effect of district designation results from controlling for neighborhood quality. Homes near districts tend to be in nicer block groups in the first place, again suggesting that the district designation process may be doing a better job of reflecting strong social fabric and neighborhood identity than of creating it.

Still, a negative effect even after controlling for neighborhood quality is somewhat surprising. One might think that the preservation of desirable areas nearby would increase home values either through a direct amenity effect or through the displacement of demand from the development-constrained areas into adjacent blocks. This is generally not the case once block-group effects are included. It is not surprising that the external effects are of the same sign as the internal effects of historic districts. The own-price effect captured by *DISTRICT* is the net price effect of (the property itself) being subject to the regulation and the within-district spillover from other districted properties. Something that decreases housing demand within the district may very well also decrease demand near it. This could arise directly if regulated neighbors are a disamenity. This would easily explain negative *DISTRICT* and *DistShare* effects. Alternatively, it could arise even in light of positive amenity effects of landmark districts. If closer proximity to districts signals increased threat of future inclusion in a landmark district, then the negative implicit price of *DistShare* echoes the nonpositive *DISTRICT* effect. As with the effects of district designation within districts, the negative effect of near-by districts is distinct from the effects of near-by historic properties, which might be positive despite the negative effects of the preservation policy.

A second impact of preservation policies is via external price effects of individual landmarks on nearby properties.  Table 4 shows the price effects from proximity to officially designated landmarks, conditional on the historical nature of the neighborhood.  The proximity to nearby landmarks is measured by *CountLmk* and $CountLmk^2$.[20]  The effects of individual landmarks seem to be largely explained by unobserved neighborhood quality measured at a scale smaller than a Community Area.[21]

**Conclusion**

After developing a simple theoretical model that casts doubt on the exogeneity of policy variables and helps identify some plausible instruments, this paper demonstrates the value of correcting for policy endogeneity in hedonic price analyses by using a more robust estimator with richer data than previous studies of the implicit price of historical landmark designation. OLS estimation of the hedonic price model offers results consistent with much of the previous literature; prices are higher in districts.  The rich data allow unprecedented controls for historic quality, neighborhood quality, and property attributes.  Controlling for historical variables influences the estimated price effect of districts in OLS, because much of these effects hinge on difficult-to-observe neighborhood historic quality.  The vital role of historical quality in the OLS and 2SLS models demonstrates how careful analysis of the impact of historic preservation policies depends on controls for existing historic quality.[22]

Most critically, the endogeneity of district designation status is explored using instruments derived from a simple model of regulator behavior.  The empirical results provide evidence that previous methods may be particularly vulnerable to endogeneity and omitted-variables biases in this context. The variable of interest (historic designation) is found to be endogenous even in the presence of neighborhood and building fixed effects. The IV approach has an advantage in being able to identify prices for designation when designations rarely occur between observed sales. Broadening the sample also allows identification of price effects for older districts, which should differ substantially from newer ones. The IV approach also allows for the explicit estimation of the determinants of designation in the sample. For a variety of instrument sets that leverage the historic quality measures from the CHRS, the first-stage results point to strong instruments whose impacts are consistent with theory.

The change in the effect as one moves from OLS to IV strategies suggests that districts appear more attractive in the simpler model because they are being systematically selected by the administrators of the program. This should not necessarily be seen as a critique of the program. In some sense, that is exactly what the program is meant to do: preserve the nicest parts of the city from undesirable changes. Given this selectivity, however, one must interpret the positive partial correlation between housing value and historic preservation policies very carefully. The policies are not applied at random, and the selection implicit in the policy tends to focus on relatively desirable historic homes in nice, historic areas.

Even though the policy effect of historic preservation policies in Chicago appears to be nonpositive, the policy itself may yet find justification via its impact on the historic quality of the building stock. A significant amenity effect of historic properties would suggest a positive externality that may be underproduced. The coefficients of the historic buffer variables in Table

4 (to the extent they are interpretable) suggest close proximity to significant historic properties may boost prices. A regulation that leads to more (or at least prevent declines in) historic quality might yet yield net benefits even though owners of the "preserved" buildings may suffer some losses. While the literature has not yet established a causal link between historic preservation policies and historic quality,[23] the results here do hold two important implications. First, the endogeneity of historic designation in the hedonic price model suggests that future researchers should be mindful of possible reverse causality stories in understanding other historic preservation policy impacts. Second, a policy that imposes costs on designees for the sake of preserving positive external effects of historic quality exhibits interesting distributional and efficiency questions worthy of future research.

In the 2SLS hedonic, the balance of the evidence points towards large, negative effects of designations on preserved properties on average, with a substantial amount of heterogeneity both within and across districts. Older districts have much stronger negative effects, while newer districts appear to have more positive effects. The results are consistent with a story in which properties included in districts due to their own historic value are negatively affected by the preservation policy while properties included in districts because of the historic value of their neighbors experience increases in sales price. The heterogeneity of the effects means that the estimated policy effects will hinge crucially on the kind of variation used to identify those effects. This heterogeneity arises in this single-city analysis that essentially holds constant program administration and metropolitan context. Program effects might differ markedly in other cities.

It merits emphasis that the hedonic price describes the marginal price effect of designation, not a welfare effect. Price impacts may result as much from shifting demand *or* supply. Since district designation is designed to constrain supply, the lower prices for homes in districts suggests that the restrictions on property use indeed lower demand for the housing assets. These lower prices may be indicative of a takings common to preservation laws where individuals suffer costly encumbrances for the sake of positive external benefits. But, since we show that the external effects of the policy are not robust to controls for unobserved neighborhood or building quality, there is no strong evidence that these external benefits are valued in the housing market. Net benefits of landmark districts may still arise due to public goods benefits of preservation not captured by property values (e.g., Kling et al. 2004, Chambers et al. 1998), however.

Evaluating a preservation policy involves several aspects. First, there is the effect of the policy on the preserved buildings – including effects on historical quality preserved and property values. Next, there is the effect of the preservation policy on the surrounding properties and neighborhood – including on neighborhood dynamics and property values. If preservation policy has increased the supply of the historic resource in a city, then some of the credit for the external effects of historic structures belongs to the policy. Preservation policy, however, might not increase the total amount of the historic resource especially if it is perceived as a taking. Preserving heritage that already exists is not the same as increasing supply. As Turnbull (2002) has shown, the threat of "preservation" can speed owners towards redevelopment, which could lead to an overall *decrease* in historic resources over the long run (even if the policy itself effectively preserves the properties it does designate). These more dynamic effects of preservation policy on the stock of historic properties in a city have received no attention to our knowledge, mostly because good data on the stock of historic resources over time in an area do not exist. These dynamic effects could play critical roles in the general efficiency of preservation programs and on their optimal administration. Even a more robust approach to estimating "own" and external price effects of landmark preservation, such as that presented here, does little to answer the larger questions of "how do preservation policies affect the stock of historic buildings?"

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**Footnotes**

1. From the National Register of Historic Places Official Website (http://www.nps.gov/nr/about.htm). For additional discussion, see Schuster (2002) and Swaim (2003).

2. Interestingly, the prominent impact studies by economists and preservationists alike focus on price impacts and economic development impacts – not on impacts on historic resources actually preserved.

3. Obviously, preservation status is discrete. Noonan and Krupka (2010) deal with this aspect of policy explicitly in a very similar model. Generally, the "restrictiveness" can be thought of as a continuous latent variable that includes the degree of curtailed transformation rights and stringency of enforcement that makes $r$ appear continuous in practice even if designation status itself is dichotomous.

4. The regulation might compensate restricted owners with subsidies or signal their property's quality to potential buyers. However, owners would prefer to have these advantages without the restrictions.

5. Neighbors might object to restrictions if preservation prevents desirable redevelopment, or if the policy also restricts their options, as might be the case in historic districts (see below).

6. These factors will include characteristics of individual properties, owners, or neighborhoods and will be discussed below. The preferences over $z$ will vary depending on which exogenous factor is considered.

7. A sufficient condition for this condition to hold are that administrative costs increase more-than-linearly, that the added benefit to neighbors is decreasing in restrictiveness, and that the costs imposed on property owners by restrictions increases about linearly.

8. This condition holds even under other regulator objective functions, such as one that sets $U_P$ very close or equal to zero as Glaeser (2006) suspects for the Landmarks Preservation Commission in New York.

9. Missing values for median income, median home value, and population density in 1990 are imputed at the block-group level using an array of lagged and concurrent variables from the Geolytics™ database, including variables on neighborhood demographics and housing stock characteristics. Further details are available from the authors upon request.

10. Following Halvorsen and Palmquist (1980), percent effects of dummy variables like designation status are derived from the expression: exp(β) – 1.

11. The coefficient on *rooms*, for instance, must be interpreted in light of the control for *lnarea*.

12. The 77 Community Areas correspond more or less to the local concept of a neighborhood. Examples include Hyde Park, Lake View, and Lincoln Park. The richness of the geographic controls sometimes makes them difficult to interpret. For instance, the positive coefficient on distance to the lake is surprising, but the lakefront is lined almost completely with parks, the distance to which has a much larger and negative coefficient.

13. Coulson and Lahr (2005) find 14 – 23% effects for Memphis districts. Coulson and Leichenko (2001) and Leichenko et al. (2001) find 5 – 20% price effects for individual designations in Texas. Coulson and Leichenko (2001), and Ahlfeldt and Maennig (2010) estimate (universally) positive price effects from proximity to other designated properties. Noonan's (2007) study of Chicago data finds a small, negative effect for *DISTRICT*. Schaeffer and Millerick (1991) also find discounts for two Chicago districts.

14. This likely arises from the CHRS surveyors downgrading color rankings for properties that have undergone renovations, which are presumably valuable to owners.

15. The coefficients for the other property attributes do not change appreciably across specifications. Full results are available from the authors upon request.

16. Thanks to an anonymous referee for this suggestion.

17. Time-invariant unit-specific unobservables could be addressed via a repeat-sales estimator. Such an approach comes at a high cost in terms of drastically reduced sample size. Noonan (2007) nonetheless estimates a repeat-sales model for Chicago and accordingly loses the ability to identify the effects of designation on the price of the sold property itself.

18. The results for the unit-specific attributes (e.g., *rooms, baths*) are similar to those reported in Table 3, lending some confidence to this fixed-effect approach.

19. Spatial autocorrelation is a concern in this kind of analysis. The preferred models with increasingly refined spatial fixed effects exhibit decreasing spatial autocorrelation (the residuals in columns 1 and 4 of Table 4 have Moran's $I$ statistics of 0.23 and 0.05, respectively, based on a weights matrix that defines neighbors as observations within one-eighth mile or one city block) while retaining similar results overall, lending some confidence to the results. The building-level fixed effects model exhibits negative autocorrelation (Moran's $I$ = -0.09), implying the standard errors may actually be inflated and thus offering conservative significance tests. More sophisticated spatial econometric approaches could bring even more robustness to the results.

20. When either a linear count variable or a single distance-to-nearest-landmark variable replaces the quadratic formulation, the effects of proximity to landmarks are essentially zero. But as the quadratic specification shows, this zero effect masks considerable heterogeneity. For the approximately 15% of observations with more than one landmark in their block group (with a

maximum of 5), the effects are negative. If a quadratic distance-to-nearest-landmark specification replaces the quadratic *CountLmk* specification, the linear distance variable is insignificant and the quadratic term is generally significant and negative as in Table 4.

21. These results compare nicely to the results in Noonan (2007), who uses only a linear *CountLmk* term. His repeat-sales estimate of the *CountLmk* effect ranged from -0.02 to -0.073, with only one being statistically significant. The estimate in column 7, our most similar model, shows a price effect somewhat closer to zero, although it employs ten times more observations because it need not rely on only repeat-sales.

22. Identifying a policy effect without controls for historical quality poses a serious challenge. In some contexts – like the NRHP but not Chicago's landmarks – eligibility is tied to a threshold structure age such as 50 years. These arbitrary cut-offs are apt to be exploited using regression discontinuity techniques to infer policy effects even in the absence of good controls for historic quality.

23. The idea that preservation policies lead to more history being preserved is often presumed by advocates and analysts. Rigorously identifying a policy effect, however, may prove challenging. The endogeneity problem (preserved historic buildings attract designations rather than designations leading to preservation) persists here. Moreover, even if the policy effect on the "treated" to prevent a decline in historic quality, the mere existence of this threat of regulatory takings may yield important and unfavorable policy effects on the "untreated" when historic preservation policies spur preemptive redevelopment (Turnbull 2002).

**Table 1: Overlap between CHRS and Landmark Designations in Sales Data**

| | | Is a landmark: landmark building | landmark district | Not a landmark | Total |
|---|---|---|---|---|---|
| In CHRS | "red" or "orange" | 176 | 548 | 1,685 | 2,409 |
| | not "red" or "orange" | 1 | 358 | 903 | 1,261 |
| | Not in CHRS | 147 | 1,090 | 66,381 | 67,618 |
| | Total | 323 | 1,996 | 68,969 | 71,288 |

Note: Table lists the frequency of landmark status and the inclusion of the property in the Chicago Historic Resources Survey. Properties in the survey were rated as being either historically significant ("red" and "orange" properties) or less significant.

**Table 2: Definitions and descriptive statistics**

| Variable | Definition | Mean | Std. Dev. |
|---|---|---|---|
| *lnP* | ln (real sales price, adjusted to 1 January 2000 $ using Chicago's housing CPI deflator) | 11.898 | 0.659 |
| *DISTRICT* | in a landmark district at time of sale? | 0.036 | 0.186 |
| *CountLmk* | number of landmark buildings in block-group, exclusive of property's own status, at time of sale | 0.328 | 0.748 |
| *DistShare* | share of block-group's land area inside a landmark district, exclusive of property's own district, at time of sale | 0.033 | 0.096 |
| *named* | sale property is in building in CHRS that has a building name (e.g., "The Overton Building") | 0.009 | 0.094 |
| *OTHER* | sale property is in CHRS not as "red" or "orange" | 0.009 | 0.095 |
| *R/O* | sale property is coded "red" or "orange" in CHRS | 0.044 | 0.205 |
| *OTHERin100* | count of non-R/O CHRS properties within 100 meters | 0.979 | 3.264 |
| *OTHERin250* | count of non-R/O CHRS properties between 100 and 250 meters | 5.201 | 11.077 |
| *OTHERin500* | count of non-R/O CHRS properties between 250 and 500 meters | 15.192 | 25.262 |
| *ROin100* | count of R/O CHRS properties within 100 meters | 3.157 | 6.564 |
| *ROin250* | count of R/O CHRS properties between 100 and 250 meters | 17.494 | 27.460 |
| *ROin500* | count of R/O CHRS properties between 250 and 500 meters | 53.228 | 71.484 |
| *lnarea* | ln (area of unit in feet$^2$) | 7.103 | 0.417 |
| *yearbuilt* | year built | 1956.792 | 26.924 |
| *units* | number of units in the building | 150.405 | 231.928 |
| *rooms* | number of rooms | 4.743 | 1.690 |
| *bedrms* | number of bedrooms | 1.866 | 0.801 |
| *baths* | number of baths | 1.553 | 0.659 |
| *mbbth* | master bathroom dummy | 0.489 | 0.500 |
| *fireplace* | number of fireplaces | 0.313 | 0.515 |
| *garage* | garage dummy | 0.368 | 0.482 |
| *parking* | garage or parking dummy | 0.884 | 0.320 |
| *parkspot* | parking spot dummy | 0.172 | 0.377 |
| *waterfront* | on the waterfront dummy | 0.072 | 0.258 |
| *dCBD* | distance to CBD in km | 6.953 | 5.023 |
| *dLake* | distance to Lake Michigan in km | 1.989 | 3.125 |
| *dWater* | distance to closest water (river, lake) feature, in km | 0.876 | 0.804 |
| *dCTA* | distance to closest CTA rail line in km | 0.737 | 0.695 |
| *dPark* | distance to closest park in km | 0.416 | 0.332 |
| *North* | on the north side of the city dummy | 0.916 | 0.277 |
| *Lat* | decimal degrees north | 41.929 | 0.050 |
| *lnINC* | median household income (in $1000s), block-group, 1990 | 33.083 | 21.964 |
| *college* | percent with a college degree, block-group, 1990 | 0.476 | 0.224 |
| *lnVAL* | median house value (in $1000s), block-group, 1990 | 219.804 | 168.199 |
| *lnDENSITY* | population density (1000s/km$^2$), block-group, 1990 | 33.923 | 23.893 |
| *white* | percent white, block-group, 1990 | 0.721 | 0.234 |
| *medyrblt* | median year built for residences, block-group, 1990 | 1953.018 | 13.458 |
| *NewCons* | percent of housing units built in last 10 years, block-group, 1990 | 0.094 | 0.150 |
| *saleyear* | year of sale | 1995.375 | 2.812 |
| *OwnOcc70* | percent of housing units occupied by owner, tract, 1970 | 0.281 | 0.322 |
| *ROdColYr* | Interaction of *R/O*, *saleyear*, and 1980-1990 change in *college* | 12.551 | 90.380 |
| *ROdIncYr* | Interaction of *R/O*, *saleyear*, and 1980-1990 change in *income* | 59.984 | 388.422 |

**Table 3: OLS Regression Results**

| Variable | 1 Coef. | 1 t | 2 Coef. | 2 t | 3 Coef. | 3 t | 4 Coef. | 4 t |
|---|---|---|---|---|---|---|---|---|
| *DISTRICT* | 0.2209 | 21.64 | 0.0385 | 4.51 | 0.0528 | 6.12 | 0.1448 | 12.41 |
| *DistShare* | | | | | 0.2576 | 16.36 | 0.4175 | 21.84 |
| *CountLmk* | | | | | 0.0608 | 10.42 | 0.0637 | 10.87 |
| *CountLmk*$^2$ | | | | | -0.0248 | -12.50 | -0.0240 | -11.91 |
| *named* | | | | | | | 0.0232 | 1.98 |
| *OTHER* | | | | | | | 0.0490 | 4.55 |
| *R/O* | | | | | | | 0.0099 | 1.09 |
| *OTHERin100* | | | | | | | -0.0077 | -12.27 |
| *OTHERin 250* | | | | | | | 0.0001 | 0.39 |
| *OTHERin 500* | | | | | | | 0.0003 | 3.92 |
| *ROin100* | | | | | | | 1.0E-05 | 0.04 |
| *ROin250* | | | | | | | -0.0012 | -13.03 |
| *ROin500* | | | | | | | -0.0001 | -2.35 |
| *lnarea* | 0.6835 | 17.69 | 0.5394 | 15.82 | 0.5382 | 15.78 | 0.5349 | 15.75 |
| *yearbuilt* | 0.0009 | 12.64 | 0.0004 | 5.88 | 0.0003 | 5.76 | 0.0004 | 5.91 |
| *units* | 0.0002 | 13.16 | -0.0001 | -4.46 | -0.0001 | -5.30 | -0.0001 | -5.53 |
| *units*$^2$ | -1.4E-08 | -6.13 | 1.1E-08 | 4.07 | 1.3E-08 | 4.61 | 1.3E-08 | 4.80 |
| *rooms* | -0.0087 | -3.45 | 0.0205 | 4.57 | 0.0211 | 4.61 | 0.0214 | 4.60 |
| *bedrms* | -0.0122 | -1.74 | 0.0840 | 5.59 | 0.0837 | 5.57 | 0.0838 | 5.59 |
| *bath* | 0.2545 | 24.32 | 0.1644 | 16.41 | 0.1623 | 16.21 | 0.1615 | 16.24 |
| *mbbth* | 0.1168 | 22.70 | 0.0648 | 18.00 | 0.0630 | 17.55 | 0.0622 | 17.44 |
| *fireplace* | 0.1390 | 22.51 | 0.0425 | 9.47 | 0.0446 | 9.93 | 0.0448 | 10.06 |
| *garage* | -0.0003 | -0.05 | -0.0462 | -11.91 | -0.0437 | -11.27 | -0.0429 | -11.12 |
| *parking* | 0.1552 | 20.62 | 0.1155 | 21.33 | 0.1160 | 21.42 | 0.1134 | 20.98 |
| *parkspot* | -0.1198 | -20.63 | -0.0847 | -20.75 | -0.0847 | -20.77 | -0.0846 | -20.80 |
| *saleyear* | 0.0300 | 33.83 | 0.0414 | 64.42 | 0.0418 | 65.05 | 0.0414 | 64.41 |
| *waterfront* | | | 0.0248 | 4.58 | 0.0223 | 4.13 | 0.0184 | 3.43 |
| *dCBD* | | | -0.0364 | -7.84 | -0.0428 | -10.04 | -0.0433 | -9.83 |
| *dCBD*$^2$ | | | 0.0039 | 16.37 | 0.0043 | 22.60 | 0.0043 | 21.69 |
| *dLake* | | | 0.0052 | 1.43 | 0.0193 | 5.49 | 0.0187 | 5.29 |
| *dLake*$^2$ | | | -0.0024 | -7.34 | -0.0033 | -11.32 | -0.0033 | -11.17 |
| *dWater* | | | 0.0475 | 5.25 | 0.0444 | 4.78 | 0.0614 | 6.37 |
| *dWater*$^2$ | | | -0.0157 | -4.85 | -0.0152 | -4.60 | -0.0176 | -5.27 |
| *dCTA* | | | 0.0264 | 3.79 | 0.0180 | 2.88 | 0.0039 | 0.63 |
| *dCTA*$^2$ | | | -0.0094 | -3.92 | -0.0040 | -2.15 | -0.0018 | -1.00 |
| *dPark* | | | -0.2393 | -15.43 | -0.2675 | -17.54 | -0.2424 | -16.06 |
| *dPark*$^2$ | | | 0.2594 | 20.01 | 0.2770 | 21.56 | 0.2482 | 19.49 |
| *North* | | | 520.97 | 13.13 | 562.60 | 14.86 | 578.22 | 15.25 |
| *Lat* | | | 3.7449 | 5.12 | 4.9151 | 7.25 | 5.1326 | 7.60 |
| *North×Lat* | | | -12.436 | -13.13 | -13.429 | -14.85 | -13.802 | -15.25 |
| *lnINC* | | | 0.0799 | 13.48 | 0.0698 | 11.84 | 0.0595 | 10.12 |
| *medyrblt* | | | -0.0056 | -31.49 | -0.0053 | -28.83 | -0.0052 | -28.04 |
| *lnVAL* | | | -0.0091 | -3.41 | -0.0148 | -5.50 | -0.0104 | -3.89 |
| *white* | | | 0.1476 | 11.70 | 0.1369 | 10.93 | 0.1493 | 11.93 |
| *lnDENSITY* | | | 0.0022 | 1.14 | -0.0014 | -0.71 | 0.0007 | 0.39 |
| *college* | | | -0.3036 | -17.40 | -0.2785 | -16.17 | -0.2305 | -13.40 |
| *NewCons* | | | 0.2639 | 18.32 | 0.2554 | 17.53 | 0.2331 | 15.82 |
| constant | -55.231 | -29.78 | CA FEs | | CA FEs | | CA FEs | |
| N | 60,178 | | 58,820 | | 58,811 | | 58,811 | |
| $R^2$ | 0.6108 | | 0.7250 | | 0.7274 | | 0.7300 | |

Note: Dependent variable is the lo fog the sales price.  Reported t-statistics are based on Huber-White robust standard errors.  Columns 2-4 include fixed effects for each of Chicago's 77 "Community Areas" which correspond to the popular definitions of neighborhoods.

**Table 4: Hedonic Results with More Robust Specifications**

| | 1 | 2 | 3 | 4 | 5 | | 6 | 7 |
|---|---|---|---|---|---|---|---|---|
| | Coef. | Coef. | Coef. | Coef. | Coef. | | Coef. | Coef. |
| *DISTRICT* | -0.1369 | 0.0044 | 0.2729 | -0.1206 | 0.3582 | | -.0036 | -0.4698 |
| | -1.69 | 0.26 | 4.74 | -1.29 | 7.07 | | -1.47 | -2.78 |
| *distysd* | -- | -- | -- | -- | -0.0250 | | -- | -- |
| | | | | | -1.98 | | | |
| *DistShare* | 0.2581 | -0.2078 | 0.3611 | -0.4726 | -0.2935 | | -0.4800 | -0.6380 |
| | 5.20 | -4.78 | 2.90 | -2.39 | -1.98 | | -3.82 | -3.55 |
| *CountLmk* | 0.0487 | 0.0097 | 0.0148 | 0.0073 | -0.0209 | | -0.0185 | -0.0236 |
| | 6.63 | 0.59 | 0.91 | 0.44 | -0.81 | | -1.36 | -1.71 |
| *CounLmk2* | -0.0218 | -0.0003 | -0.0017 | 0.0003 | 0.0039 | | 0.0068 | 0.0084 |
| | -10.33 | -0.08 | -0.43 | 0.09 | 1.27 | | 2.07 | 2.51 |
| *named* | 0.0221 | 0.0233 | 0.0115 | 0.0287 | -0.0125 | | -- | -- |
| | 1.86 | 1.88 | 0.88 | 2.19 | -0.70 | | | |
| *OTHER* | 0.0874 | 0.0292 | 0.0205 | 0.0333 | 0.0445 | | -- | -- |
| | 5.38 | 2.86 | 1.92 | 3.15 | 2.79 | | | |
| *R/O* | 0.0302 | 0.0420 | 0.0314 | 0.0469 | 0.0467 | | -- | -- |
| | 2.85 | 4.84 | 3.45 | 4.78 | 3.61 | | | |
| *OTHERin100* | -0.0042 | -0.0046 | -0.0060 | -0.0039 | -0.0060 | | -- | -- |
| | -3.73 | -7.17 | -8.69 | -4.89 | -6.17 | | | |
| *OTHERin250* | -0.0001 | 0.0004 | 0.0010 | 0.0001 | -0.0006 | | -- | -- |
| | -0.52 | 1.48 | 3.40 | 0.35 | -0.93 | | | |
| *OTHERin500* | 0.0005 | -0.0011 | -0.0009 | -0.0012 | -0.0006 | | -- | -- |
| | 5.11 | -6.39 | -5.20 | -6.42 | -3.40 | | | |
| *ROin100* | 0.0013 | 0.0014 | 0.0016 | 0.0013 | 0.0017 | | -- | -- |
| | 2.66 | 4.75 | 5.22 | 4.51 | 4.28 | | | |
| *ROin250* | -0.0005 | -0.0015 | -0.0015 | -0.0016 | -0.0013 | | -- | -- |
| | -1.96 | -14.82 | -14.95 | -14.52 | -9.61 | | | |
| *ROin500* | -0.0001 | 0.0003 | 0.0003 | 0.0003 | 0.0002 | | -- | -- |
| | -3.20 | 4.30 | 4.62 | 4.10 | 3.15 | | | |
| **1st-stage** | **DISTRICT** | | **DISTRICT** | **DISTRICT** | **DISTRICT** | **distysd** | | **DISTRICT** |
| *CHRSin100* × | 0.0087 | -- | 0.0120 | -- | 0.041 | 0.120 | -- | -- |
| *OwnOcc70* | 7.80 | | 9.22 | | 22.13 | 7.36 | | |
| *CHRS* × | -0.0103 | -- | 0.0721 | -- | -0.237 | -2.438 | -- | -- |
| *OwnOcc70* | -0.61 | | 4.92 | | -6.52 | -10.88 | | |
| *ROin100* × | -- | -- | -- | 0.0041 | -0.0414 | -0.172 | -- | -- |
| *OwnOcc70* | | | | 5.58 | -21.45 | -8.88 | | |
| *R/O* × | -- | -- | -- | 0.1671 | 0.2966 | 2.0655 | -- | -- |
| *OwnOcc70* | | | | 7.14 | 6.89 | 8.84 | | |
| *ROdColYr* | -- | -- | -- | -- | -- | -- | -- | 0.0207 |
| | | | | | | | | 6.40 |
| *ROdIncYr* | -- | -- | -- | -- | -- | -- | -- | 0.0099 |
| | | | | | | | | 6.66 |
| Hansen *J* | 1.719 | -- | 8.257 | 2.657 | 3.906 | | -- | 0.285 |
| p-val | 0.1898 | -- | 0.0041 | 0.1031 | 0.1419 | | -- | 0.5934 |
| K-P Wald *F* | 31.913 | -- | 93.838 | 53.334 | 46.984 | | -- | 22.316 |
| C-stat | -- | 28.643 | -- | -- | -- | | 7.094 | -- |
| p-val | | 0.0000 | | | | | 0.0077 | |
| Fixed Effects | CA | BG | BG | BG | BG | | Building | Building |
| N | 58811 | 60169 | 60169 | 60169 | 57610 | | 56623 | 56623 |
| centered $R^2$ | 0.7253 | 0.7057 | 0.7031 | 0.7052 | 0.7033 | | 0.6078 | 0.6043 |

Note: The log of the unit sales price is the dependent variable. Regressions in columns 1-5 contain unreported controls for the variables in column 4 of Table 3. Columns 5 and 6 contain unreported controls for the unit-specific variables from column 1 of Table 3. *Z* or *t* statistics printed under the coefficients are based on Huber-White robust standard errors. The second panel of the table reports the coefficients of the excluded instruments from the first stage equations, when they exist. K-P Wald F is the Kleinbergen-Paap rk Wald F statistic. The C-stat is the difference-in-Sargan C-statistic. Fixed effects are included for Chicago Community Areas (CA), block groups (BG) or buildings, as indicated.