Appendix

Since 421-a is theorized to have a positive, non-zero price response, our goal is to measure its size on average. The price premium, Δp, is the difference between what a 421-a condo sells for and what it would sell for without 421-a. Expressing the premium relative to the present value of the tax savings (S_{pv}) yields a measure of the degree of capitalized tax benefit. When $\frac{\Delta \mathrm{p}}{\mathrm{S_{pv}}}=1$, the 421-a benefit is fully capitalized, and when $0<\frac{\Delta \mathrm{p}}{\mathrm{S_{pv}}}<1$, it is partially capitalized.

The average price premium ($\overline{\Delta \mathrm{p}}$) is calculated as:

$$\overline{\Delta p} \;=\; (\bar{p})(\bar{b})(\overline{\Delta y}) \qquad (1)$$

where $\bar{p}$ is the average sales price, $\bar{b}$ is the average number of 421-a benefit years remaining at the time of sale, and $\overline{\Delta \mathrm{y}}$ is the average annual market value of one more year of 421-a benefits. While $\bar{p}$ and $\bar{b}$ are observable from the data, $\overline{\Delta y}$ must be inferred from the behavior of condo-market participants. Though all apartments in a condo building receive the tax benefit contemporaneously, different apartments sell at different points in time, creating variation in how much 421-a benefit remains when apartments return to the market. We take advantage of this variation to estimate the exemption's effect on its sales price.

However, the decision to purchase a condo is motivated by reasons other than the presence of a tax break. Bid prices may reflect buyers' preferences for living in a particular neighborhood, having access to higher quality schools, having greater access to subway stations, being geographically closer to parks, and so on. Condo apartments and condo buildings will also vary in terms of amenities (i.e. desirable views, doormen, square footage). All of these other types of benefits, if buyers are decisive about them, influence asking prices to some degree. The statistical challenge is to distinguish the price response due to the 421-a tax break from the price responses due to differences in building and neighborhood amenities.

Our strategy for isolating the tax effect begins by treating sales prices as a function of both years of 421-a remaining and housing quality:

$$lnp_{it} = f(y_{it}, q_i, q_{it}, \boldsymbol{Z}_i, \boldsymbol{Z}_{it}) \qquad (2)$$

where lnp_{it} is the log sales prices of condo *i* in fiscal year *t*; y_{it} measures the number of tax benefit years remaining as of the year of sale; q_i is a time-invariant apartment-specific quality effect; q_{it} is a time-varying apartment-specific quality effect; and $\boldsymbol{Z}_i$ and $\boldsymbol{Z}_{it}$ are vectors of time-invariant and time-varying neighborhood quality variables, respectively.

By distinguishing time-varying and time-invariant effects, we allow different sources of quality to influence prices differentially over time. In such instances it is useful to think of housing quality as having long-run and short-run dimensions. All quality in the long-run is variable, but in the short-run some quality is more variable than others. Indeed, over a short enough period long-lived amenities are essentially fixed. Short of a new building becoming an obstruction, for example, a view of Manhattan's skyline is fixed. Likewise, the number of bedrooms and bathrooms are common proxies for apartment quality that are essentially fixed over time. Neighborhood amenities also degrade slowly over time if at all—distance to the nearest subway entrance or park, for example.

Unfortunately, the city does not collect data on apartment amenities nor does it collect data on variables that are often used to proxy apartment quality such as the number of bedrooms and bathrooms. The only apartment-level data known is square footage, but this is an imprecise proxy for quality because each square foot does not contribute the same amount to construction and maintenance costs, and hence sales prices. Because of plumbing, cabinets, and appliances, for instance, the cost per square foot to build and maintain a bathroom is greater than the cost to build a bedroom. Without better apartment-level data, a standard hedonic model would likely suffer omitted variable bias due to unobserved differences in housing quality.

Instead, we isolate tax effects from quality effects using a repeat-sales approach, in which case all observed and unobserved time-invariant quality is removed by measuring changes in prices for condos that have resold multiple times. Repeat-sales models have been criticized for introducing some sample selection bias because homes that sell multiple times may not be representative of all homes, and for discarding too much data. [1,2] Hybrid models combining elements of hedonic and repeat-sales models have been developed, but the selection of a repeat-sales approach in this study is motivated by data constraints as noted in the previous paragraph, not by theoretical or estimation issues. Notwithstanding these criticisms, the repeat-sales method is preferred to hedonic methods because the former avoids the specification bias rampant in the latter. [3,4] Indeed, empirical evidence indicates sample selection bias in repeat-sales price indices is dwarfed by the specification bias in hedonic price indices, reinforcing a

common view that repeat-sales is the preferred strategy.[5,6] With respect to Equation 2, the repeat-sales approach results in q_i and $\boldsymbol{Z}_i$ dropping from the equation.

The repeat sales model must further be augmented to account for other tax programs available to condos. Buildings not already receiving 421-a are eligible to receive a J-51 exemption for residential rehabilitation projects, or a 421-g exemption for commercial to residential conversions in lower Manhattan.[7] We add these programs to the model.[8]

In addition, these three programs share the common feature that they phase out over time. During the final years of the benefit periods, property owners receive a dwindling fraction of a full exemption. Thus, not every benefit year is made equal. To account for this, we measure benefit years in full-value terms. For example, the first 2 years of a 10-year 421-a exemption provide a 100 percent exemption of post-construction taxable value. Every two years thereafter, the exemption percentage declines by 20 percentage points. Rather than measuring the exemption as 10 years, which values each year equally, we measure the exemption as 6 years, the sum of the exemption percentage over the benefit period. Thus, the measure tells us how many 100 percent exemption years it would take to equal the value of the 10 exemption years with a phaseout.

Furthermore, we proxy for neighborhood quality with control variables measuring changes in median math proficiency score of elementary schools in the district ($\Delta m_i{}^*$) and number of felony crimes occurring in the condo's police precinct ($\Delta c_i{}^*$). Using median math proficiency scores within school districts is less desirable than within school zones, the former often being much larger than the latter, but it is a necessary tradeoff because we do not have school zone data for the entire observation period. School districts thus may be too large to detect the signal of school quality in condo sales prices. Meanwhile, crime is measured at the precinct level and is measured according to the number of felonies committed per 10,000 people.[9]

The repeat-sales model thus becomes:

$$\Delta lnp_i{}^* = f(|\Delta y_i{}^*|, |\Delta j_i{}^*|, |\Delta g_i{}^*|, \Delta m_i{}^*, \Delta c_i{}^*) \quad (3)$$

where Δ takes the traditional interpretation of a change from t to the last fiscal year of sale, $t - n$ such that $\Delta X^* = X_t - X_{t-n}$. Because Δy^*, Δj^*, and Δg^* are negative, we take their absolute value in order to give the coefficients a more intuitive interpretation: as the number of benefit years that pass between

sales increases, the sales price is expected to fall, all else equal. The tradeoff of the repeat-sales approach is that we cannot estimate the effect on prices of building-level and apartment-level quality that do not change over time.

We further allow changes in prices to vary across the city and over time by giving each of the 127 neighborhoods (k) their own year dummy variable (N_{kt}). Neighborhoods are defined by the Department of City Planning. Neighborhoods are the lowest level of geography for which we would still have sufficient degrees of freedom for estimation; if this strategy were duplicated at the building-level, it would add 20,997 variables. In contrast, neighborhood location and year controls result in 1,134 variables. Moreover, there are considerably fewer repeat sales occurring within buildings and within tax blocks (one in many cases) whereas within neighborhoods there are sufficient repeat sales to reliably estimate variations in sales price changes across the city.

Estimating $k-1$ additional parameters is computationally intensive, however. To overcome this, we demean the neighborhood effects before estimating the parameters so that for each variable Z, $\tilde{Z} = Z - \frac{\sum Z}{n_k}$. Thus, the neighborhood-year effects drop from Equation 3 because $\widetilde{N}_{kt} = 0$. Though this process reduces computation time, it sacrifices estimating N_{kt}, which is of no policy significance for the present purpose.

The equation to be estimated is thus:

$$\Delta ln\widetilde{p_\iota}^* = \alpha_i + \beta_1|\Delta\widetilde{y_\iota}^*| + \beta_2|\Delta\widetilde{j_\iota}^*| + \beta_3|\Delta\widetilde{g_\iota}^*| + \beta_4\Delta\widetilde{m_\iota}^* + \beta_5\Delta\widetilde{c_\iota}^* + \varepsilon_i \qquad (4)$$

Importantly, Equation 4 does not account for the well-known problem that repeat sales regressions are heteroskedastic.[10] Homes with longer periods between sales are more likely to experience price changes due to nonmarket or unobserved factors than homes with shorter periods between sales. The variance of the conditional mean thus varies over time, implying that the ordinary least squares estimator is no longer the best linear unbiased estimator. To resolve this issue the repeat sales regressions were estimated with time between sales serving as a weight. In effect, weighted least squares (WLS) relaxes the assumption that properties during the observation period did not undergo physical changes. Alterations that affect market prices need to be isolated from the price effect of the tax exemption. Using ordinary least squares forces q_{it} to equal zero in order to give the coefficient the desired interpretation as the conditional mean effect of one more 421-a benefit year. This may be too strict an

assumption. Since the city does not track physical changes that do not need a building permit (such as a kitchen remodel), WLS is a more sound alternative to ordinary least squares.

IBO collected 17,717 repeat condo sales among 101,477 condo sales occurring from fiscal year 2005 through 2015 and estimated the 421-a tax effects via WLS. The parameter estimates are displayed in the table below. Two versions of Equation 4 were estimated: one covering Manhattan transactions and one limited to transactions occurring in the other boroughs. There were insufficient observations to disaggregate the boroughs outside Manhattan any further. The direction of the key variable of interest ($|\Delta\widetilde{y_i}^*|$) is in the expected negative direction, and is statistically significant at the 99 percent level. In Manhattan, we estimate that each additional 100 percent equivalent year of 421-a lost between sales decreases the sales price by 0.46 percent while our estimate of the decline in prices in the other boroughs is 0.40 percent and is significant at the 90 percent level.

Each Additional Year of 421-a Benefit Lost Lowers a Condo's Sales Price by 0.46 Percent on Average in Manhattan and by 0.40 Percent Elsewhere

Variable	**Manhattan**	**All Other Boroughs**
Consumed 421-a Benefit Years Between Sales	-.0043**	-.0040*
	(.0007)	(.0013)
Consumed 421-g Benefit Years Between Sales	.0006	
	(.0017)	
Consumed J-51 Benefit Years Between Sales	.0031	-.0005
	(.0028)	(.0015)
Change in School District Math Proficiency	.0021**	.0017**
	(.0002)	(.0002)
Change in Felony Crime in Precinct (per 100,000 people)	-.0001**	.0017**
	(.0001)	(.0002)
N	12,333	5,384
R2	.256	.510

NOTES: **p < .010 * p < .100
"All Other Boroughs" includes the Bronx, Brooklyn, Queens, and Staten Island. The dependent variable is the change in log sales price. The estimator is weighted least squares weighted by number of years between sales.

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Most of the coefficients for 421-g and J-51 are not statistically significant at high levels, indicating that these tax breaks are not capitalized into sales prices. While contrary to expectations, there are substantially fewer 421-g and J-51 repeat sales. The null effects could reflect too few data points to observe a meaningful relationship rather than the market actually being indifferent to the development

incentives. The remaining control variables, school quality and crime, are generally in the direction hypothesized. School district quality as measured by median elementary school proficiency rates is positively associated with condo prices. Condos in police precincts with higher incidence of felony crime, however, are only negatively associated with prices in Manhattan. In the other boroughs the effect is positive. While a positive relationship between crime and home prices is unexpected, some research indicates that properties can appreciate faster in areas with higher crime during periods of economic expansion, which applies to core areas of Brooklyn and Queens.[11]

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[2] Mark, J. H., & Goldberg, M. A. (1984). "Alternative housing price indices: An evaluation." *Real Estate Economics*, *12* (1), 30-49.
[3] Case, B., & Quigley, J. M. (1991). "The dynamics of real estate prices." *The Review of Economics and Statistics*, *73* (1), 50-58; Meese, R. A. & Wallace, N. E. (1997). "The construction of residential housing price indices: A comparison of repeat-sales, hedonic regression, and hybrid approaches." *The Journal of Real Estate Finance and Economics*, *14* (1-2), 51-73.
[4] Shiller, R. J. (2009). "Derivatives markets for home prices." In Edward L. Glaeser and John M. Quigley (eds.), *Housing Markets and the Economy: Risk, Regulation, and Policy*, 17-33. Cambridge: Lincoln Institute of Land Policy.
[5] Liang, J., Phillips, P. C. B., & Yu, J. (2015). "New methodology for constructing real estate price indices applied to the Singapore residential market." *Journal of Banking & Finance*, *61* (S2), S121-S123.
[6] Shiller, 2009; Van Order, R. (2009). "Commentary: Derivatives markets for home prices," in Edward L. Glaeser and John M. Quigley (eds.), *Housing Markets and the Economy: Risk, Regulation, and Policy*, 37-38. Cambridge: Lincoln Institute of Land Policy.
[7] The 421-g program began in 1995 in order to encourage the conversion of commercial office space in Lower Manhattan to residential use. The program provided a 12-year exemption (13 years if the property was landmarked) with the final 4 years being a phase out period. Conversions were also eligible for a contemporaneous abatement equal to the preconversion tax liability for 14 years. The state Legislature allowed the program to expire after 2007, but existing beneficiaries continue to receive the tax breaks through the scheduled benefit period.
[8] Non-421-a condos are eligible for the condo-coop abatement. In fact, through 2013, all non-421-a condos received the abatement. Therefore, the benefit only varies across non-421-a condos for two years during the dataset. Excluding the 166 non-421-a condos whose abatement status changed during the observation period did not affect the point estimates to a noticeable degree.
[9] There are seven types of felonies: burglary, felony assault, grand larceny, grand larceny of a motor vehicle, murder, rape, and robbery.
[10] Case, K. E., & Shiller, R. J. (1987). "Prices of single-family homes since 1970: New indexes for four cities." *New England Economic Review*, *87*, 45-56.
[11] Case, K. E., & Mayer, C. J. (1996). "Housing price dynamics within a metropolitan area." *Regional Science and Urban Economics*, *26* (3), 387-407.