

Unintended or intended consequences? The effect of below-market housing mandates on housing markets in California*

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ABSTRACT

Inclusionary zoning, also known as below-market housing mandates, is now in place in one-third of California cities and is spreading around the United States. Supporters of this policy advocate making housing more affordable by placing price controls on a percentage of new homes. But if almost all economists agree that price controls on housing reduce quantity and cause shortages, why do so many policymakers or voters support them? Ellickson (1981) argued that inclusionary zoning may be popular precisely because, contrary to the expressed goals of the program, it actually restricts supply and leads to higher prices. Incumbent homeowners and policymakers catering to them can benefit from restricting new supply. Using panel data and a first difference model, we test how the policy affected the price and quantity of housing in California cities between 1980, 1990, and 2000. Under various specifications we find that cities adopting below-market housing mandates end up with higher prices and fewer homes. Between 1980 and 1990, cities imposing below-market housing mandates end up with 9 percent higher prices and 8 percent fewer homes overall. Between 1990 and 2000 cities imposing below-market housing mandates end up with 20 percent higher prices and 7 percent fewer homes overall. Consistent with Ellickson's hypothesis, the program may not be about increasing the supply of housing or making it more affordable overall.

JEL Classification: C23, R31, R53

Keywords: Inclusionary zoning, affordable housing, price controls

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1. Introduction

Economists have long documented the negative consequences of price controls on housing, yet price controls on housing continue to resurface with new benign sounding names such as inclusionary zoning, affordable housing, or below-market housing mandates. Pioneered in Fairfax in the early 1970s, below-market housing mandates are now under consideration or are being adopted in many of the largest cities in the United States, including New York, Los Angeles, and Chicago (*The New York Times* Editorial Desk, 2007; Helfand and Hyman, 2007; Hinz, 2007, Buckley, 2010).¹ In California mandates typically require developers to sell 10 to 20 percent of new homes at prices affordable to low income households. Below-market units typically must be interspersed among market rate units, have similar size and appearance as market price units, and retain price controls for period of at least 55 years. While participation in some city's inclusionary zoning ordinances is voluntary and other ordinances allow developers to bypass selected zoning laws, most ordinances are mandatory price controls without significant compensation.²

Because the majority of economics agree that price controls do not end up benefiting consumers, why would anyone support such a policy? Advocates typically defend the policy without any economic theory or evidence. One explanation is that advocates fail to understand the economics of these programs, but a potential public choice informed explanation is that they support the programs because they actually favor their

¹ The United States Secretary for Housing and Urban Development holds them up as one of the ways “to make housing affordable for every American” (U.S. Department of Housing and Urban Development, 2009; Calmes, 2008, p.A11; Simon and Hersh, 2010).

² For details about the program, see California Coalition for Rural Housing and Non-Profit Housing Association of Northern California (2003) and Powell and Stringham (2004a).

“unintended” consequences. Robert Ellickson (1981) was the first law and economics scholar to analyze inclusionary zoning and came up with the following hypothesis. Because inclusionary zoning imposes price controls on new construction, it restricts supply in one part of the market but does not for existing owners. With new supply restricted prices of existing homes can rise, so we might expect support from existing homeowners and policymakers catering to them. The main group harmed is potential residents who would have moved into the now restricted supply, but non-residents have less say in political or zoning processes. Thus we have a public choice explanation for the popularity of inclusionary zoning. Inclusionary zoning might be a form of exclusionary zoning after all.

Although below-market housing mandates have been growing in popularity, they have not been subject to rigorous statistical analysis. In California the building industry attempted a legal challenge against the program, but the California Courts of Appeal ruled against the Building Industry Association, stating that affordable housing mandates are legal because they actually (1) benefit developers and (2) “necessarily increase the supply of affordable housing” (*Home Builders Association of Northern California v. City of Napa*, 2001, pp.195-6). The certainty of these claims notwithstanding, a report by the California Coalition for Rural Housing and Non-Profit Housing Association of Northern California (2003, p.3) admitted, “These debates, though fierce, remain largely theoretical due to the lack of empirical research.”

This article is the first to use panel data to investigate how below-market housing mandates actually affect housing markets. To what extent do below-market housing mandates affect the price and quantity of housing in California cities? Are the policies serving their stated goal, or are they actually restricting supply and leading to higher prices

Ellickon predicted? After providing a brief literature review and discussing some of the specifics of the programs in California, we investigate econometrically, using a first difference model, whether below-market housing mandates truly make housing more affordable overall. We find that between 1980 and 1990, cities that imposed below-market housing mandates actually drove up housing prices by 9 percent and ended up with 8 percent fewer homes. Furthermore, we find that between 1990 and 2000, cities that imposed below-market housing mandates drove up housing prices by 20 percent and ended up with 7 percent fewer homes. These statistically significant findings indicate that below market housing mandates have the unintended (or potentially intended) consequences of restricting the supply of housing and making the overall housing market less affordable.

2. Literature Review and Description of Below-Market Housing Mandates in California

The majority of the literature on inclusionary zoning appears in urban studies journals and law reviews, or as advocacy pamphlets (Calavita, Grimes, and Mallach, 1997; Calavita and Grimes, 1998; Padilla, 1995; Dietderich, 1996; Kautz, 2002; Higgins, 2003; California Coalition for Rural Housing and Non-Profit Housing Association of Northern California, 2003). Much of it focuses on specific examples of completed projects and stories about the residents who moved in. Although the residents of the price controlled units can benefit, very little attention has been paid to the economic costs or the efficacy of the programs overall.

Inclusionary zoning as practiced in California has some similarities to, but some important differences from, what is also called inclusionary zoning in other states. One might assume that inclusionary zoning is defined as the opposite of exclusionary zoning.³ In principle, advocates of inclusionary zoning could simply advocate eliminating exclusionary zoning laws as a way of increasing the supply of housing and making housing more affordable.⁴ In practice, most California inclusionary zoning ordinances are simply price controls with minimal changes or relaxation in other zoning laws (Powell and Stringham, 2005).⁵

Powell and Stringham (2004a and 2004b) investigated the program details for each of the cities with inclusionary zoning in the San Francisco Bay Area and Los Angeles and

³ Exclusionary zoning, or “snob zoning,” is a set of policies that (often intentionally) increases housing prices through laws such as minimum lot size, height restrictions, density limits, or any other policy that precludes more affordable housing from being built (Fischel, 1985).

⁴ In certain states other than California the policy is associated with waivers for certain zoning rules. For example, in the Mount Laurel Decision the New Jersey court ruled that municipal governments are not allowed to use zoning laws to prevent developers from building housing for lower income households (Fischel, 1985, pp.319-323). The ruling enabled builders to proceed with otherwise restricted developments if they dedicated a percentage (typically 20 percent) of the units to be sold at below market prices. Similarly, Massachusetts General Laws Chapter 40B (the “Anti-Snob Zoning Law”) limits the use of exclusionary zoning by municipalities (Siskind, 2006, p.176). According to the law, developers in certain areas can bypass many zoning restrictions if they promise to sell 20 percent of their housing at restricted prices. Thus, although these laws include price controls, they are more complex policies because they also relax many other zoning rules.

In California the policies are associated with few waivers of zoning rules. In California, only a minority of towns offer zoning waivers, such as growth control exemptions (California Coalition for Rural Housing and Non-Profit Housing Association of Northern California, 2003, p.17). California Government Code Section 65915 does require municipalities to offer a density bonus of at least 25 percent to developers who make 20 percent of a project affordable to low income households, but conversations with builders and government officials revealed that density bonuses are often completely unusable because density restrictions are just one of a set of rules regarding how many units will fit on a particular property. Other mandatory guidelines such as setbacks, minimum requirements for public and private open space, floor area ratios, and even tree protections can make obtaining approval to build the maximum number of units on a property impossible.

⁵ Just as inclusionary zoning could be interpreted to mean: (1) “ending exclusionary zoning laws” or (2) “developers must sell a percentage of new units at price controlled rates,” an affordable housing mandate could be interpreted to mean: (1) “we have a mandate preventing municipalities from prohibiting affordable housing” or (2) “we mandate that developers sell a percentage of their homes at rates that government deems affordable.” In this paper we refer to inclusionary zoning and affordable housing mandates using the second, more common, meaning of the term.

Orange counties to estimate the magnitudes of the price controls. By calculating the difference between the price controls and market rates, one can estimate how much below-market housing mandates make sellers forego on those units. Figure 1 shows that in the median Bay Area city with a below-market housing mandate in 2004, each price controlled unit must be sold for more than \$300,000 below market price. In cities with high housing prices and restrictive price controls, such as Los Altos and Portola Valley, developers must sell below-market rate homes for more than \$1 million below the market price. The prices in Figure 1 also represent the amount of the effective subsidy from the developers to the buyers of the price controlled units.

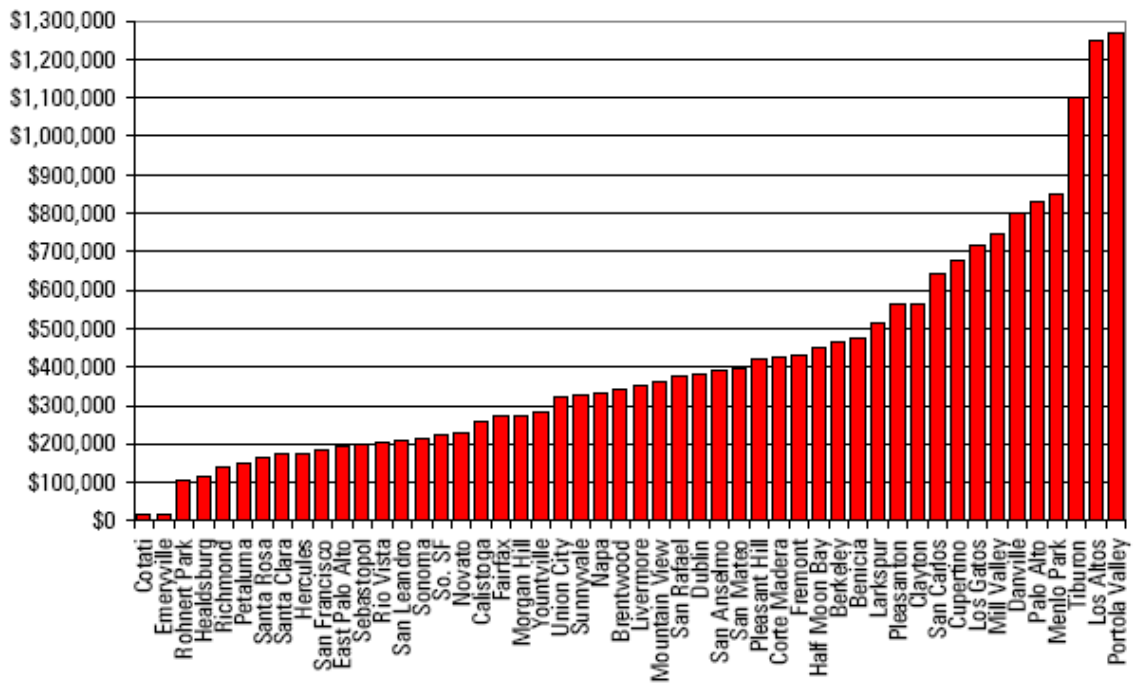


Figure 1: Average lost revenue associated with selling each below-market-rate unit in San Francisco Bay Area cities *Source: Powell and Stringham (2004a, p.15)*

The economics of inclusionary zoning is similar to the economics of rent control, but it is slightly more complicated because inclusionary zoning applies to both for sale and rental units, and to some units but not all. Because developers must sell the price-controlled units as a condition for selling the non-price-controlled units, the burden of the price-controlled units may be viewed as a tax that developers must pay to sell the non-price controlled units. For example, in Mill Valley one out of ten units must be sold for \$750,000 less than market price, so that effective tax will be spread over the remaining nine units, resulting in an equivalent tax of \$83,333 per non-price controlled unit.⁶ Figure 2 displays the levels of the tax on non-price controlled housing in the San Francisco Bay Area. In the median city with a below-market housing mandate, the effective tax per non-price controlled home is more than \$40,000, and in cities such as Berkeley and Palo Alto, that tax is well more than \$100,000. Unless Palo Alto writes a check for \$100,000 or provides an equivalent amount of other benefits to developers for each market rate home, the policy will act as a tax on new housing.⁷

6 The tax per non-price controlled unit, t , is calculated using the formula $[(P_m - P_{pc})(r)]/(1-r)=t$ where P_m is the market price, P_{pc} is the price controlled price, and r is the required percentage of units under price controls. For Mill Valley, $[(\$750,000)(0.10)]/(0.90) = \$83,333$ tax per non-price controlled unit.

7 Kautz (2002, p.1019) argues that inclusionary zoning has significant offsetting benefits that outweigh the costs, and so it should not be considered a tax on housing. Powell and Stringham (2005) address this argument in more depth, but one can determine whether the offsetting benefits outweigh the costs in at least two ways. The first would be if one observed the building industry actively lobbying for these programs. But in California the building industry is usually the most vocal opponent of these programs (*Home Builders Association of Northern California v. City of Napa*, 2001). The second and even simpler way to determine whether affordable housing mandates provide significant benefits to compensate for their costs would be to make inclusionary zoning programs voluntary. Developers could weigh the benefits and costs of participating, and if the benefits exceeded the costs, they could voluntarily comply. A few municipalities in California tried adopting voluntary ordinances, and perhaps unsurprisingly, the programs did not attract developers. One advocate of affordable housing mandates argues that the problem with voluntary programs is “that most of them, because of their voluntary nature, produce very few units” (Tetreault, 2000, p.20). From these simple observations, we can infer that the “benefits” of these programs to developers are not as significant as the costs.

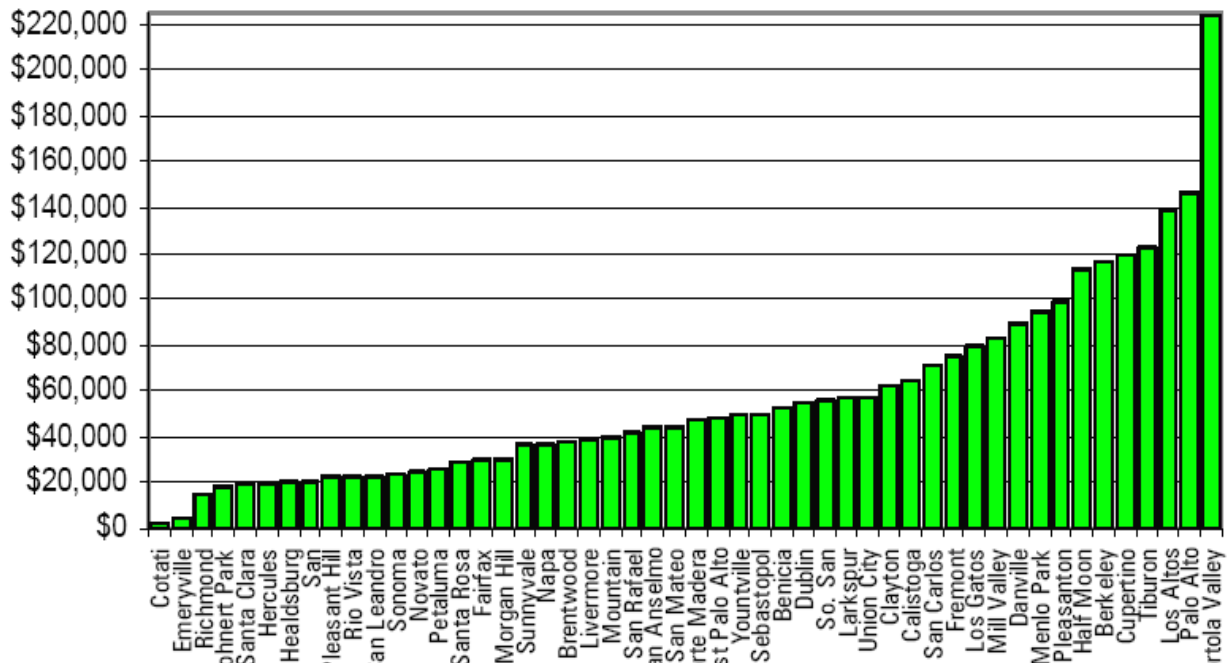


Figure 2: Effective tax imposed on non-price controlled units by below-market housing mandates in San Francisco Bay Area cities *Source:* Powell and Stringham (2004a, p.17)

Without offsetting benefits, below market housing mandates are basically price controls on a portion of the units and a tax on the remainder of the units. Relative elasticities will determine exactly who bears the burden of the tax, but some combination of landowners, builders, and other new homebuyers must bear it. Glaeser, Gyourko, and Saks (2005b, p.337) explain why builders are the least likely to bear the incidence of effective taxes from zoning. The building industry is highly competitive (nationwide 138,850 establishments construct single family homes, and more than 1,700 of those have more than \$10 million in revenue per year), so attempts to impose costs on builders will lead them to take their business elsewhere unless they can pass those costs on to landowners or homebuyers.

Many people assume that landowners alone will bear the costs of below-market housing mandates,⁸ because the supply of land is perfectly inelastic, but this is unlikely to be the case. Although the supply of land may be fixed (perfectly inelastic), the amount that landowners devote to new housing is not. Landowners can choose to devote their land to agricultural, commercial, industrial or residential uses, or to keep the land unused until a later date. Even with zoning laws restricting the options (thereby making the supply of housing less elastic than it otherwise would be⁹), since most land in California starts with a preexisting use, landowners will only change that use to new residential housing if it pays to do so.¹⁰ On the demand side of the market, to the extent that consumers are willing to make tradeoffs but still value city-specific amenities, the demand in any particular city will be downward sloping rather than perfectly inelastic or perfectly elastic.¹¹ For example, many people really want to live in San Francisco or really want to live in Palo Alto and because no exact substitutes for those cities exist one can observe people willing to pay a price premium to be in a city. In any city with an upward sloping supply and a downward

8 Among others, Calavita and Grimes (1998, p.152) make this argument, which is essentially that landowners must accept lower offers since the land supply is perfectly inelastic. What proponents fail to see is that this “tax” is not levied on all landowners but only on landowners that decide to develop new housing. For an in-depth discussion of the incidence of below market housing mandates, see Powell and Stringham (2005).

9 Chapman (1998) discusses how the state and local public finance structure in California create a bias against municipalities providing permits for developers to build housing, since local jurisdictions see housing as more costly and a source of less revenue than other forms of development, such as commercial. This is called the fiscalization of land use.

10 For this reason, the supply curve of land for housing is upward sloping rather than vertical. This bears out in the data of Green, Malpezzi, and Mayo (2005), who find an upward sloping supply curve for housing in all of the cities they study.

11 If housing demand is highly *inelastic*, a tax may have little effect on quantity, but it will lead to an increase in price. If housing demand is highly *elastic*, a tax may have little effect on price, but it will lead to a decrease in quantity. The relative magnitudes of each will depend on the specific elasticities per city, but increases in the effective taxes associated with below-market housing mandates will affect price or quantity or both. For a theoretical and empirical discussion of the elasticity of demand for housing, see Hanushek and Quigley (1980).

sloping demand for housing, an effective tax will lead to an increase in prices and a decrease in the quantity of housing.

Following this logic, we would predict cities with below-market housing mandates to have less housing construction and higher prices for all non-price controlled units. The mandate may increase the supply of price controlled units, but we predict the overall supply to grow slower than cities without the policy.¹² To estimate the impact of the policy we investigate this question using a first-difference panel model of California cities.

3. Description of the Data

We constructed our panel data for California cities from a few sources. First we collected U.S. Census data for 1980, 1990, and 2000 from the Census website and the Minnesota Population Center's National Historical Geographic Information System. Census data includes data on housing and community characteristics that we use as control variables. We restrict our analysis to towns or cities with populations of more than 10,000. Data on affordable housing mandate adoption dates came from the California Coalition for Rural Housing and Non-Profit Housing Association of Northern California. We also collected average home sale prices for each city from the RAND California Statistics website. The RAND data does not report 1990 home sale prices for all cities, resulting in fewer

12 The California Court of Appeals' statement below market housing mandates will "necessarily increase the supply of affordable housing" can be shown to be incorrect whether one interprets "the supply of affordable housing" as number of price controlled units or the degree of housing affordability. First, just because a city adopts a below-market housing mandate does not guarantee that any below-market units will actually be built. Powell and Stringham (2004) document that seeing no price controlled units built is quite common. Second, policy that restricts supply push prices upward and lead to less affordability overall. A few households may benefit if they are able to purchase the price controlled units but it can come at the expense of overall housing affordability. An advocate of affordable housing should answer the question whether they believe a narrowly distributed wealth transfer outweighs the negative impacts of raising prices for everyone else.

observations using our price model, but not for the output model. As far as other potential control variables, statewide panel data indicating changes in other regulations is extremely limited, but we include measures of regulations from two sources. Data from Landis et al. (2000) enables us to create a measure of how regulations change over time, but this data is limited to the San Francisco Bay Area.¹³ Quigley and Raphael (2005) use a statewide survey conducted by Glickfield and Levine (1992) to create an index of regulation. This index provides a measure of regulation at the time of the survey, but it does not indicate how regulations change over time.¹⁴ Due to these limitations, we include these measures of regulation in some regressions but not all. Table 1 provides summary statistics for the major variables we utilize.

For our below-market housing mandate policy variable, we create an indicator variable, I_{Zyr} , which is defined to equal one if the city passed a below-market-rate housing ordinance in that year or in a prior year. In 1989, 23 California cities in our sample had an ordinance in place; by 1999 that number had increased to 59. The indicator variable for those cities switches from a zero to a one; thus the difference between I_{Z1989} and I_{Z1999} is 36 additional cities passing an ordinance. We will be using this Below-Market Housing Mandate Variable in our first difference (FD) model to investigate how changes in the policy variable affect price and quantity. Figure 3 indicates the number of California jurisdictions with below-market housing mandates over time. The difference variables are fairly constant and capture a large number of cities that passed ordinances during the decade.

13 Section 5 provides details of this index.

14 Dalton and Zabel (2008) provide an excellent summary of the housing regulation data, and they point out that most data on land use regulation is cross-sectional.

Table 1: Summary Statistics

<u>Variable</u>	<u>Observation</u>	<u>Mean</u>	<u>Standard</u> <u>Deviation</u>	<u>Minimu</u> <u>m</u>	<u>Maximu</u> <u>m</u>
Population (2000-1990 difference)	N=431	8,480	(16,536)	-28,688	209,436
Density: persons/acre (2000- 1990 difference)	N=431	0.817	(1.104)	-6.196	5.694
Median Household Income (2000-1990 difference)	N=431	13,851	(8,692)	-1,255	69,532
Proportion College Degree (2000- 1990 (2000-1990 difference))	N=418	0.103	(0.071)	-0.012	0.495
Proportion Age > 65 (2000-1990 (2000-1990 difference))	N=418	0.005	(0.022)	-0.120	0.081
Proportion Below Poverty Level (2000-1990 (2000-1990 difference))	N=418	0.019	(0.030)	-0.082	0.203
Affordability Index (1990) (Annual Rent/Median HH Income)	N=431	0.190	(0.036)	0.097	0.524
Log(Units2000/Units1990)	N=431	0.014	(0.279)	-0.753	3.219
Log(Mean Home Price 2000/Mean Home Price 1990)	N=348	0.272	(0.276)	-0.697	1.661
Mean Travel Time to Work (2000- 1990 (2000-1990 difference))	N=418	3.031	(2.602)	-3.656	26.727
Below-Market Housing Mandate Policy (2000-1990 difference)	N = 446	0.081	(0.273)	0	1
Below-Market Housing Mandate Policy (2000-1990 difference)	N=446	0.040	(0.197)	0	1
Number of Infrastructure Requirements	N=311	2.789	(3.361)	0	11
Number of Service Restrictions Required	N=311	0.646	(1.088)	0	6
Number of Development Impact Fees	N=311	5.370	(2.724)	0	12
Change in Regulations (HCD Survey Cities only)	N=446	0.078	(0.269)	0	1

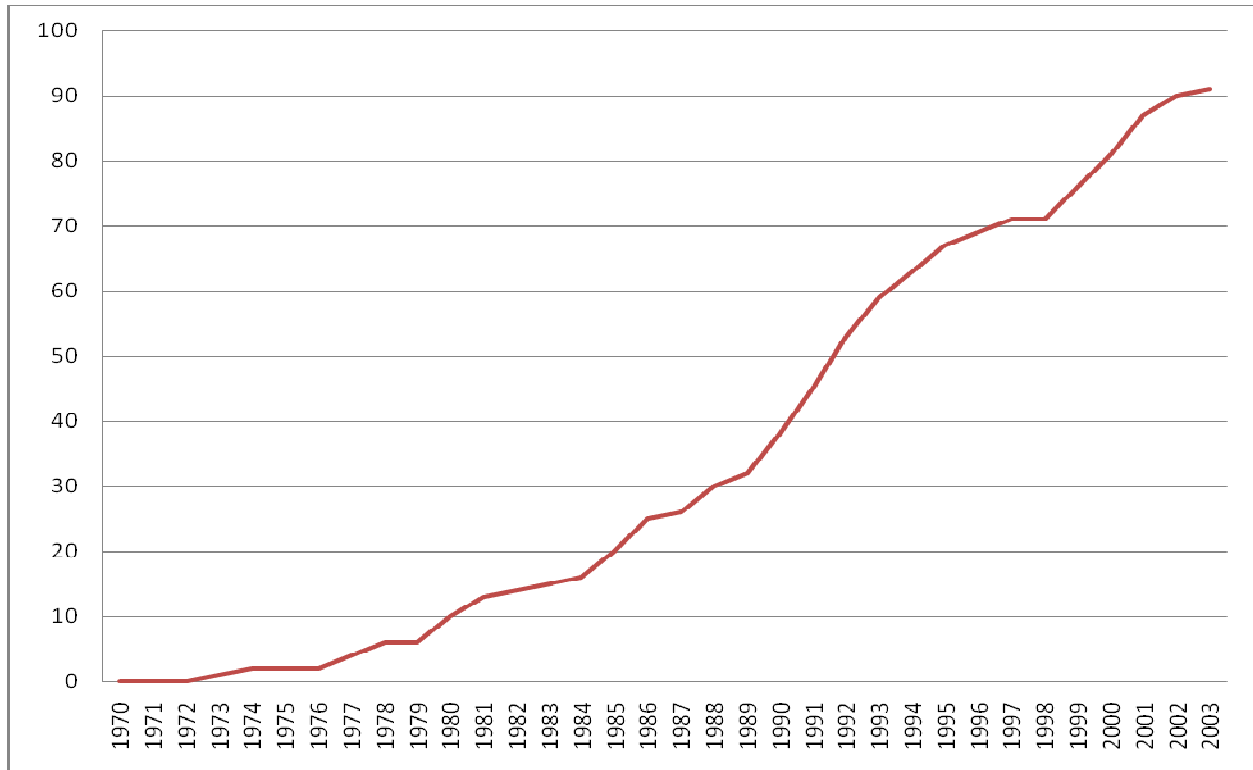


Figure 3: Number of California Cities with Below-Market Housing Mandates

4. The Empirical Model and Tests

Wooldridge (2006) provides an excellent discussion of how to test the impact of a policy using a two-period panel data set. Our approach is to specify a model with unobserved city-specific characteristics that are assumed to be constant over the decade in our regression and then estimate a first difference model (FD) to eliminate the fixed city effect. We also specify a semi-log model so that the first difference model yields the log of the ratio of the dependent variables over the decade. Estimating the model in logs also simplifies the interpretation of the policy variable coefficient as an approximate percentage change rather than an absolute difference in averages.

The FD model provides a way to estimate a difference-in-differences coefficient (DID), which measures the difference in means between a control and treatment group over the two time periods. The control group is defined as cities that did not pass an ordinance, and the treatment group as those cities that passed an ordinance during the time period. In our case the standard definitions are slightly altered. For example, consider the 1990-2000 period. In our sample some cities passed an ordinance prior to 1990, which means that they participate in both periods (1990 and 2000). When the policy dummy variable for program participation is differenced, it will be one for cities that passed an ordinance during the time period (treatment group), zero for cities that already passed an ordinance previously, and zero for cities that did not pass an ordinance as of 2000 (control group).¹⁵ Participation before the time period alters the interpretation of the DID slope estimate. It provides the change in the dependent variable from increased participation in the program.

Eliminating the fixed city effect has significant advantages. Even if two cities are completely different in regard to many different city-specific characteristics, as long as those city-specific characteristics remain constant over ten years, then the change in the policy indicator variable will estimate how much the change in the policy affects our dependent variables, price and quantity. Additional control variables are only necessary to pick up changes in observed city-specific characteristics.¹⁶

We ran our regressions for the 1980-1990 and 1990-2000 periods, and found similar results. The specific coefficients were obviously different, but the coefficients of

¹⁵ We also estimated the model by dropping control group cities that had already passed an ordinance. The general results do not change, but the number of observations in the control group is decreased by 30.

¹⁶ The main disadvantage of including more control variables is the lack of precision arising from control variables that do not vary significantly over the decade.

interest were all of the same sign and statistically significant. The later period contains more data points and control variables so the following discussion focuses on the regressions for the 1990-2000 period because they are more informative.

Let us first consider starting with an ordinary least squares (OLS) model. A cross-sectional model for 1990 and 2000 would be specified as:

$$\ln Y_{it} = \beta_0 + d_{0,t}YR2000_{it} + d_{1,t}IZyr_{it} + \beta_{1,t}X_{it} + a_i + v_{it} \quad \text{(Equation 1)}$$

i = city
t = 1990, 2000

The dependent variables are housing prices or quantity in the respective regressions. The independent variables explaining quantity or price are the policy indicator variable (IZyr), a dummy variable allowing the intercept to change over the decade (YR2000), and other control variables (X_{it}). The problem is that the error term contains two terms: the usual error component (v_{it}) and the unobserved fixed city component (a_i).

The coefficients for each variable are time subscripted to allow for different slopes (or impacts) for each time period. Our policy variable is defined as a lag variable. A new ordinance applies only to projects submitted after the policy date. Other housing projects already in the queue may be exempt from the ordinance. Since housing projects may take several years to complete, lagging the policy variable allows us to see how much time passes before the policy actually impacts the housing market in terms of output and prices. Our approach is to specify a single lag period for each model we estimate. A more general approach would be to specify a set of lags going back in time. Adding lagged variables may help identify a specific lag structure but will likely not change the long run impact of the

policy.¹⁷ We tested several lag periods (regressions available upon request) and found similar significant results for all of the regressions we ran. The coefficient in front of the below-market housing policy variable indicates how adopting the policy between 198X and 199X affects the dependent variables, price and quantity of housing.

The main issue with OLS is that if the unobserved fixed effects (any city-specific characteristic) are correlated with the observed variables, then OLS of the level models will be biased.¹⁸ For example, if cities with nice coastal views happen to be more likely to adopt the policy, then the coefficient of the observed variable of policy adoption may be picking up some of the unobserved characteristics associated with the nice views. Better views could cause higher prices, for example, but a simple OLS regression could attribute the higher prices to policy adoption.

To overcome this problem, differencing the level model eliminates the potential bias of the fixed city effect. Assuming the slopes for both periods are equal, we can simplify the expression into a first difference model (FD):

$$\ln(Y_{i,2000}/Y_{i,1990}) = d_0 + d_1\Delta IZyr_{i,t} + \beta_1\Delta X_{i,t} + \epsilon v_{i,t} \quad \text{(Equation 2)}$$

i = city

Equation 2 indicates an advantage of the first difference model. When there are only two time periods, the first difference model is equivalent to the fixed effects model and can be

17 We did estimate price and output models with all five lagged policy variables (IZ1995-IZ1999) in the model. As expected, the coefficients varied in sign and significance, but the long run impacts were similar to the single lag variable models. The joint F-test of the sum of the lagged policy coefficients was rejected, confirming the observation that simplifying the model does not alter the basic long run impact of the policy.

18 The same problem occurs with OLS on a pooled cross section of both periods.

estimated assuming unequal slopes. The FD model is also easier to adjust and to compute standard errors if heteroskedasticity is present in the error term.¹⁹

Before proceeding to FD model, let us see whether accounting for the fixed city effect is important. The regression we provide below (and others available upon request) found that differencing is very important. For the models measuring price, differencing reduced the absolute value of the policy variable coefficient. The more interesting results came from differencing the models measuring quantity of construction. The OLS level regressions suggest that cities with the policy have higher output. However, as we predict, differencing switches the policy coefficient sign to negative (and statistically significant), providing strong evidence of the importance of eliminating the fixed city effect.

Table 2 reports the first difference estimates (Equation 2) for the average housing price and the number of units in each city when a single lag period is specified. The coefficient for the Below-Market Housing Mandate Policy variable indicates how adopting the policy affects price (first column) and quantity (second column) of housing. The statistically significant results indicate that the policy increases average prices and decreases quantity.²⁰ We also ran regressions with other lags, and all results indicate that cities that imposed a below-market housing mandate during the 1985 to 1995 or up to the 1989 to 1999 time period ended up with approximately 20 percent higher housing prices

19 We estimated several models assuming unequal slopes and concluded that specifying the FD model did not significantly alter the results. We also performed several tests for heteroskedasticity and concluded that only the output model needed adjustment of the standard errors.

20 We also estimated models using the number of households (a number smaller than the total number of housing units in a city) as the dependent variable, and we used per capita income instead of median household income for both the price and output models. We have also estimated the model by dropping the cities from the control group that already passed the policy prior to 1990. The major results did not change.

and 7 percent fewer housing units compared to cities that did not adopt the policy.²¹ Other control variables are insignificant, but, as expected, positive changes in income and education level are associated with increases in the average price of homes.²² For the output model, adjusting for heteroskedasticity reduced most of the standard error estimates on the control variables, but density and travel time to work remain statistically significant.²³ The coefficients of the below-market housing variable were significant in every version of the regression we ran.

We also ran the regressions for changes between 1980 and 1990 and found similar results: cities that imposed a below-market housing mandate in the 1980s ended up with higher prices and lower output. For this period we also ran both the OLS level model and the FD model so we could compare the results.²⁴ Eliminating the city-specific fixed effects had the same impact on the models as in the 1990-2000 period. Tests of the output model also suggested adjusting for heteroskedasticity. The 1980-1990 price effect was smaller in magnitude than that for the 1990-2000 period, but still positive and statistically significant,

21 The exact percentage is calculated with the adjustment of the log estimate into a percentage change for the dependent variable: $\% \Delta Y = \exp(d) - 1$.

22 Interpreting the coefficients of density (population/area) and population together is tricky. Strictly speaking, the density coefficient measures the impact of holding population, but not area, fixed. Similarly, the impact of population could be measured by allowing density to increase while holding the area fixed. Specifying area and population separately might be preferred, but recall that FD models require sufficient exogenous variation over the decade. The lack of variation for most cities would lead to less precise coefficient estimates.

23 The heteroskedastic tests did not reject the null hypothesis of homoskedasticity for the average price models, but they did reject it for the output models. STATA has several options to adjust the standard errors. In most cases, adjusting the output model translated into greater statistical significance of the policy estimates.

24 With three time periods the preferred approach would be the FE model. However, this method would incur some additional problems. The most important is the difficulty of getting comparable variables for all three periods. For the 1980-1990 FD model the control variables were limited to population, density, and income. Given that both FD models provided similar results of a supply restriction, it is doubtful a FE model would yield different results. Finally, recall that the earliest policy date is 1972, and fewer ordinances were in place during the 1980-1990 period. In our sample, the number of cities adopting ordinances between 1980 and 1990 is in the low teens, compared to 1990-2000, when the number jumps to the mid-thirties.

and the 1980-1990 output effect was similar in size and significance. The coefficient of the policy variable was 0.08555 in the regression on price and -0.0781 in the regression on quantity,²⁵ indicating that for the 1980 and 1990 period cities that adopted a below-market housing mandate ended up with 9 percent higher prices and 8 percent fewer homes.

Table 2: How Below Market Housing Mandates Affect the Average Price and Quantity of Housing: First Difference Regression Results.

Dependent Variable:	ln(average price 2000/1990)	ln(# of units2000/1990)
Independent Variables (differenced 2000-1990)	Coefficients and (Standard Errors)	Coefficients and (Standard Errors)
	N=328	N=418
Below-Market Housing Mandate Policy (1985-1995)	0.2187*** (0.0378)	-0.0672** (0.0289)
Population (100,000)	-0.0952 (0.650)	-0.0134 (0.0119)
Density	0.0056 (0.0125)	0.0950*** (0.0178)
Median HH Income (\$1000)	0.128*** (0.0209)	-0.269 (0.166)
Proportion with College Degree	0.8128*** (0.2739)	0.1407 (0.2102)
Proportion > Age 65	0.1215 (0.5914)	0.3313 (0.4239)
Proportion Below Poverty level	-0.9336* (0.5254)	-0.4489 (0.3550)
Mean Travel Time to Work	0.0051 (0.0046)	0.126*** (0.0041)
Constant	-0.0257 (0.0379)	-0.0397 (0.0279)
Adj. R-Squared	0.4788	0.3452
F-Stat	38.55***	28.48***

Note: *, **,*** denotes significance at the .10, .05, .01 levels, two-tailed test.

25 For the 1980-1990 price regression the policy coefficient had a P-value equal to 0.062, and for the quantity regression the policy coefficient had a P-value equal to 0.058.

5. Checks for Robustness

The results indicating that the policy leads to less output and higher prices appear very robust. In this section we describe some of our robustness checks, and none of them lead us to question the overall results. As noted earlier, we performed several specification tests on the lag structure and on whether the slopes for both time periods are equal. These tests suggested little impact from simplifying the model by specifying a single lagged policy variable, assuming equal slopes, and proceeding with a FD model. We performed all of the standard tests for heteroskedasticity. As discussed above, only the output model failed to pass these tests. The general heteroskedasticity tests do not identify a single variable as a problem, but from the adjustments in standard errors, population was clearly the main culprit. Standard errors for population and density were reduced significantly while the standard error for the policy variable was increased.

There are three other estimation issues that are important to address in more detail.

5.1 Potential reverse causality

Using census data to test the policy creates a potential reverse causality issue. The ten-year difference in the dependent variable overlaps the ten-year difference in the policy variable. Even if we use a five-year lag there is still an overlap from 1990-1995. For example, with a five year lag for the policy variable, if we want to analyze what happens to housing prices between 1990 and 2000 for cities that adopted the policy between 1985 and 1995 there is still an overlap from 1990 to 1995. The potential problem is that even if the policy variable predicted higher housing prices, it does not tell us when those increases

occurred. It is possible that the price increases occurred prior to passage of the policy.²⁶ The simplest way to avoid this potential problem is to look at how policy changes in prior periods affect the dependent variable later periods (for example how do policy changes in the 1980s affect price and output changes in the 1990s).²⁷ This eliminates the potential for reverse causality (although it does limit the number of participating cities in the sample and only measures the effect of a policy years after it was first introduced). Table 3 reports how changes in the policy between 1977 and 1987 or 1979 and 1989 period are correlated with changes in prices and output between 1990 and 2000.²⁸ Even with this long lag, the results still suggest a supply restriction from the policy adoption. The coefficients in Table 3 indicate that cities adopting the policy in a decade before the 1990s, end up in 2000 with 15 percent higher prices and five percent fewer homes. These results are particularly important because the cities adopting the policy in these regressions are almost completely different from the cities adopting the policy in the earlier regressions and yet the results are quite similar. Like the cities adopting the affordable housing mandates a decade later, cities adopting affordable housing in the 1980s negatively affect the supply of housing between 1990 and 2000. The lone exception is the negative but not significant output effect for the 1979-1989 policy variable.

26 For example, a city passing an ordinance in 1995 may have experienced large price increases prior to 1995 and even if it experienced a price decrease after 1995 the total decade change could show up positive. The correlation between the price change and policy indicator would be positive even though the price increase occurred before the policy. (As a practical matter average home prices in California actually declined 2.7% from 1991-1995 and grew 37.87% from 1995-2000 [Rand California Statistics Website] so very few cities in the early nineties were experiencing higher growth rates compared to the latter part of the decade, but we did not to dismiss this possibility so we ran the regressions described in Table 3.)

27 An alternative approach would be to use annual data so that the price difference (e.g. between 1999 to 2000) would have occurred after the differenced lagged policy variable. Annual data are available for housing prices and output but unfortunately this would limit the use of census data in providing control variables.

28 We still want to lag prior to 1990 so that the policy has time to impact the 1990 period.

Table 3: How Below Market Housing Mandates Affect the Average Price and Quantity of Housing A Decade or More Later: First Difference Regression Results.

Dependent Variable:	ln(average price 2000/1990)		ln(# of units2000/1990)	
Independent Variables	Coefficients and (Standard Errors)	Coefficients and (Standard Errors)	Coefficients and (Standard Errors)	Coefficients and (Standard Errors)
	N=328	N=328	N=418	N=418
Below-Market Housing Mandate Policy (1979-1989)	0.1514*** (0.0521)		-0.0432 (0.0310)	
Below-Market Housing Mandate Policy (1977-1987)		0.1496*** (0.0346)		-0.0584** (0.0237)
Adj. R-Squared	0.4390	0.4379	0.3518	0.3405
F-Stat	32.98***	32.84***	27.75***	27.91***

Note: *, **, *** denotes significance at the .10, .05, .01 levels, two-tailed test.
(Control variables included but coefficients not reported)

5.2 Potential endogeneity

Another issue to look for is potential endogeneity of the policy variable. Specifically, is the policy variable correlated with the error term? Proponents claim that below-market housing mandates are necessary because housing prices are too high and unaffordable, so cities with relatively higher housing prices would be more inclined to impose an ordinance. We estimated a model using 2SLS and performed several tests, but before discussing our results, let us explore whether an instrumental variables approach may or may not be necessary. First, we estimated several FD models by lagging the policy variable from one to twelve years and found that every policy impact estimate indicates decreased output and

increased price. A lag of these periods (for a potential dependent variable) should reduce or eliminate the potential simultaneity bias.

The issue of endogeneity could still arise if government officials enact price controls based not on current high prices, but on previous high prices that are somehow correlated with the future rate of price appreciation. The FD model reduces the likelihood of potential endogeneity by eliminating the unobserved fixed city effect. Comparing the results of the OLS level model to the FD model suggests that eliminating the fixed city effect is important, as it reduced most of the potential correlation between the error term and the policy variable. Since the difference-in-differences estimator is estimating not price levels but instead price changes over ten years, the issue of endogeneity could arise only if policymakers in cities adopting the policies were able to successfully predict that their housing prices would appreciate (for reasons other than inclusionary zoning) at a higher *rate* than other cities. Since few people are able to predict future prices (especially politicians) we question whether politicians are enacting these price controls with such knowledge in mind.

Nevertheless, we estimated several models and ran several tests to test for the potential impact of endogeneity. We summarize these results below and then present a final set of two stage least squares (2SLS) results. First, we tested for endogeneity using the Hausman (1978) suggestion of estimating the structural price/output equation by including either the predicted (or residual) values from the underlying reduced form equation for the policy variable. If the predicted values are statistically insignificant one can conclude that the policy variable is not endogenous. We estimated the reduced form equation (using LPM, Logit, and Probit) to predict the probability a city would adopt an

ordinance for the periods 1977-87 and 1979-89 as a function of the level of affordability. As our instrument we used a measure of housing affordability defined as the ratio of observed housing rental values to income measures.²⁹ The predicted values were marginally significant (indicating potential endogeneity) for some of the output models and were insignificant (indicating that the policy variable was not endogenous) for the price models.³⁰ Adjusting for heteroscedasticity had a minimal impact on the standard errors. These results may indicate that the rental value to income ratio is not the best instrument but may also suggest the *lack of endogeneity* at least for the price equation.

To investigate whether cities adopted the policy in the 1990s due to housing conditions in the 1980s, we ran a test to see if policy adoption during the 1990-2000 period was correlated with housing output and prices during the 1980-1990 period. We regressed the 1980-1990 price and output changes on the number of cities adopting an ordinance during 1990-2000 along with the control variables used in Table 2 (but differenced for 1980-1990). The results indicated a significant correlation for prices (indicating that cities with significant increases in prices in the 1980s were more likely to adopt the policy in the 1990s) and no significant correlation for housing quantity.

These tests led us to estimate a 2SLS model that included both the lagged policy variable and lagged housing price or output changes as endogenous variables (Table 4). The signs of the coefficients support the hypothesis that the policy restricts supply, but the magnitude of the supply effect should be interpreted with caution. Interpreting the second

29 We also tried using the ratio of rental values to median housing prices as an instrument. This second measure of affordability has the disadvantage of including a price measure on both sides of the price equation, but it reflects the speculative nature of higher housing prices making it more difficult for renters to buy homes. We also tried change in density, population, and household income for 1980-1990 as potential instruments and did not consider any of them to be promising.

30 The t-values ranged for the three models ranged from 1.44 to 2.50.

stage estimate of the policy variable is more complicated since it is defined in terms of a probability rather than a dummy variable. For example the estimated price coefficient of 0.8139 means that a 10% increase in the probability of passing an ordinance leads to a 0.08139 increase in the average housing price. Similar statements apply to the negative but not significant output models. These results do not lead us to reject the findings from our FD regressions. The impact of the policy is smaller and still provides statistically significant results for the price impact but not for output.³¹

Table 4: Two Stage Least Squares Estimates of How Below Market Housing Mandates Affect the Average Price and Quantity of Housing

Independent Variables	ln(average price 2000/1990)		ln(# of units2000/1990)	
	Coefficients and (Standard Errors) N=323	Coefficients and (Standard Errors) N=323	Coefficients and (Standard Errors) N=401	Coefficients and (Standard Errors) N=401
Below-Market Housing Mandate Policy (1979-1989)	0.8139** (0.3264)		-0.6432 (0.4127)	
Below-Market Housing Mandate Policy (1977-1987)		0.9442** (0.3841)		-0.6940 (0.4431)
% Change in Housing Prices (1980-1990)	-0.1390 (0.1876)	-0.1391 (0.1379)		
% Change in Housing Output (1980-1990)			-0.3117*** (0.1033)	-0.2976*** (0.0988)
Density	0.0153 (0.0167)	0.0158 (0.0176)	0.0910*** (0.0142)	0.0902*** (0.0140)
Median HH Income (\$1000)	0.0124*** (0.0027)	0.0116*** (0.0029)	0.0028 (0.0025)	0.0033 (0.0026)
Proportion with College Degree	0.4500 (0.5449)	0.4598 (0.5652)	0.3189 (0.3689)	0.2820 (0.3523)

31 More meaningful 2SLS results would require finding better instruments, if they even exist at all. None of the affordability measures performed well as an instrument in terms of overall correlation with the policy variable. One obvious reason is that our instrument uses rental prices. Rents will be correlated with housing prices, though (rent/income) is not as strongly correlated because of the non-linearity and also possibly due to short run market fluctuations. It may also be that political factors rather than economic conditions are more important in determining the probability of adoption.

Proportion below Poverty Level	-1.6680** (0.7112)	-1.7470** (0.7575)	-0.5721 (0.3207)	-0.5374 (0.5389)
Constant	0.1304 (0.1300)	0.1376 (0.1371)	0.1023** (0.0463)	0.0952** (0.0466)
Adj. R-Squared	0.1205	0.0157		
F-Stat	28.23***	25.31***	9.66***	9.77***

Note: *, **,*** denotes significance at the .10, .05, .01 levels, two-tailed test.

5.3 Changes in other unobserved variables

A fixed effects model works best when all city-specific characteristics are the same in the different time periods. For example, Sausalito had nice views of the San Francisco Bay in 1990 and 2000, and since we are differencing the data, that city specific characteristic will not affect any of the policy coefficients in the regression. Nevertheless, if a city adopting a policy has other unobserved city-specific changes, the effects of those changes can manifest in the policy coefficient.³² Our regressions include various control variables to minimize this problem, but some of the more difficult ones to include due to data

³² Using the average sales price for a city may fail to properly control for heterogeneous housing units. Suppose, for example, that new housing units are larger or higher quality. Here the proper interpretation of the coefficient would be that cities imposing below-market housing mandates have 20 percent higher prices overall rather than them increasing the price of a given home by 20 percent. Some of the increase in average prices over the decade could be attributed to changes in quality. But it's unclear how the policy would impact housing quality. Most developers build in various cities and must supply market rate units based on demand from homebuyers who choose where to locate. It may be that part of the impact could be explained by an increase in the change in higher quality units for cities that adopt the policy, but we suspect that increase would be minor.

Another issue related is the policy variable. As we stated earlier, each ordinance has many characteristics and some are more restrictive than others. For example some cities may require 20 percent of the units to be price controlled while another city may require a small in-lieu fee and/or offset it with developer incentives. But currently we are basing our tests on whether a city adopts the policy or not, which assumes equal impact for all cities adopting the ordinance. Such a variable may not adequately capture this diversity of policy, but this would seem to suggest a diluted impact of the policy variable when compared to a more complex policy variable.

limitations are changes in regulation. If, on the one hand, a city adopted inclusionary zoning while reducing other zoning restrictions, the negative effects of the price controls would be less pronounced (that is, the coefficient of the inclusionary zoning policy variable for housing production would be less negative). If, on the other hand, a city adopted inclusionary zoning while increasing other zoning restrictions, the coefficient of the policy variable would be more pronounced. Thus, unobserved changes in regulations could push the coefficient of the policy variable in either direction. If one takes advocates of inclusionary zoning at their word about wanting to increase the supply of affordable housing, one might observe the adoption of inclusionary zoning at the same time as the removal other exclusionary laws. If, however, advocates of inclusionary zoning have ulterior motives and actually want to use price controls to restrict the supply of housing, as Ellickson (1981) hypothesizes, one might see cities adopt inclusionary zoning at the same time as other restrictions on development.

Since a priori theory cannot tell us which effect predominates, it would be ideal to obtain data on all regulatory changes during the time periods in question. Many cities would have similar sets of zoning regulations between 1990 and 2000, but some would not. By including a measure of how other zoning laws change during the time under study, we could better isolate the effects of below-market housing mandates on construction and housing prices. Obtaining panel data on changes in other zoning laws is difficult, however, because the most comprehensive statewide surveys of California municipality zoning laws are cross sectional (Glickfield and Levine, 1992), and attempts to recreate the same survey over multiple years have been narrowed to cities in certain areas (Quigley, Raphael, and

Rosenthal, 2008).³³ To deal with this we utilized a couple approaches, both of which have limitations, but are the best that can be done until better panel data on regulations exist.

The more promising approach for panel data regressions was to create a mini index of the number of zoning regulations that increased between 1979 and 1998 using a California Department of Housing and Community Development (HCD) survey (Landis et al., 2000).³⁴ Since our interest is in examining changes in a city's regulation over time, we simply created an indicator variable with a value of 1 any time the data indicate a city increasing regulations (for the 1990 and 2000 regressions this would be (number of regulations in 1998 minus regulations in 1989) > 0) and a value of zero for the remaining cities.³⁵ But, as noted earlier, there are important tradeoffs in terms of available data. The HCD survey is limited to eight San Francisco Bay Area counties, and, as with all data based on surveys of city officials, the quality of the information depends on the accuracy of officials filling out the survey. Certain cities clearly did not fill out their surveys completely in this case.³⁶ Recognizing the limitations of this regulation data, we added it and still find

33 Pendall and Rosenthal (2008) are working to create a national survey on zoning regulations, but these results remain in the future. Gyourko, Saiz, and Summers (2008) also present a national index of local regulations, but this is cross sectional and includes less than half of the California jurisdictions in our sample, making it less useful for our purposes.

34 We thank John Quigley and his colleagues for sharing this data with us. The data is from a survey in which city officials were asked if they have a specific type of regulation and, if so, when it was adopted. We follow the method of Malpezzi (1996) and Quigley and Rafael (2005), who count the number of regulations in a jurisdiction to create a regulatory index. As is well known, this method is imprecise because it does not measure the magnitude of the restrictiveness of the various policies. Imprecise as this method may be, Malpezzi and Quigley and Raphael's indexes of zoning laws do correlate with increases in housing prices and decreases in stock, indicating that the indexes have some predictive power.

35 In this case a zero means no reported information for the HCD surveyed cities and unknown for the non-HCD surveyed cities.

36 If a city did not complete the survey, our indicator variable is coded as a zero, even though they may have had increases in regulation during that decade.

that the effect of inclusionary zoning is significant.³⁷ These regressions slightly reduce the magnitude of the coefficients but not the statistical significance (the coefficient indicate a 17 increase in price and 6 decrease in quantity),³⁸ indicating the supply restriction caused by below-market housing mandates.³⁹

6. Conclusion

Using panel data and a first difference model, we found that below-market housing mandates actually lead to decreased construction and increased prices. Between 1980 and 1990, cities that imposed a below-market housing mandate ended up with, on average, 8 percent fewer homes and 9 percent higher prices. Between 1990 and 2000, cities that imposed a below-market housing mandate ended up with, on average, 7 percent fewer homes and 20 percent higher prices. These results are highly significant both statistically and economically. Likewise, the results appear very robust. We estimated several variants of the first difference model and found very little variation on the policy variables.

37 This regression implicitly assumes that regulations remain constant in cities where we are unaware of changes in regulation. If one limits the regression only to cities reporting changes in regulations (n=36), as expected, with so few degrees of freedom the results are insignificant for all variables.

38 With this regulatory measure added, in the regression on price, the policy coefficient was 0.175 and had a P-value = 0.000, and in the regression on quantity, the policy coefficient was -0.0614 and had a P-value = 0.008. The coefficient for the regulation variable was insignificant, however, which we find understandable given how limited the HCD survey was.

39 Separately, we added the 1992 cross-sectional Glickfield-Levine data, which provides various measures of municipal requirements in almost all cities statewide. This data has the advantage of a larger number of cities with variation in data, but the disadvantage is that with only one time period, one cannot properly specify the regulatory variables as first differences. (Referring back to equations (1) and (2), adding an un-differenced variable for one period assumes that the slopes for the two time periods are unequal and that the slope is equal to zero for the other period.) Adding these regulation variables also slightly reduces the magnitude of the coefficients (but not the significance), indicating the supply restriction caused by below-market housing mandates. With this regulatory measure added, the policy coefficient for the price model was 0.1616 with a P-value equal to 0.000, and for the regression on quantity, the policy coefficient was -0.0489 with a P-value equal to 0.013. Unfortunately, it is impossible to measure the real impact of this cross-sectional data without a way to properly capture the difference in the regulatory impact. Overall, the results of including the regulatory impact from available data are encouraging, but at this point an adequate panel data set on other regulations in all California cities does not exist for inclusion in a fixed effects model.

Our findings are consistent with Ellickson's public choice hypothesis that advocates of inclusionary zoning may support the program to because they know it restrict supply. Our findings are also consistent with the hypotheses of developers we interviewed who told us of politicians requiring below-market elements knowing that will make development less attractive and helping kill the development at the approval stage (Personal interview, June 25, 2004, Monterey, California). We found that many of the advocates of inclusionary zoning are also advocates of limiting growth. Economists such as Levine (1999), Quigley and Raphael (2004, 2005), Green, Malpezzi, and Mayo (2005), and Glaeser, Gyourko, and Saks (2005a, b) have documented examples of zoning laws that make housing less affordable, and the results in this article suggest that below-market housing mandates should be added to that list. Inclusionary zoning has unintended consequences exactly opposite as its expressed intentions. Perhaps the consequences are intended.

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