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Below-Market Housing Mandates as Takings: Measuring their Impact

Tom Means, Edward Stringham, and Edward Lopez
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Executive Summary

Housing affordability has become a major issue in recent years. To address the problem, many cities have adopted a policy known as below-market housing mandates or inclusionary zoning. As commonly practiced in California, below-market housing mandates require developers to sell 10–20 percent of new homes at prices affordable to low-income households.

Many developers, however, argue that the program is in violation of the takings clause of the U.S. Constitution because it forces developers to use some of their property to advance a public goal. Nevertheless, in *Home Builders Association of Northern California v. City of Napa* (2001), the court ruled against the regulatory takings argument, saying that below-market housing mandates are legal because (1) they offer compensating benefits to developers and (2) they necessarily increase the supply of affordable housing.

This study investigates these claims in the following way: Section 2 discusses the history of regulatory takings and discusses why below-market housing mandates may be considered a taking. Section 3 investigates how much below-market housing mandates cost

developers. Section 4 investigates econometrically whether below-market housing mandates actually make housing more affordable.

Our research indicates that the decision by the California Courts of Appeal is on shaky ground. Below-market housing mandates require developers to forego substantial amounts of revenue and they provide little offsetting benefit. A mandate in Marin, California, for example, would require developers to forfeit roughly 40 percent of revenue from a project, and builders are offered almost nothing in return.

We can see how below-market housing mandates affect housing markets by using econometrics to analyze data of price and quantity for California cities in 1990 and 2000. Our regressions show that cities that impose a below-market housing mandate actually end up with 10 percent fewer homes and 20 percent higher prices.

For developers, inclusionary zoning has an effect similar to a regulatory taking. For society in general, affordable housing mandates decrease the supply of new housing and increase prices, which exacerbates the affordability problem.



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Below-Market Housing Mandates as Takings

Measuring their Impact

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I. Introduction

High housing prices in recent years are making it increasingly difficult for many to purchase a home. Prices have been rising all over the United States, especially in cities on the East and West Coasts. In San Francisco, for example, the median home sells for \$846,500 (Said, 2007, p.11), which requires yearly mortgage payments of roughly \$63,000 (plus yearly property taxes of \$8,500).¹ Not only is the median home unaffordable to most, but there is a dearth of affordable homes on the low end, too. In San Francisco, a household making the median income of \$86,100 can afford (using traditional lending guidelines) only 6.7 percent of existing homes (National Association of Homebuilders/Wells Fargo, 2007). Households making less are all but precluded from the possibility of home ownership (Riches, 2004).

As a proposed solution, many cities are adopting a policy often referred to as below-market housing mandates, affordable housing

mandates, or inclusionary zoning (California Coalition for Rural Housing and Non-profit Housing Association of Northern California, 2003). The specifics of the policy vary by city, but inclusionary zoning as commonly practiced in California mandates that developers sell 10–20 percent of new homes at prices affordable to low-income households. Below-market units typically have been interspersed among market-rate units, have a similar size and appearance as market-rate units, and retain their below-market status for a period of fifty-five years.² The program is touted as a way to make housing more affordable, and as a way to provide housing for all income levels, not just the rich. In contrast to exclusionary zoning, a practice that uses housing laws to keep out the poor, inclusionary zoning is advocated as a way to help the poor. Because of its expressed good intentions, the program has gained tremendous popularity. First introduced in Palo Alto, California, in 1973, the program has increased in popularity in the past decade

and is now in place in one-third of the cities in California (Non-Profit Housing Association of Northern California, 2007). And it is spreading nationwide, having been already adopted in parts of Maryland, New Jersey, and Virginia (Calavita, Grimes, and Mallach, 1997).

But the program is not without controversy.³ In *Home Builders Association of Northern California v. City of Napa* (2001), the Home Builders Association maintained that by requiring developers to sell a percentage of their development for less than market price, the “ordinance violated the takings clauses of the Federal and State Constitutions.” A ruling by the Court of Appeals in California stated that affordable housing mandates are legal and not a taking because (1) they benefit developers, and (2) they necessarily increase the supply of affordable housing. This report investigates these claims by examining the costs of the programs and reviewing econometrically how they affect the price and quantity of housing.

Our report is organized as follows: Section 2 discusses the history of regulatory takings decisions by the courts and relates them to affordable housing mandates. It provides a brief overview of regulatory takings decisions and discusses the arguments about why affordable housing mandates may or may not be considered a taking. When government allows certain buyers to purchase at below-market prices, it is making sellers sell their property at price-controlled prices. If sellers are not compensated for being forced to sell their property at a below-market price, that may be considered a taking.

Section 3 investigates how much affordable housing mandates cost developers. By calculating the price-controlled level and comparing it to the market price, we can observe the costs to developers each time they sell a price-controlled

home. After estimating how much the program costs developers, we discuss to what extent they are being compensated. We find that the alleged benefits to developers pale in comparison to the costs.

Section 4 investigates econometrically whether below-market housing mandates actually make housing more affordable. Using panel data for California cities, we investigate how below-market housing mandates affect the price and quantity of housing. *We find that cities that adopt below-market housing mandates actually drive housing prices up by 20 percent and end up with 10 percent fewer homes.* These statistically significant findings thus bring into question the idea that mandating affordable housing necessarily increases the amount of affordable housing.

Section 5 concludes by discussing why, contrary to *Home Builders Association of Northern California v. City of Napa* (2001), below-market housing mandates should be considered a taking.

2. Below-market Housing Mandates and Takings

What are “takings,” and should affordable housing mandates be considered a taking? The most familiar form of taking is when the government acquires title to real property for public use, such as common carriage rights of way (roads, rail, or power lines). Precedent for these types of takings is evident in early U.S. jurisprudence, which institutionalized the principle that the government’s chief function is to protect private property.⁴ As such, the government’s takings power was limited in several key respects. Most impor-

tant, the nineteenth-century Supreme Court prohibited takings that transferred property from one private owner to another and upheld the fundamental fairness doctrine that no individual property owner should bear too much of the burden in supplying public uses.

But government's takings power has expanded over time. Takings restrictions were gradually eroded beginning in the Progressive Era and accelerating during the New Deal, as the Supreme Court increasingly deferred to legislative bodies and an ever-expanding notion of public use. Starting in the latter half of the twentieth century, the stage was set to approve takings for "public uses" such as urban renewal (*Berman v. Parker*, 1954), competition in real estate (*Hawaii Housing v. Midkiff*, 1984), expansion of the tax base (*Kelo v. New London*, 2005), and other types of "economic development takings" (Somin, 2004). By the final decade of the twentieth century, one prominent legal scholar described the public use clause as being of "nearly complete insignificance" (Rubinfeld, 1993, p.1078).

Regulatory takings differ in that they are generally not subject to just compensation, because they rest on the government's police power, not the power of eminent domain. Regulatory takings differ also in that the owner retains title to the property but suffers attenuated rights. For example, a government might rezone an area for environmental conservation and thereby prevent a landowner from developing his property. But does an owner still own his property if he is deprived of using it according to his original intent? These were the essential characteristics of the regulation challenged in *Lucas v. South Carolina Coastal Council* (1992).⁵ In that case, David Lucas owned two plots of land that he bought for nearly \$1 million and

intended to develop. But the South Carolina Coastal Council later rezoned his property, stating that it would be used for conservation. The Court sided with Lucas, saying that if he was deprived of economically valuable use, he must be compensated. Under *Lucas*, federal law requires compensation if the regulation diminishes the entire value of the property, such that an effective taking exists despite no physical removal.

This so-called "total takings" test is one of several doctrines that could be used to judge regulatory takings. For example, the diminution of value test could support compensation to the extent of the harm done to the property owner. This was the Court's tendency in the 1922 case *Pennsylvania Coal v. Mahon*, which found that a regulatory act can constitute a taking depending on the extent to which the value of a property is lowered.⁶ So the *Lucas* Court was not up to something new. As a matter of fact, the concept of regulatory takings was discussed by key figures in the American founding era and became an important topic in nineteenth-century legal scholarship as well.⁷

Following in this tradition, the *Lucas* Court addressed several sticking points with regulatory takings law. For example, the majority opinion cited Justice Holmes as stating the maxim that when regulation goes too far in diminishing the owner's property rights, it becomes a taking. However, as the majority opinion pointed out, the Court does not have a well-developed standard for determining when a regulation goes too far to become a taking. Finally, and most important for our purposes, the *Lucas* Court also stressed that the law is necessary to prevent policymakers from using the expediency of police power to avoid the just compensation required under eminent domain. The *Lucas* Court exam-

ined regulators' incentives and voiced its discomfort with the "heightened risk that private property is being pressed into some form of public service under the guise of mitigating serious public harm."

Because they rezone land, requiring owners to provide a public service of making low-income housing, below-market housing mandates seem like they fit into the *Lucas* Court's description of what could be considered a taking. This specific issue, however, is still being debated in the courts. In 1999, the Home Builders Association of Northern California brought a case against the City of Napa for mandating that 10 percent of new units be sold at below-market rates. The Home Builders Association argued that the affordable housing mandate violated the Fifth Amendment's takings clause stating that "private property [shall not] be taken for public use without just compensation." The trial court dismissed the complaint, and in 2001, the Court of Appeals decided against the Home Builders Association, arguing that "[a]lthough the ordinance imposed significant burdens on developers, it also provided significant benefits for those who complied."⁸ In addition, the California court argued that because making housing more affordable is a legitimate state interest, then below-market housing mandates are legitimate, because they advance that goal. Judge Scott Snowden (who was affirmed by Judges J. Stevens and J. Simons) wrote, "Second, it is beyond question that City's inclusionary zoning ordinance will 'substantially advance' the important governmental interest of providing affordable housing for low and moderate-income families. By requiring developers in City to create a modest amount of affordable housing (or to comply with one of the alternatives) the ordinance will necessarily increase the supply of affordable

housing."⁹ The Home Builders Association's subsequent attempts to have the case reheard or reviewed by the Supreme Court were denied.

So the Court's argument rests on two propositions that it considers beyond question: (1) affordable housing mandates provide significant benefits to builders that offset the costs, and (2) affordable housing mandates necessarily increase the supply of affordable housing. Both of these are empirical arguments that can be tested against real-world data. We investigate these propositions in the following two sections.

3. Estimating the Costs of Below-market Housing Mandates

If one wants to state that "[A]lthough the ordinance imposed significant burdens on developers, it also provided significant benefits for those who complied," one needs to investigate the costs of below-market housing mandates in these programs. Yet when this statement was issued by the Court in 2001, there had been no study of the costs.¹⁰ The first work to estimate these costs was done by Powell and Stringham (2004a). Let us here provide some sample calculations and then present some data for costs in various California cities. Once we present the costs, we can consider whether the programs have significant, offsetting benefits for developers.

First let us consider a real example from Marin County's drafted Countywide Plan.¹¹ According to the plan, affordable housing mandates would be designated for certain areas of the county (with privately owned property). In these areas, anyone wishing to develop their property would have to sell or lease 50–60 percent of their property at below-market rates.¹²

The plan requires the below-market-rate homes to be affordable to households earning 60–80 percent of the median income, which means price-controlled units must be sold for approximately \$180,000–\$240,000.¹³ How much does such an affordable housing mandate cost developers? New homes are typically sold for more than the median price of housing, but for simplicity let us assume that new homes would have been sold at the median price in Marin, which is \$838,750. For each unit sold at \$180,002, the revenue is \$658,748 less due to the price control. Consider the following sample calculations for a ten-unit project in Marin that show how much revenue a developer could get with and without price controls.

Sample calculations for a ten-unit, for sale development in Marin County

Scenario 1:

Development without price controls

Revenue from a ten-unit project without price controls

[(ten market-rate units) x (\$838,750 per unit)] = \$8,387,500

Scenario 2:

Development with below-market mandate

Revenue from a ten-unit project, with 50 percent of homes under price controls set for 60 percent of median-income households

[(five market-rate units) x (\$838,750 per unit)] + [(five price-controlled units) x (\$180,002 per unit)] = \$5,093,760

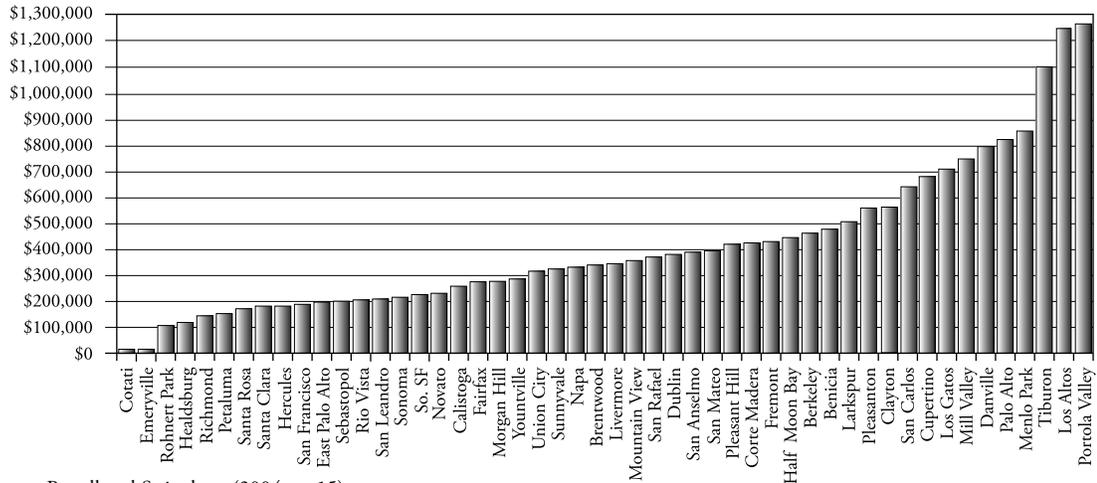
As these calculations show, the below-market housing mandate decreases the revenue from a ten-unit project by \$3,293,740, which is roughly 40 percent of the value of a project. This is just one example, and there are many more.

Powell and Stringham (2004a and 2004b) estimate the costs of below-market housing mandates in the San Francisco Bay Area, Los Angeles, and Orange counties. By estimating how much units must be sold for at below-market rates and comparing this to how much homes could be sold for without price controls, one can estimate how much money below-market housing mandates make developers forgo. Even using conservative estimates (to not overestimate costs), these policies cost developers a substantial amount. Figure 1 shows that in the median San Francisco Bay Area city with a below-market housing mandate, each price-controlled unit must be sold for more than \$300,000 below the 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.

One can estimate the costs imposed by these programs on developers by looking at the cost per unit times the number of units built. This measure is not what economists call deadweight costs (which attempts to measure the lost gains from trade from what is not being built), but just a measure of the lost revenue that developers incur for the units actually built. In many cities, no units have been built as a result of the program, but nevertheless, the costs (in current prices) are quite high. The results for the San Francisco Bay Area are displayed in figure 2. In five cities—Mill Valley, Petaluma, Palo Alto, San Rafael, and Sunnyvale—the amount of the “giveaways” in current prices totals over \$1 billion.

The next important question is whether developers are getting anything in return. If Mill Valley, Petaluma, Palo Alto, San Rafael, and Sunnyvale were to issue checks to develop-

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)

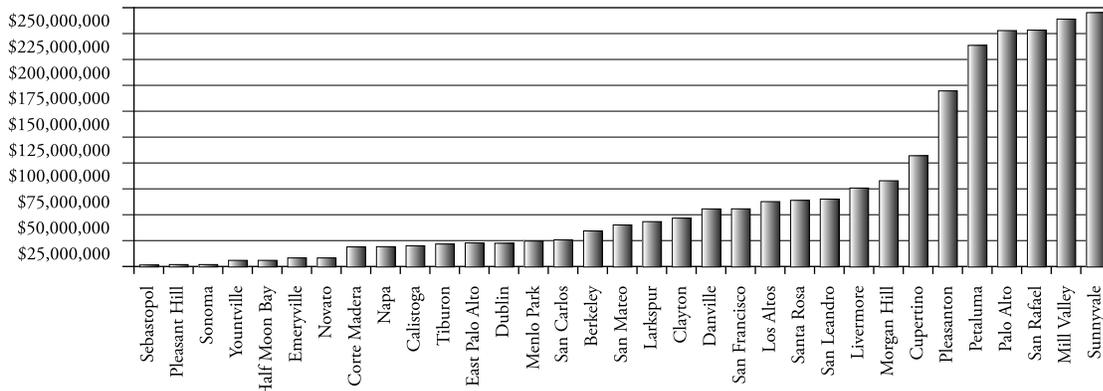
ers totaling \$1 billion, one could say that even though there was a taking, there was also a type of compensation. But the interesting aspect about affordable housing mandates as practiced in California and most other places is that government offers no monetary compensation at all. In fact, this is one of the reasons why advocates of the program and governments have been adopting it. In the words of one prominent advocate, Andrew Dieterich (1996, p. 41), “a vast inclusionary program need not spend a public dime.” In contrast to government-built housing projects, which require tax revenue to construct and manage, affordable-housing mandates impose those costs onto private citizens, namely housing developers. Here we have private parties losing billions of dollars in revenue and receiving no monetary compensation in return.

Monetary compensation for developers is not present, but are affordable housing mandates accompanied by nonmonetary benefits? The Court in *Home Builders Association v. Napa* (2001) stated that “[D]evelopments that include affordable housing are eligible for expedited

processing, fee deferrals, loans or grants, and density bonuses.”¹⁴ According to California Government Code section 65915, government must provide a density bonus of at least 25 percent to developers who make 20 percent of a project affordable to low-income households. The value of these offsetting benefits will vary based on the specifics, but for full compensation to take place, these benefits would have to be more than \$300,000 per home in the median Bay Area city with inclusionary zoning.

One could determine in two ways that the offsetting benefits were worth more than the costs.¹⁵ The first way would be if one observed the building industry actively lobbying for these programs. But in California and most other areas, the building industry is usually the most vocal opponent of these programs. In *Home Builders Association of Northern California v. City of Napa* the court provided no explanation of why the Home Builders Association would be suing to stop a program if it really did provide “significant benefits for those who complied.” If the programs really did benefit developers,

Figure 2
Average Lost Revenue Per Unit Times the Number of Units Sold in
Below-market Programs in San Francisco Bay Area Cities



Source: Powell and Stringham (2004a, p.15)

there would be no reason why developers would oppose them.

Why don't builders want to sell units for hundreds of thousands less than market price for each unit sold? Or why don't California builders want to forgo billions in revenue? All of the builders with whom we have spoken have stated that the offsetting "benefits" are no benefits at all. For example, a city might grant a density bonus, but the density bonus might be completely unusable, because density restrictions are just one of a set of restrictions on how many units will fit on the property. Other constraints such as setbacks, minimum requirements for public and private open space, floor area ratios, and even tree protections make it extremely complicated to get more units on the property. Conventional wisdom suggests that building at 100 percent of allowable density will maximize profits, but in reality developers tend to build out at less than full density. The City of Mountain View recently passed a policy requiring developers to provide an explanation for projects that failed to meet 80 percent of the

allowable density.¹⁶ Prior projects had averaged around 65 percent of allowable density. So giving builders the opportunity to build at 125 percent of allowable density is often worth nothing, when so many other binding regulations exist.

The second and even simpler way to determine whether the affordable housing mandates provide significant benefits to compensate developers for their costs would be to make the inclusionary zoning programs voluntary. Developers could then weigh the benefits and costs of participating, and if the benefits exceeded the costs, the developers could voluntarily comply. A few cities in California tried to adopt voluntary ordinances, and perhaps unsurprisingly, they 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 significant "benefits" of these programs are not as significant as the costs. In this sense, the program has the character of a regu-

latory taking. In addition to observing whether builders would support or voluntarily participate in these programs, we can also analyze data to observe how these programs affect the quantity of housing. If the Court in *Home Builders Association v. Napa* is correct that the benefits are significant, then we would predict that imposing an affordable housing mandate would not affect (or it would encourage) housing production in a jurisdiction. If, on the other hand, the program is not compensating for what it takes, we would predict that cities with the program will see less development than in otherwise similar cities without the program. Here the program is a taking that will hinder new development.

4. Testing How Below-market Housing Mandates Affect the Price and Quantity of Housing

The court in *Home Builders Association v. Napa* puts forth an important proposition, which we can examine statistically. The court states: “By requiring developers in City to create a modest amount of affordable housing (or to comply with one of the alternatives) the ordinance will *necessarily increase* the supply of affordable housing” (emphasis added). Although the court suggests that it is an a priori fact that price controls will increase the supply of affordable housing, the issue may be a bit more complicated than these appellate judges maintain. Before getting to the econometrics, let us consider some simple economic theory and simple statistics about the California experience. First, if a price control is so restrictive, developers cannot make any profits and so the price control can easily drive out all development from an area. Cities such as

Watsonville adopted overly restrictive price controls, and they all but prevented development until they scaled back the requirements (Powell and Stringham, 2005). Over the course of thirty years in the entire San Francisco Bay Area, below-market housing mandates have resulted in the production of only 6,836 affordable units, an average of 228 per year (Powell and Stringham, 2004a, p. 5). Controlling for the length of time each program has been in effect, the average jurisdiction has produced only 14.7 units for each year since adopting a below-market housing mandate. Since the programs have been implemented, dozens of cities have produced a total of zero units (Powell and Stringham, 2004a, pp. 4–5). So unless one defines zero as an increase, it might be more accurate to restate “necessarily increase” as “might increase.”

Economic theory predicts that price controls on housing lead to a decrease in quantity produced. Because developers must sell a percentage of units at price-controlled rates in order to get permission to build market-rate units, this policy also will affect the supply of market-rate units. Powell and Stringham (2005) discuss how the policy may be analyzed as a tax on new housing. If below-market-rate housing mandates act as a tax on housing, they will reduce quantity and increase housing price. This is the exact opposite of what advocates of below-market-rate housing mandates say they prefer. So we have two competing hypotheses, that of economic theory, and that of the court in *Home Builders Association v. Napa*. Luckily, we can test these two hypotheses by examining data for housing production and housing prices in California.

Our approach is to use panel data, which has a significant advantage over simple cross-sectional or time-series data. Suppose a city adopts the policy, there is an unrelated statewide

decline in demand, and housing output falls by 10 percent. A time-series approach would still have to control for other economic factors that might have changed and reduced housing output. One would still need to compare the reduction in output from a city that adopted the policy to a nearby similar city that did not. A cross-sectional approach can control overall economic factors at a point in time but will not control for unobserved city differences. Our approach is to set up a two-period panel data set to control for unobserved city differences and to control for changes over time. The tests, which we explain in detail below, will enable us to see how adopting a below-market-rate housing mandate will affect variables such as output and prices.

4.1. Description of the Data

The first set of data we utilize consists of the 1990 and 2000 census data for California cities. The 2000 census data are restricted to cities with a population greater than ten thousand, while 1990 census data are not. A decrease in population for some cities during the decade resulted in a loss of fifteen cities from the sample. We do not include the 1980 census, because there were few policies in effect during this decade (Palo Alto passed the first policy in 1972). Focusing on this decade also highlights some economic issues. From 1987 to 1989, housing prices grew very rapidly. Prices for the first half of 1989 grew around 25 percent, only to fall by this amount for the second half of the year, and continue to slide as the California economy declined. For some areas, prices did not recover to their original level until halfway through the 1990 decade. The California economy grew faster in the second half of the decade due to the dot-com boom

in the technology sector. Data from the RAND California Statistics Web site provided average home sale prices for each city for the 1990 and 2000 period. The RAND data do not report 1990 home sale prices for some cities, resulting in a loss of more observations. Summary statistics are provided in table 1.

Data on the policy adoption dates came from the California Coalition for Rural Housing and Non-profit Housing Association of Northern California. Table 2 describes the summary statistics of the policy variables that we constructed. *IZyr* is a dummy variable defined to equal one if the city passed a below-market-rate housing ordinance that year or in prior years. As noted above, differences in population cutoff points and missing 1990 housing prices reduced the sample of cities that passed (or did not pass) an ordinance. Starting in 1985, our sample contains fifteen California cities that had passed an ordinance. The number increased to fifty-nine cities by the end of 1999. The last column reports the difference between decades. In other words, *iz95delta* reports the number of cities that passed an ordinance between 1985 and 1995. The difference variables are fairly constant and capture a large number of cities that passed ordinances during the decade. Focusing on the 1990–2000 decade should allow us enough observations to capture the impact of the policy.

4.2. Empirical Tests

Jeffrey Wooldridge (2006) provides an excellent discussion of how to test the impact of a policy using two-period panel data. Our approach is to specify a model with unobserved city effects that are assumed constant over the decade (1990–2000) and estimate a first-difference model

to eliminate the fixed effect. We also specify a semilog model so that the first difference yields the log of the ratio of the dependent variables over the decade. Estimating the models in logs also simplifies the interpretation of the policy variable coefficient as an approximate percentage change rather than an absolute difference in averages. For the policy variable, we define $IZyr$ as a dummy variable equal to one if the policy was in effect during the current and previous years. To see the importance of the first-difference approach, consider a model specified for each decade.

Level Model:

$$\ln Y_{i,t} = \beta_0 + d_0 YR2000_{i,t} + d_1 IZyr_{i,t} + \beta_1 X_{i,t} + a_i + v_{i,t}$$

(Equation 1)

$i = \text{city}$

$t = 1990, 2000$

The dependent variable is either housing output or housing prices, $YR2000$ is a dummy variable allowing the intercept to change over the decade, $IZyr$ is the policy dummy variable, and the X are control variables. The error term contains two terms: the *unobserved* fixed city component (a_i) considered fixed for the decade (e.g., location, weather, political tastes); and the usual error component (v_{it}). If the unobserved fixed effect is uncorrelated with the exogenous variables, one can estimate the model using ordinary-least-squares for each decade. The coefficient for $IZyr$ measures the impact of the policy for each decade.¹⁷ Unfortunately, estimating the level model may not capture the differences between cities that passed an ordinance and the ones that did not. In other words, suppose cities with higher housing prices are more likely to adopt the policy. The dummy variable may cap-

ture the impact of the policy along with the fact that these cities already have higher prices.

The above issues can be addressed by differencing the level models to eliminate the fixed city effect, which yields the first-difference model.¹⁸

First-Difference Model

$$\ln Y_{i,2000} - \ln Y_{i,1990} = d_0 + d_1 IZyr_{i,2000} - d_1 IZyr_{i,1990} + \beta_1 X_{i,2000} - \beta_1 X_{i,1990} + v_{i,2000} - v_{i,1990}$$

(Equation 2)

$i = \text{city}$

which can be rewritten as:

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

(Equation 3)

$i = \text{city}$

$t = 2000$

Eliminating the unobserved fixed city effect, which we show below in the last two columns of tables 3 and 4, has an important effect on estimating the impact of the policy variable. Differencing the panel data also yields a dummy variable that represents the change in policy participation over the decade (an example of this is the $iz95delta$ appearing in tables 2 through 6). When policy participation takes place in both periods (1990 and 2000), the interpretation of the differenced dummy is slightly different from the usual policy treatment approach. The differenced dummy variable predicts the average change in the dependent variable due to an increase (or decrease) in participation.

To see the advantage of the first-difference approach, we first estimated (without control variables, which we will add in tables 5 and 6) the un-differenced equations of the log of aver-

Table 1 Summary Statistics

| Variable | Observations | Mean | Standard Deviation | Minimum | Maximum |
|---------------------------------|--------------|---------|--------------------|---------|-----------|
| Population 2000 | N=446 | 65,466 | (197,087) | 10,007 | 3,694,834 |
| Population 1990 | N=431 | 58,468 | (187,014) | 1,520 | 3,485,398 |
| Households 2000 | N=446 | 22,251 | (68,673) | 1,927 | 1,276,609 |
| Households 1990 | N=431 | 20,512 | (66,074) | 522 | 1,219,770 |
| Housing Units 2000 | N=446 | 23,278 | (71,843) | 2,069 | 1,337,668 |
| Housing Units 1990 | N=431 | 21,745 | (70,331) | 597 | 1,299,963 |
| Density 2000 (persons/acre) | N=446 | 7.62 | (6.06) | 0.42 | 37.32 |
| Density 1990 (persons/acre) | N=431 | 6.87 | (5.88) | 0.08 | 37.01 |
| Median Household Income 2000 | N=446 | 52,582 | (21,873) | 16,151 | 193,157 |
| Median Household Income 1990 | N=431 | 38,518 | (14,543) | 14,215 | 123,625 |
| Per Capita Income 2000 | N=446 | 23,903 | (13,041) | 7,078 | 98,643 |
| Per Capita Income 1990 | N=431 | 16,696 | (8,070) | 4,784 | 63,302 |
| Rents/Income 2000 | N=446 | 27.60% | (3.1%) | 14.4% | 50.1% |
| Rents/Income 1990 | N=431 | 28.9% | (2.7%) | 14.9% | 35.1% |
| Average Home Price 2000 | N=360 | 300,594 | (235,436) | 49,151 | 2,253,218 |
| Average Home Price 1990 | N=352 | 206,754 | (112,804) | 52,858 | 1,018,106 |

age housing prices and output ($\ln Y_{i,t} = \beta_0 + d_l IZ_{y_{i,t}}$) over various lagged policy dummies. The first four columns in table 3 report the estimated coefficients (d_l) for each lag year for the level models. The left two columns show the

coefficient estimates for the five regressions that look at housing prices in 1990 and have $iz1985$, $iz1986$, $iz1987$, $iz1988$, or $iz1989$ as the policy variable. The third and fourth columns in table 3 show the coefficient estimates for the five regres-

Table 2 Summary Statistics – Policy Variables

| Variable | # of cities with inclusionary zoning (in that year) | Variable | # of cities with inclusionary zoning (in that year) | Variable | Change in # of cities with inclusionary zoning (over 10 years) |
|----------|---|----------|---|---------------------------------------|--|
| iz1985 | 15 | iz1995 | 50 | iz95delta (which is iz1995-iz1985) | 35 |
| iz1986 | 19 | iz1996 | 52 | iz96delta (which is iz1996-iz1986) | 33 |
| iz1987 | 19 | iz1997 | 54 | iz97delta (which is iz1997-iz1987) | 35 |
| iz1988 | 22 | iz1998 | 54 | iz98delta (which is iz1998-iz1988) | 32 |
| iz1989 | 23 | iz1999 | 59 | iz99delta (which is iz1999-iz1989) | 36 |

Table 3 Summary of Policy Coefficients from Fifteen Regressions on the Price of Housing by Model and by Lag Year

Dependent Variable: ln(Price)

| Level models for 1990 data | | Level models for 2000 data | | First-difference models (2000–1990) | |
|----------------------------|--------------------------------|----------------------------|--------------------------------|-------------------------------------|--------------------------------|
| Policy Variable | Coefficient of Policy Variable | Policy variable | Coefficient of Policy Variable | Policy variable | Coefficient of Policy Variable |
| iz1985 | .389 | iz1995 | .627 | iz95delta | .312 |
| iz1986 | .431 | iz1996 | .642 | iz96delta | .298 |
| iz1987 | .431 | iz1997 | .637 | iz97delta | .278 |
| iz1988 | .442 | iz1998 | .637 | iz98delta | .270 |
| iz1989 | .457 | iz1999 | .642 | iz99delta | .265 |

sions that look at housing prices in 2000 and have iz1995, iz1996, iz1997, iz1998, or iz1999 as the policy variable. For example, the 0.389 in the first row indicates that cities with inclusionary zoning in 1985 had 47.6 percent ($\exp(0.389) - 1$) higher than average prices in 1990, and the 0.627

in the first row indicates that cities with inclusionary zoning in 1995 had 87.2 percent higher-than-average prices in 2000. For both decades, the impact increases slightly as the lag period is decreased, though the impact for the 2000 period is much larger than the 1990 period.

Table 4 Summary of Policy Coefficients from Fifteen Regressions on the Quantity of Housing by Model and by Lag Year

Dependent Variable: ln(Housing Units)

| Level models for 1990 data | | Level models for 2000 data | | First-difference models (2000–1990) | |
|----------------------------|--------------------------------|----------------------------|--------------------------------|--|--------------------------------|
| Policy Variable | Coefficient of Policy Variable | Policy variable | Coefficient of Policy Variable | Policy variable | Coefficient of Policy Variable |
| iz1985 | .777 | iz1995 | .665 | iz95delta | -.045 |
| iz1986 | .751 | iz1996 | .614 | iz96delta | -.024 |
| iz1987 | .751 | iz1997 | .585 | iz97delta | -.027 |
| iz1988 | .679 | iz1998 | .585 | iz98delta | -.038 |
| iz1989 | .653 | iz1999 | .618 | iz99delta | -.051 |

The estimated coefficients (d_1) for 1990 and 2000 range from 0.389 to 0.642 and indicate that cities with inclusionary zoning have 48–90 percent higher housing prices, but this does not take into consideration the possibility that cities that adopted the policy already had higher prices when they did so. To account for this potential problem, the first-difference model estimates how changes in the policy variable (adopting a below-market housing ordinance) alone affect housing prices. The last two columns of table 3 report the first-difference estimates ($\ln(Y_{i,2000}/Y_{i,1990}) = d_0 + d_1\Delta IZyr_{i,t}$). For example, the 0.312 in the last column of the first row indicates that cities with below-market housing mandates have 36.6 percent higher prices. Each of the estimated coefficients in table 3 are significant at the 1 percent level. The results in the last two columns indicate that below-market housing mandates have increased the price of the average home by 30 to 37 percent.

The results for housing output (the number of units) are even more interesting. These results are presented in table 4. The estimates of d_1 for

the level models for 1990 and 2000 are positive and statistically significant at the one percent level, which indicates that cities with inclusionary zoning have more housing production, but similar to the housing price regressions do not take into consideration the possibility that cities that adopted the policy already were growing when they adopted the policy. Again, we need to look at the difference in output based on cities adopting the policy. The last two columns in table 4 show how changes in the policy variable (adopting a below-market-rate housing ordinance) alone affect the quantity of housing. Eliminating the unobserved fixed effect by differencing the data switches the sign of the policy variable from positive to negative (though most are statistically insignificant without control variables). This switch in sign of d_1 provides strong evidence of the importance of eliminating the unobserved fixed city effect. The negative impact increases in size and statistical significance when control variables are added to the first-difference model.

Table 5 Regression Results of How Below-market Housing Mandates Affect the Price of Housing: First-difference Model with Control Variables

Dependent Variable: $\ln(\text{average price } 2000/1990)$

| Independent Variable | Coefficients and (Standard Errors) | Coefficients and (Standard Errors) |
|----------------------|---------------------------------------|---------------------------------------|
| | $N=431$ | $N=431$ |
| Constant | 0.001 (0.025) | -0.009 (0.025) |
| iz95delta | 0.228*** (0.038) | |
| iz99delta | | 0.217*** (0.037) |
| median income | 0.173*** (0.0126) | 0.178*** (0.0125) |
| density | -0.007 (0.011) | -0.008 (0.011) |
| population | -0.0017 (0.00661) | -0.00112 (0.00662) |
| rent % | -0.002 (0.005) | -0.003 (0.005) |
| Adj. R-Squared | 0.4332 | 0.4300 |

Tables 3 and 4 indicate the importance of differencing the data and removing the unobserved fixed city effect.¹⁹ The next set of regressions in table 5 report first-difference estimates for housing prices for the five-year and one year lag while adding other control variables that may change over time.²⁰ The other models (using lag periods iz96delta, iz97delta, and iz98delta) yielded similar results. Adding income, whether median household income or per capita income, increases the size of the estimated policy effect. All policy estimates of d_1 are larger than 0.20, suggesting that *cities that impose an affordable housing mandate drive up prices by more than 20 percent*. Dropping the insignificant variables and adjusting for heteroscedasticity had little impact on the policy and income variables.

The final set of results in table 6 reports the estimated effects on housing quantity for the same lag periods as the price estimates. The results are nearly identical for the other lag periods (iz96delta, iz97delta, and iz98delta). Adding control variables increases the policy impact and its statistical significance. Substituting the number of households for the number of units as the dependent variable does not alter the main results. Adjusting for heteroscedasticity did increase the statistical significance levels slightly for the policy variable. The negative policy coefficients (-0.104 and -0.097) suggest that cities that impose an affordable housing mandate reduce housing units by more than 10 percent.

Table 6 Regression Results of How Below-market Housing Mandates Affect the Quality of Housing: First-difference Model with Control Variables

Dependent Variable: ln(units 2000–1990)

| Independent Variable | Coefficients and (Standard Errors) | Coefficients and (Standard Errors) |
|----------------------|---------------------------------------|---------------------------------------|
| | N=431 | N=431 |
| Constant | -0.056** (0.023) | -0.054** (0.023) |
| iz95delta | -0.104** (0.042) | |
| iz99delta | | -0.097** (0.041) |
| median income | 0.0683*** (0.0132) | 0.0660*** (0.0131) |
| density | 0.113* (0.011) | 0.114 (0.011) |
| population | 0.0233* (0.00729) | -0.0230* (0.00729) |
| Adj. R-Squared | 0.2921 | 0.2911 |

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

5. Conclusion

Our research provides answers to two important questions: How much do below-market housing mandates cost developers, and do below-market housing mandates improve housing affordability? After showing that below-market housing mandates cost developers hundreds of thousands of dollars for each unit sold, we discussed how developers do not receive compensation in this amount. Next we investigated how these policies affected the supply of housing. Using panel data and first difference estimates, we found that below-market housing mandates lead to decreased construction and increased prices. Over a ten-year period, cities that imposed a below-market housing mandate on average ended up with 10 percent fewer homes

and 20 percent higher prices. These results are highly significant. The assertion by the court in *Home Builders Association v. Napa* that “the ordinance will necessarily increase the supply of affordable housing” is simply untrue.

The justification for the decision that below-market housing mandates are not a taking rests on some extremely questionable economic assumptions. We are not sure about the amount of economics knowledge of Judges Scott Snowden, J. Stevens, and J. Simons. Below-market housing mandates are simply a type of price control, and nearly every economist agrees that price controls on housing lead to a decrease in quantity and quality of housing available (Kearl et al., 1979, p.28). Because these price controls apply to a percentage of new housing, and builders must comply with them

if they want to build market-rate housing, price controls also will affect the supply of market-rate housing. Because price controls act as a tax on new housing, we would expect a supply shift leading to less output and higher prices for all remaining units.

New names for price controls, like “inclusionary zoning,” make the policy sound innocuous or even beneficial (who can be against a policy of inclusion?), but in reality the program is a mandate that imposes significant costs on a minority of citizens. The costs of below-market housing mandates are borne by developers and other new homebuyers who receive little or no compensation. From this perspective, below-market housing mandates are a taking no different in substance from an outright taking under eminent domain. Below-market housing mandates represent the sort of abuse the *Lucas* Court forewarned, and they should rightly be considered a taking. In terms of economics, below-market housing mandates only differ from an outright taking in degree—there is not a “total taking” but a partial taking and clearly a diminution of value without any compensation. The amount of harm imposed by below-market housing mandates should inform their status under the law.

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Notes

- 1 Assuming a 30-year fixed-interest-rate mortgage with an interest rate of 6.3 percent.
- 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).
- 3 For review of the literature, see Powell and Stringham (2005).
- 4 "The country that became the United States was unique in world history in that it was founded by individuals in quest of private property. . . . [T]he conviction that the protection of property was the main function of government, and its corollary that a government that did not fulfill this obligation forfeited its mandate, acquired the status of a self-evident truth in the minds of the American colonists." Pipes (1999, p.240).
- 5 *Lucas v. South Carolina Coastal Council*, 505 U.S. 1003 (1992).
- 6 *Pennsylvania Coal v. Mahon* 260, U.S. 393 (1922).
- 7 As legal scholar James Ely writes, "In his famous 1792 essay James Madison perceptively warned people against government that 'indirectly violates their property, in their actual possessions.' Although Madison anticipated the regulatory takings doctrine, the modern doctrine began to take shape in the last decades of the nineteenth century. For example, in a treatise on eminent domain published in 1888, John

- Lewis declared that when a person was deprived of the possession, use, or disposition of property ‘he is to that extent deprived of his property, and, hence . . . his property may be taken, in the constitutional sense, though his title and possession remain undisturbed.’ Likewise, in 1891 Justice David J. Brewer pointed out that regulation of the use of property might destroy its value and constitute the practical equivalent of outright appropriation. While on the Supreme Judicial Court of Massachusetts, Oliver Wendell Holmes also recognized that regulations might amount to a taking of property. ‘It would be open to argument at least,’ he stated, ‘that an owner might be stripped of his rights so far as to amount to a taking without any physical interference with his land.’” (Ely, 2005, p.43, footnotes in original omitted.)
- 8 *Home Builders Association of Northern California v. City of Napa* (2001), p. 188.
 - 9 *Home Builders Association of Northern California v. City of Napa* (2001), pp. 195–6.
 - 10 The California Coalition for Rural Housing and Non-profit Housing Association of Northern California (2003, p.3) stated, “These debates, though fierce, remain largely theoretical due to the lack of empirical research.”
 - 11 Marin County is one of the highest-income and most costly areas in the San Francisco Bay Area.
 - 12 http://www.co.marin.ca.us/EFiles/Docs/CD/PlanUpdate/07_0430_IT_070430091111.pdf (accessed August 19, 2007). To simplify the specifics, developers have the choice of selling 60 percent of homes to low-income households or 50 percent of homes to very-low-income households, which calculates to roughly the same loss of revenue, so for simplicity we will focus on the latter scenario.
 - 13 Median income for a household of four is \$91,200, so a household earning 80 percent of median income earns \$73,696, and a household earning 60 percent of the median income earns \$55,272. The specific affordability price control formula will depend on certain assumptions (for example, the level of the interest rate in the formula), but using some standard assumptions we can create an estimate (assuming homes will be financed with 0 percent down, a 30-year, fixed-rate mortgage, and an interest rate of 7 percent, and assuming that 26 percent of income will pay mortgage payments and 4 percent of income will pay for real estate taxes and other homeowner costs).
This formula gives us how much a household in each income level could afford and the level of the price controls. In Marin County, a home sold to a four-person household earning 80 percent of median income could be sold for no more than \$240,003, and a home sold to a four-person household earning 60 percent of the median income could be sold for no more than \$180,002.
- The price controls may be set at stricter levels, depending on the city ordinance. For example, the City of Tiburon sets price controls for “affordability” much more strictly than the above formula. Its ordinance assumes an interest rate of 9.5 percent and assumes that 25 percent of income can be devoted to a mortgage. According to Tiburon’s ordinance, a “moderate,” price-controlled home can be sold for no more than \$109,800.
- 14 *Home Builders Association of Northern California v. City of Napa* (2001), p.194.
 - 15 Powell and Stringham (2005) discuss this issue in depth.
 - 16 Policy on Achieving Higher Residential Densities in Multiple-Family Zones, (September 13, 2005).
 - 17 For those readers unfamiliar with semilog models, d_1 provides an interpretation of the policy variable as a percentage change. The estimate of d_1 is interpreted as the approximate percentage change in Y for cities that pass an ordinance. When the estimate of d_1 is large (greater than 10 percent), the more accurate estimate is $\% \Delta Y = \exp(d_1) - 1$.
 - 18 The first difference model is the fixed-effects model when there are two time periods.
 - 19 Controlling for the endogeneity of the policy variable will have little or no impact. The data reveal that cities that passed an ordinance also have higher housing prices on average. It may be that higher-priced cities are more likely to pass an ordinance. Given our results, we have some doubts about whether this will impact our conclusion. First we lagged the policy variable from one to five years and found very little variation in the OLS estimates. A lag of five years (for a potential dependent variable) should reduce or eliminate the potential bias. Second, the first-difference approach reduced the price effect and significantly changed the output effect by controlling for unobserved fixed effects. Finally, there are some limits to finding instrumental variables for a first-difference model. Clearly it would not be appropriate to use any of the 2000 data to control for policies passed in earlier years. One could use the 1990 census data, but even here there are some cities that passed the policy prior to 1990. For these reasons, we believe controlling for endogeneity will not change the basic results.
 - 20 The income and population variables are rescaled in units of ten thousand to simplify the coefficient presentation.

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