Do Housing Bubbles Generate Fiscal Bubbles?

Evidence From California Cities*

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Abstract

This paper examines the effects of the most recent U.S. housing bubble on the fiscal policy of California cities. We use an instrumental variables approach that helps isolate fiscal consequences of house price appreciation by taking advantage of the influence of local topological constraints on the elasticity of house prices with respect to interest rates. Our analysis generates three main findings. First, despite Prop 13 fiscal constraints, rapid house price appreciation has a strong effect on property tax revenue. Second, the resulting increase in property tax revenue was largely offset by a reduction in other local tax revenue. This offsetting response leaves total city expenditure unrelated to local house prices. In that sense the housing bubble did not produce local fiscal bubbles. Third, we find that fiscal adjustments to house price appreciation depend on local political institutions.

Keywords: Housing Bubble, Local Government, Fiscal Policy, Political Institutions, California Cities.

JEL Classifications: H2, R5

1 Introduction

The U.S. housing downturn that triggered the recent financial crisis and economic recession was preceded by a decades-long housing boom, peaking around 2005. According to U.S. Census Bureau data, median sale prices of new homes more than doubled between 1991 and 2005. This latter part of the boom saw the steepest increases in prices. Interestingly, this upward price trend was a rare constant during this period that has witnessed two recessions and a severe stock market downturn.

Housing market developments are of special interest to local government officials and voters because property taxes are one of the two major items of own-source revenues in city budgets, along with sales taxes. Many have wondered about the extent to which house prices affect property tax collections. While there is clearly a strong mechanical relationship in the absence of any tax policy adjustment, the answer to this question is complicated by several institutional constraints.

First, property taxes are levied not on property prices but on property values as assessed by city officials. As the assessments typically take two years to reflect new housing market conditions (NLC 2010), the lag on reassessment may mitigate any relationship. Second, in states like California and Florida, voters have passed measures that restrict the city government's ability to alter their property tax bill. In the case of California Prop 13 places a hard cap on the tax rate as well as on property value appreciation. Under Prop 13, the adjustment of property tax revenue to conditions in the housing market occurs through the reassessment of newly traded property to current market prices. Thus, whether property tax revenue closely tracks housing market conditions or Prop 13 effectively insures city governments from any fluctuations is an open question.

More broadly, we seek to understand how city fiscal policy as a whole is affected by housing market trends. Unlike the federal government, cities operate under a balanced budget requirement. Therefore any windfall in tax revenues has to either be offset by cuts in other taxes and fees, or needs to be spent on additional services. Identifying this causal effect requires removing sources of endogeneity, as house prices are likely to be strongly related to local labor demand, public good supply, etc. Finally, local political institutions may significantly affect the magnitudes of these fiscal adjustments. For example, home rule cities or cities with indirectly accountable executives may have more flexibility in adjusting to emerging fiscal conditions.

This paper addresses these questions using city-level data from the state of California from 1993 to 2007. To deal with the likely endogeneity of house prices we instrument for house prices using topology-based housing supply constraints (Saiz 2010) interacted with national borrowing cost trends. To our knowledge we are the first to utilize this identification strategy to measure the direct effect of house prices on city government policy.

Our analysis reveals three main results. First, using our instrumental variables approach we find that the elasticity of property tax revenue with respect to house prices is 0.74, and not statistically different from one. The elasticity is significantly higher than our Ordinary Least Squares (OLS) results, and an earlier OLS estimate of 0.4 due to Lutz (2008), indicating a quite elastic relationship between house prices and property tax collections. This number is particularly noteworthy in a state like California where voter initiatives like Prop 13 have capped increases in assessed values at 2 percent, or the inflation rate, whichever is smaller, until the property is resold. It seems likely that fiscal rigidities were alleviated by the increased volume of transactions that characterized the housing bubble.[1]

A second major finding of our instrumental variables analysis is that city spending does not respond to house price increases. In that sense, the recent housing bubble has not led to a fiscal bubble. Instead, we find evidence that cities roll back other local taxes to maintain budget balance. Importantly, these finding are very different from the positive spending effects that our OLS analysis indicates, suggesting that unobservable local economic shocks may lead to upward biases in a more correlational approach.

Lastly, we document significant effects of local political institutions on cities' fiscal adjustment to housing shocks. Home rule cities and manager-council cities cut non-property taxes more. We find no evidence that the number of elected officials matters for

[1] In the state of California there were 321,494 units traded in 1995 and 649,837 units traded in 2005, according to real estate data compiled by the private company DataQuick.

the magnitude of the fiscal adjustment.

Our elasticity results are related to Lutz, Molloy, and Shan (2010) who use state-level aggregate data to measure the impact of house price declines on four state and local tax revenues. They find that property tax revenues do not tend to drop when house prices decline. They conclude that the drop in state and local tax revenues is primarily driven by the overall economic recession than by the housing market. Other studies of the response of local government revenue to housing market fluctuations come to similar conclusions. Doerner and Ihlanfeldt (2010) examine how local governments in Florida adjust to housing price fluctuations and conclude that property price appreciation plays a small role in determining local revenues as millage rates are adjusted to tax base changes. Similarly, the analysis in Alm and Sjoquist (2010) of local governments in Georgia reveals that local jurisdictions routinely adjust millage rates to bring revenues in line with expenditures.

Our spending and institutional response results echo Poterba (1996) who uses state-level data on unanticipated revenue and spending in a period of fiscal stress between 1988 and 1992. He finds that states respond to unexpected deficits primarily by increasing taxes in the next fiscal year, in a one-to-one relationship. There is also evidence of a cut in spending in the current year, but the effect is smaller, about 60 cents spending cut per capita for every dollar of unexpected deficit. Political factors play an important role in the speed of fiscal adjustment: unified control of state government and non-election years increase the magnitude of the adjustment. The analysis in Skidmore and Scorsone (2010) reveals that Michigan cities respond to fiscal stress by cutting spending on recreation and capital improvements as well as road and building maintenance.

The next section discusses the housing bubble of the 1990s and the first half of the 2000s, and the institutional environment of California city governments. Section three presents the empirical strategy, followed by a discussion of the data in section four. The last section presents the estimation results. In the conclusion we suggest several directions for future work.

2 Background

In this section we discuss the housing bubble and the potential effects on, and responses of, city governments.

Housing Bubbles and Property Tax Revenue. In many ways the recent U.S. housing price bubble has been unprecedented. As Glaeser, Gottleib and Gyourko (2010) note, between 1996 and 2006 real housing prices rose by 53 percent based on the Federal Housing Finance Agency price index. The picture is even starker in California. House prices more than tripled over this time period going from $158,991 in 1996 to $520,124 in 2006 in California MSAs. As many have noted if property tax rates do not adjust the large increases in house prices may well produce substantial windfalls for local governments. More ominously, as the housing bust begins to generate substantial property tax revenue reductions, many have wondered what the implications for local government will be. Of course, because house prices can affect local governments in many ways – and may simply reflect rather than cause local government outcomes – whether house prices have a causal effect on local government outcomes is an empirical question.

Before proceeding to a discussion of the potential impacts of house price inflation on city government finances more broadly, it is important to understand how property tax is applied in California. In 1978, California voters passed a ballot measure to substantially reduce the growth in property tax collection by local government (see e.g. Coleman 2005). The measure, known as Prop 13, limits property taxes to 1% of the assessed value of the property and, perhaps more importantly for the present study, limits increases in property assessed value to 2% per year.

These limits on assessed value appreciations are clearly much lower than the actual increases in house prices during the bubble. However, the law also requires that the baseline value of a property resets every time there is a new market transaction. Thus, while Prop 13 does place a hard cap on increases in non-transacting property values, newly purchased property fully reflects market values, property taxes in California should change more under market bubble conditions due to the increased volume of transactions.[2]

[2]It is however important to note that while Prop 13 shares some features with property tax limitations

Housing Bubbles and City Fiscal Policy. There are a number of potential channels through which a housing bubble can affect local governments.[3] The first and most obvious effect is though property tax revenue. If property tax rates are not being adjusted to housing market conditions, then any effect will be purely mechanical. Focusing on the purely mechanical relationship between house prices and the property tax leads many to conclude that the substantial decline in house prices in the ensuing housing bust will lead to a large reduction in local government spending.

While it seems likely the decline in house prices will affect local fiscal outcomes, the exact magnitude of the response depends on a number of factors. First, while property tax revenues may represent an important source of local government own-source revenues, these revenues account for a relatively small fraction of city spending. In the case of California cities, we can see from the first column of Table 1 that property tax revenues only account for about 10% of total expenditure. Thus, even if property tax revenues were to fall by 50%, this would only directly reduce city spending by about 5%. Second, as elected officials are able to respond to windfalls in property taxes by adjusting other taxes the effect of house prices on city spending is unclear. For example, if an increase in property tax revenue is fully offset by cuts in other local taxes (such as sales, hotel or utility tax rates etc.), we expect that there will be little relationship between property tax revenue and total government spending. Of course, the fact that property tax revenue represents a large fraction of discretionary revenue (Coleman 2005) the response of fiscal policy to property value changes may well be more elastic than the modest share would suggest.

Third, any relationship between house prices and local government spending could be due to local economic shocks that affect both other tax revenues and house prices simultaneously. In this case, any relationship between house prices and local government outcomes would reflect unobservable local economic shocks rather then the causal effect of house prices per se, so that the correlation between house prices and local government outcomes would say little about the effect of a housing market bubble on local budgets.

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in other states any features Prop 13 that are specific to California may limit the transportability of our estimates to municipalities in other states.

[3]See Lutz, Molloy, and Shan (2010) for an excellent discussion of the potential channels for the effects of house prices on local governments

To get a first take on the relationship between house prices and city fiscal policy we plot time series graphs in Figure 1 and Figure 2. The first noticeable pattern in the figures is the large increase in average house prices, especially after the year 2000. The figures also reveal two other findings of interest. First, Figure 1 shows that the increase the average house price is accompanied by a much larger increase in non-property taxes than in property taxes. This suggests that a key source of revenue from the housing bubble was not due to the property tax channel alone. Second, Figure 2 shows that while average house prices increased dramatically during this period there was relatively little change in city government spending.

Taken together the figures suggest that: (1) the effect of house price appreciation on city government was not primarily due to the property tax channel, and (2) the effect of the housing bubble on city government spending was quite small. As drawing conclusions from time-series graphs alone is fraught with many difficulties we next outline an empirical approach designed to determine the extent to which this data can inform us about the causal effect of housing market conditions on city budgets.

3 Empirical Strategy

This section introduces our empirical approaches. It first describes a simple Ordinary Least Squares (OLS) approach to estimate the effect of house prices on city government tax and spending policy. The OLS estimates serve as a useful baseline for our Instrumental Variables (IV) estimation approach. This second approach addresses the key sources of bias likely present in the OLS estimates.

Ordinary Least Squares. The most straightforward approach is to simply estimate the following model by OLS:

$$y_{it} = \gamma_0 + \gamma_1 HP_{i,t-2} + year_t + city_i + e_{it}, \tag{1}$$

where y_{it} is the log tax revenue or spending per capita in city i in year t, $HP_{i,t-2}$ is the average house price in city i year $t-2$, $year_t$ is a set of year fixed effects, $city_i$ is a set of city fixed effects, and e_{it} is the error term. The coefficient of interest is γ_1 which measures the house price-fiscal policy gradient.

Clarity about the timing of our variables is especially important given the fact that we are identifying our parameter of interest off of changes in variables. Cities in California receive a share of the property tax revenue collected by counties (school districts receive the other major share of property tax revenue). As noted above property tax revenue is reassessed annually. This reassessment frequency implies that property valuation changes affect property taxes with a one year lag. We also introduce an additional lag in the relationship between house prices and fiscal policy measures as city budgets are generally based on the previous year's tax collection. Furthermore, the two year lag structure closely corresponds to the 18 to 24 month lag estimate by the National League of Cities (NLC 2010).

We would expect that γ_1 would reflect a positive relationship between house prices and property tax revenue. However, what to expect for the relationship between house prices and other taxes or total expenditure is less clear. On the one hand, special interests may be able to capture a tax revenue windfall. For example public sector unions paying close attention to city revenue could bargain for higher wages when the city experiences a revenue windfall. In this case, we would expect little reduction in other taxes and a corresponding increase in expenditure due to an increase in property taxes from house price appreciation.

On the other hand, the demand for public goods by voters may not depend on house prices, and they may push their elected officials to return the windfall to them in the form of lower taxes. In this case, we would expect little increase in total expenditure and an offsetting reduction in other local taxes if special interests are held in check by effective electoral accountability. Ultimately, the relationship between house prices and local policymaking is an empirical question.

As noted above, the simple approach in equation (1) may not yield a parameter with a causal interpretation. One key concern is that both house prices and local fiscal policy may simply reflect local business cycles. When economic activity in a location is high it is also likely that local housing prices are elevated. As high levels of economic activity are likely to also result in high levels of revenue, we would estimate a positive effect even if none were present. In addition, as the level and composition of economic activity is likely to change across cities over time, and be correlated with house prices, omitted variable bias is an important concern.

Instrumental Variables. To address the concern that house prices might be related to unobservable determinants of fiscal policy we also estimate an IV specification. This empirical approach attempts to identify potentially exogenous sources of variation in housing prices by taking advantage of the fact that changes in national borrowing costs are likely to affect different housing markets differently. Specifically, our IV strategy takes advantage of the fact that housing prices in areas with geographic constraints on housing supply are likely to be more responsive to national changes in borrowing costs.

In particular, we instrument for $HP_{i,t-2}$ in equation (1) with the following first-stage model:

$$HP_{i,t-2} = \beta_1(BC_{t-3} \times SC_i) + \beta_2 BC_{t-3} + year_{t-2} + city_i + u_{i,t-2} \quad (2)$$

where $HP_{i,t-2}$ is the average house price in city i in year $t-2$, $BC_{t-3} \times SC_i$ is the interest rate on a ten-year bond at $t-3$ (BC_{t-3}) interacted with the geographic constraints on housing supply in city i (SC_i), $year_{t-2}$ is a set of year fixed effects, $city_i$ is a set of city fixed effects, and $u_{i,t-2}$ is the error term. As the city fixed effects flexibly control for all time invariant differences in house prices across cities we do not include the main effect of the time-invariant geographic supply constraints SC_i in the model. In implementing our IV approach we also include BC_{t-3} as an additional control in equation (1) for consistency even though the year fixed effects flexibly control for time series variation.

The intuition behind our identification strategy is straightforward. Even though all cities experience the same change in the demand for housing due to national changes in borrowing costs, any effects on house prices will depend on the responsiveness of housing supply to changes in demand. Cities where supply is very responsive due to weak land use regulations or much available land will see much smaller changes in the price of housing than cities where supply is not very responsive. Thus, armed with a measure of the supply elasticity of a city's housing market, we can use changes in long term interest rates to obtain plausibly exogenous variation in house prices.

A few other estimation details are worth noting. First, we cluster the standard errors at the city level to address the concern that city fiscal policy may be serially correlated within a city. Second, in measuring the scale of city expenditure, we use total spending from all revenue sources: city taxes, state support, federal grants, fee income, city borrowing, etc. By comparing the total local tax response to the total expenditure response, we are able to get a sense of whether other non-local fiscal policy adjustments are important in explaining city government responses to house price changes. Third, we follow Glaeser, Gottleib and Gyourko (2010) and use the 10-year Treasury Bond rate as our measure of national borrowing costs trends BC_{t-3}.

It is important to point out exactly what parameter our approach IV identifies. Our IV analysis seeks to obtain the causal effect of house prices on local government fiscal policy. As such, our approach does not identify other effects of the housing bubble on local government outcomes from tax revenue generated by expanded economic activity, building permit fees, or other non-house price channels.[4]

Our approach has a number of limitations. First, it does not account for changes in the quality of homes built in a city. Our estimates will also pick up the effect of house price appreciation due to higher quality, and perhaps larger homes, driven by borrowing costs. To the extent that the presence of higher quality homes is correlated with the demand for public goods, perhaps due to the income level of the population our estimates will also pick up these effects.

Second, as our estimates are for California cities alone our estimates will pick up any specific features of constraints placed on property tax rate setting and distribution by Prop 13. While our approach cannot fully separate out the effects of Prop 13 from other factors we can get a sense of the importance of Prop 13 by looking at the symmetry of city responses to house price fluctuations. Prop 13 is only binding for *increasing* house prices. Thus, if the constraints imposed by Prop 13 play a key role in determining our estimates, we would expect that the responses to house prices would be highly asymmetric. Alternatively, if the fiscal policy responses to increasing and decreasing house price fluctuations are similar, Prop 13 constraints are unlikely to be central in determining our estimated effects.

[4]Recent evidence reported in Lutz, Molloy, and Shan (2010) indicates that the total effect on government revenue of the housing bubble through these other channels is modest.

Potential Challenges to the Identification Strategy. Our identifying assumption is that absent national borrowing cost changes, local fiscal policy in cities with different supply constraints for housing would have changed similarly. This assumption is reasonable since both national borrowing costs and supply constraints for housing should not be correlated with changes in a city's fiscal policy directly. Of course, cities with different geographic constraints on housing supply may differ in other ways that could affect fiscal policy, such as the income level of the population, the presence of strong interest groups, or the availability of valuable amenities. Any such differences that are time-invariant will be differenced out in the fixed effects model. Only differential trends in fiscal policy across cities driven by unobservables that are correlated with the supply constraints could pose a threat to our identification strategy. While it seems reasonable that our assumption is valid, it is instructive to consider cases where it might be violated.

It is possible that borrowing cost shocks affect city budgets differentially. First, city budgets themselves might be directly affected by changes in borrowing costs. For example, cities with access to capital markets may be more sensitive to borrowing conditions. If cities with geographic constraints on housing supply are say more likely to carry high debt levels our identifying assumption may be threatened. While all cities in California are subject to the same debt limits and a balanced budget requirement reduces the likelihood that any jurisdiction is particularly indebted, we address this potential concern by comparing our estimates of the local tax revenue response to the total expenditure responses. If cities do not primarily adjust through borrowing, we will see that local tax responses are commensurate with the expenditure response.

Second, it is possible that borrowing costs affect other sources of city revenue. For example, small firms that are more credit constrained may be more sensitive to cyclical conditions (see e.g. Moscarini and Postel-Vinay 2009). If the location of credit constrained firms is correlated with geographic supply constraints, then our identification assumption may be threatened.

In sum, while we cannot completely rule out the possibility that some of the effect reported below reflects time-varying city-specific changes in unobserved fiscal policy within a city, it appears that many sources of spurious correlation are controlled for.

Heterogeneous Effects. To get a better sense of how political institutions affect the response of fiscal policy to house price changes we also test for heterogeneous changes across cities. Our approach tests for these mechanisms in a very straightforward way. We simply interact the house price variable in model (1) with the relevant measure of city political institutions. We consider three different measures of city political institutions. First, we examine whether cities with a form of government based on an executive who is appointed (council-manager) rather than elected (mayor-council) have different responses. Second, we examine whether the response of cities depends on the number of elected officials. Finally, we examine whether home rule cities have a different response from general law cities.

To estimate heterogenous effects we alter model (1) in the following manner:

$$y_{it} = \gamma_0 + \gamma_1 HP_{i,t-2} + \gamma_2 (HP_{i,t-2} \times PI_i) + year_t + city_i + e_{it}, \tag{3}$$

where all the variables are defined as above and PI_i is the political institution in city i. The coefficient γ_2 reveals whether city responses to house price changes do indeed depend on political institutions.[5]

4 Data

Our city-level fiscal policy data for fiscal years 1993 to 2007 comes from the *City Financial Transactions Report* (CFTR). The CFTR data comprise a uniquely detailed database

[5]To implement IV versions of model (3) we also alter the first stage model to include the additional interaction between the excluded instrument ($BC_{t-3} \times SC_i$) and the political institution variable (PI_i) as the additional excluded instrument is needed to identify the interaction effect. More specifically our first-stage model for these specifications is given by:

$$HP_{i,t-2} = \beta_1 (BC_{t-3} \times SC_i) + \beta_2 BC_{t-3} + \beta_3 (BC_{t-3} \times SC_i \times PI_i) + year_{t-2} + city_i + u_{i,t-2} \tag{4}$$

together with an analogous model for $HP_{i,t-2} \times PI_i$, where the variables are defined as above. An implicit assumption in this approach is that city political institutions are not endogenously determined by fiscal policy. While we cannot completely rule out the possibility that the choice of city political institutions could be affected by fiscal policy the evidence in Vlaicu and Whalley (2011) on council-manager government and Whalley (2010) on city treasurer appointive status shows that accounting for endogeneity does little to alter estimates of the effect of city political institutions on city policymaking.

with extensive coverage of many financial variables collected annually by the California State Controller for each city. All of the current 475 cities in California are required to file a report. More than 99% of cities file a financial transactions report in any given year.[6] We use variables on total expenditures, property tax revenue, and other local own-source revenues. We construct our measure of fiscal policy by using annual city population data from the California Department of Finance to express the variables in per capita terms. We then match in data on annual house prices in each city based on the transactions reported by DataQuick, an independent provider of real estate data and analysis. Our borrowing costs variable is the 10-year interest rate reported in Shiller (2010). All monetary variables are reported in nominal terms as the central question concerns the effect of house price inflation on policy and controlling for flexible time trends effectively adjusts for any state-wide trends.

We match this data with the housing supply data from Saiz (2010) and the Wharton Land Use Index (Gyourko, Saiz, and Summers 2007) to measure fixed geographic constraints on housing supply and the stringency of local land use regulations, respectively. Together the data from CFTR and the Saiz (2010) data form the panel of California cities that we use in our analysis. We match in data from two further sources. First, data from the 1990 Decennial Census provides measures of population, demographic, and economic characteristics of each city. Second, the 1992 Census of Governments contains data on city government organization.

We construct our sample in the following way. We first identify all California cities that appear in the 1992 Census of Governments. These 456 cities form our base population. The few cities that incorporate after 1992 are not in the sample. We then match these data to 1990 Decennial Census and CFTR data by city. Finally, we drop cities that are not located in an MSA, as the housing supply constraint data exists only for cities in MSAs. Our final analysis sample contains 140 cities.

Table 1 presents descriptive statistics for all our sample cities. Columns (1) and (2) show the means and standard deviations computed over all city-year observations dividing cities by above and below median of the house price distribution. The comparison yields

[6] Government Code section 12463 directs the California State Controller to annually compile and report to the public the financial transactions of all California cities.

a number of interesting results. First, governments in cities with higher house prices spend more per capita and receive more taxes from property and other local sources. The spending in cities with high house prices are more than 30% greater than those in cities with low house prices.

Second, residents of cities with high house prices also have higher average income per capita, are more educated, and less likely to be non-white. These differences in per capita income and demographics could well lead to higher demand for public goods regardless of the level of house prices. As cities with higher house prices have more stringent land use regulations and less available land the higher house prices in these cities could be due to supply constraints and directly cause the higher levels of public spending. The descriptive statistics thus indicate that cities with higher house prices spend and tax more, but differ on key observables which likely affect the size of the public sector independently of house prices.

5 Results

We first present estimation results for a baseline model of house price effects on major fiscal categories. We then explore how these effects vary with local political institutions: home rule, city council size, and city form of government.

5.1 OLS Results

Table 2 presents the estimated effects of house prices on three fiscal variables: property tax revenues, other local tax revenues, and total spending, all measured in nominal per capita dollars. We use a specification with fixed effects and year effects to control for time-invariant heterogeneity across cities that is correlated with both house prices and fiscal outcomes, such as city location, as well as for factors common to all California cities that affect house prices and fiscal outcomes over time, such as state-level fiscal policy.

Column (1) of the table shows that a 1 percent increase in the average house price is associated with a 0.26 percent increase in property tax collections. The effect is significantly different from unity, implying an inelastic response: property tax revenues change less than proportionally with the tax base. Other tax revenues do not change significantly with home prices. However, total spending does respond, by a magnitude comparable to the property tax revenue increase.

These results suggest that house price appreciation is associated with increased city spending. To what extent is this relationship causal? The fixed effect specification does not rule out the possibility that this effect partly reflects reverse causality: improvements or expansions of city services attract home owners to the city, driving up house prices. Even if the effect is causal, is it measured without bias? It could be that a confound distorts the identification of the causal effect: for instance it could be that population growth driven by local demand shocks, puts pressure on house prices, even as it increases a city's ability to provide public goods more cheaply due to economies of scale.

5.2 IV Results

To deal with potential endogeneity we instrument for house prices using an exogenous geographic determinant of housing supply. In a recent paper, Saiz (2010) identified urban geography as a major factor in residential development. This is measured by the percentage of land supply that is constrained. Since our empirical approach requires variation in house prices within a city, we interact constraints on land supply with the 10-year interest rate and look at how the cost of financing a house purchase affects house prices differently in cities with different geography.

Ideally we would like an instrument that is clearly not determined by the outcome of interest and is a very strong predictor of the endogenous variable, house prices. The analysis in Saiz (2010) suggests two housing supply responsiveness measures that could qualify as possible instrument candidates. First, we use the percentage of land vacant for geographic reasons. This measure is largely determined by the topology of certain areas within a city being too steep to build on safely. In that sense it is an excellent candidate for an instrument as it is clearly not determined by city fiscal policy. The second measure is land supply elasticity. This measure includes both geographic and city regulatory constraints in determining the response of housing supply to increases in prices. As city regulatory policy might be related to fiscal policy regardless of house prices, this measure may be less exogenous to fiscal policy. Of course, it might also have better explanatory power of city house price fluctuations as it uses more information. To decide which measure is a stronger instrument we start by examining both.

The results of fitting equation (2) are reported in Table 3. Each column in the table reports the results from one regression. The estimates in column (1) show that a one percentage point increase in the previous year's 10-year interest rate reduces house prices by 6 percent more in cities with a higher percentage of constrained land supply compared to less land-constrained cities. House prices are thus significantly more sensitive to mortgage rates in cities where residential development is constrained by the presence of steep-sloped terrain.

Turning to column (2), we again see that house prices in cities that have less available land are more sensitive to mortgage rates than those with more available land. However, the relationship between house prices and the land supply constraint-mortgage rate interaction is stronger in column (1).[7] Thus, we chose to form our instrument based on the geographic land supply constraint in column (1). However, as the F-statistic is below the rule-of-thumb level of 10 for the stronger instrument, we also report weak instrument consistent confidence intervals based on inverting the Anderson-Rubin test statistic (Anderson and Rubin, 1949; Dufor and Taamouti, 2005).

The effects of the instrumented house prices are presented in Table 4. Again the table reports the results from one regression in each column. In column (1) a one percent increase in house prices leads to a 0.74 percent increase in property tax collections two years later. The effect is almost three times larger than the OLS estimate in Table 2. Notice that the elasticity is not statistically different from one: a one percent increase in the tax base leads to a one percent increase in property tax revenues. This result is in sharp contrast to Lutz, Molloy, and Shan (2010), who in the context of the recent housing downturn, find little change in property tax revenues accompanying fluctuations in the

[7] It is important to point out that while our first-stage excluded instrument is statistically significant the fact that the F-statistic is not above 10 indicates that some concerns with finite sample bias may remain (Bound, Jaeger, and Baker, 1995).

local housing market.

In column (2) a one percent increase in house prices leads to a 0.55 percent decrease in revenues from other local taxes. Again, this estimate is measurably different from its OLS counterpart. It seems to suggest that an increase in property taxes is offset by a roll-back in other sources of revenues. This is not surprising, given the tight balanced budget rules under which city governments operate. The result is similar to Poterba (1996) who finds that an $100 unexpected state budget surplus per capita is followed by a $13 tax cut in the next fiscal year.[8] In column (3) the IV results imply a zero effect of house price increases on city spending. A possible explanation is that city government keeps the level of spending in lockstep with the demand for public services, and this is not significantly affected by home values, at least in the short term. Thus, a windfall of property tax receipts does not translate in an immediate increase in spending, but, consistent with column (2), is rather offset through rolling back other taxes.

The weak instrument consistent confidence intervals reveal that the inferences based on the IV estimates do not seem to depend on the weakness of the instruments. A property tax revenue elasticity of one is within the confidence interval. The confidence interval for the other local tax revenue does not contain zero, but the confidence interval for total spending does contain zero. Thus, in our context, any concerns with the weakness of the instrument do not affect inferences drawn from the IV estimates.

Importantly the difference between the IV and OLS estimates suggests that OLS estimates suffer from substantial bias. The bias may reflect (1) reverse causality: improvements or expansions of city services leads to an appreciation of property values and/or (2) omitted variables: the relocation of a large employer to the city improves the city's fiscal position and encourages more residents to settle in the city for a longer period, driving up the prices of homes.

Given our estimate of the causal effect of house prices on city fiscal policy a natural question to ask is whether the effect is symmetric relative to the direction of price change. To answer this question we break down the main effects by looking at city responses to

[8]Poterba (1996) instruments deficit and surplus surprises with econometric forecasts of state revenues and expenditures based on lagged spending, revenue and state personal income.

house price increases versus decreases. We present the results from the IV version of this specification in Table 5. While this distinction was important in previous work on state fiscal responses to budget deficits versus surpluses (Poterba 1996), here we cannot reject the null hypothesis of equal and opposite-sign effects of increases and declines in house prices on any of the dependent variables. A lack of strong evidence for asymmetric effects also suggests, as discussed in Section 3, that the constraints imposed by Prop 13 may not be binding in a period of intense housing market activity.[9]

5.3 Heterogeneous Effects

In Tables 6 through 8 we explore the extent to which the housing market's impact on local budgets depends on local political institutions. Throughout we focus on IV estimation of the interaction models as the OLS approach appears to entail significant bias.

In Table 6 we consider the differences between home rule cities and state-dependent cities. The estimates in column (1) suggest that the elasticity of property tax revenues to house prices does not depend on city type. Nor should it, since as long as the city council cannot change the property tax rate in the short run - because of the Prop 13 constraints on the tax rate - this elasticity should only reflect an accounting identity.

Column (2) shows that home-rule cities respond differently to house price increases: their other tax revenues drop by more (by 0.27 percentage points for each 1 percent). This may reflect a stronger incentive to thoroughly offset the revenue windfall, either because of more effective local accountability or because the city cannot rely so much on the county or state to bail them out. The spending effects in column (3) suggest that spending in home rule cities is also trimmed down after a rise in house prices. In Table 7 we estimate the effect of city council size on fiscal responses to the housing market boom. The estimates in column (1) show that the elasticity of property tax revenues to the tax base, as in Table 6, does not depend on the institutional environment.

[9] We are unfortunately unable to calculate weak instrument consistent confidence intervals for the interaction models because procedures to calculate weak instrument consistent confidence intervals do not yet exist for the case with more than one endogenous variable.

The political economy literature has suggested two channels through which legislatures affect spending. One is a representational inefficiency driven by common pool incentives: legislators with discretionary spending power do not internalize the social cost of a public project and therefore spend more than socially optimal. This is known as the Law of $1/n$ (Weingast, Shepsle and Johnsen 1984): the inefficiency increases with the legislature size. The other channel is internal accountability: a larger legislature makes it more costly for the agenda setter to pass a wasteful budget proposal because he needs to buy more legislative votes. This reduced discretion can be exploited by voters who hold the agenda setter to a higher standard of performance (Vlaicu 2008).

The size of the effect thus depends on which channel is stronger. The estimates in column (3) suggest no effect of the number of elected officials on cities' spending response to increased revenues. Column (2) is similarly consistent with the notion that a reduction in other types of local taxes that does not depend on the number of elected officials.[10]

In Table 8 we test whether there are heterogeneous effects according to form of government. As before, property tax revenue elasticity does not depend on the form of city government - see column (1). Manager cities experience a larger drop in other tax revenues, however, by 0.19 percentage points for every percent increase in house prices. It is possible that managers have more expertise and skill in handling budget modifications, so they are able to offset budget windfalls more completely than mayors. Another explanation is that tax hikes are generally unpopular, so mayors being more dependent on popular favor are more reluctant to cut taxes now if they perceive this decision needs to be reversed in a future year. Finally, in column (3) we find no difference in the housing market effect on city spending due to city form of government.

6 Conclusion

This paper has explored California cities' fiscal responses to the housing boom of the past two decades. We proposed a strategy to eliminate potential sources of endogeneity, namely

[10] It is important to note that our test may lack power as there is little variation in the number of elected officials in our sample. The vast majority of cities in our sample have a five-member council.

isolating the sensitivity of house prices to mortgage rates according to city topological features. The results presented in this paper indicate that: (1) house prices have a large impact on property tax collections, and (2) house prices do not affect city spending, rather, cities respond to revenue windfalls by rolling back non-property taxes. More broadly our results imply that the housing bust that followed the boom will have little implications for city spending.

While our analysis generates new insights into the role of local housing market conditions in determining city fiscal policy the results should be properly interpreted. First, we emphasize that when estimating house price impacts we are keeping constant the evolution of the non-housing sectors of the local economy. In most states the housing sector is closely interconnected with the rest of the economy, and so the potential for indirect effects can be large. Secondly, our analysis is based on examining how short-term fluctuations in fiscal policy respond to short-term fluctuations in house prices. The extent to which our results apply to longer-term house price fluctuations depends on how the balanced budget requirements cities face do in fact constrain fiscal policy.

Lastly, we do not fully account for the psychology of a housing bubble, in particular the role of expectations in this phenomenon. It is likely that the fiscal response to a transitory shock is different from the response to a shock perceived to be permanent. This may imply that the response in the early years of the bubble is different from the response in the later years. Studying the adjustment dynamics in more depth can give us more insight into the full economic and fiscal effects of housing bubbles. We hope that future research will take up these questions and thereby deepen our understanding of the role that housing market performance plays in city fiscal policies.

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TABLE 1: Baseline Descriptive Statistics in 1993

	Full Sample (1)	Low House Prices (2)	High House Prices (3)	(2) – (3) t-stat (4)
A. City Government Expenditure, Revenue, and House Prices				
Total Spending Per Capita	829	706	952	2.41
	[615]	[593]	[616]	(0.017)
Property Tax Revenue Per Capita	83	47	119	5.99
	[79]	[31]	[95]	(0.000)
Other Local Tax Revenue Per Capita	205	151	260	3.24
	[206]	[167]	[227]	(0.002)
Median House Price	196271	115328	277214	8.98
	[133721]	[30549]	[147631]	(0.000)
B. City Government Characteristics				
City Manager Form of Government	0.86	0.84	0.89	0.74
	[0.34]	[0.37]	[0.32]	(0.463)
Number of Elected Officials	4.87	4.96	4.79	-1.09
	[0.92]	[0.91]	[0.95]	(0.276)
Home Rule City	0.21	0.21	0.20	-0.21
	[0.41]	[0.41]	[0.40]	(0.836)
C. City Economic Characteristics (1990 Census)				
Population	79146	63256	95036	0.60
	[314992]	[146006]	[421958]	(0.552)
Income Per Capita	18300	11602	24998	8.55
	[11426]	[3703]	[12580]	(0.000)
Percent College Graduate	16	8	24	9.73
	[13]	[6]	[12]	(0.000)
Percent Non-White	26	33	18	-5.19
	[19]	[20]	[14]	(0.000)
D. City Housing Supply Characteristics				
Wharton Housing Regulation Index	0.65	0.50	0.80	2.41
	[0.73]	[0.61]	[0.82]	(0.017)
Land Supply Elasticity	1.03	1.31	0.75	-7.11
	[0.54]	[0.63]	[0.18]	(0.000)
Percentage of Land Supply Constrained	50	38	61	8.16
	[20]	[21]	[10]	(0.000)

N	140	70	70

Notes: Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, Saiz Land Supply, and Wharton Land Use Index data. The unit of observation is city. The sample contains all 140 cities in California located in MSAs. The main entries in columns (1)-(3) are the mean of the indicated variable, with the standard deviation in square brackets. The main entries in column (4) are t-statistics for the test the means in column (2) and column (3) for the variable indicated are the same, with the p-values for the test in parentheses.

TABLE 2: The Effect of House Prices on Tax Revenue and Spending: OLS Estimates

Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	OLS (1)	OLS (2)	OLS (3)
Log(House Prices$_{t-2}$)	0.26*** (0.08)	0.12 (0.08)	0.25*** (0.09)
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1813	1813	1813

Notes: Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, and Dataquick data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (1) by ordinary least squares for the dependent variable indicated. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 3: The Effect of Mortgage Rates on Local House Prices and Tax Revenue: OLS Estimates

Dependent Variable:	Log(Mean House Price)	Log(Mean House Price)
Model:	OLS	OLS
	(1)	(2)
Percentage of Land Supply Constrained X 10 Year Interest Rate (t-1)	-0.06**	--
	(0.02)	
Land Supply Elasticity X 10 Year Interest Rate (t-1)	--	0.02**
		(0.01)
10 Year Interest Rate (t-1)	0.37	0.33
	(0.47)	(0.47)
F-Statistic	6.16	4.04
	[0.014]	[0.046]
Time Fixed Effects	Yes	Yes
City Fixed Effects	Yes	Yes
N	1953	1953

Notes: Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, Saiz Land Supply, and Wharton Land Use Index data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (2) by ordinary least squares for the dependent variable indicated. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 4: The Effect of House Prices on Tax Revenue and Spending: IV Estimates

Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	IV	IV	IV
	(1)	(2)	(3)
Log(House Prices$_{t-2}$)	0.74**	-0.55**	-0.08
	(0.29)	(0.27)	(0.25)
	[0.27, 1.41]	[-1.14,-0.10]	[-0.61, 0.37]
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1673	1673	1673

Notes: Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, and Saiz Land Supply data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (2) by instrumental variables for the dependent variable indicated. The excluded instrument is *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)*. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. Weak instrument consistent confidence intervals are reported in square brackets. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 5: The Effect of House Prices on Tax Revenue and Spending: IV Estimates, House Price Drop Interactions

Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	IV	IV	IV
	(1)	(2)	(3)
Log(House Prices$_{t-2}$)	0.74**	-0.76**	-0.33
	(0.33)	(0.38)	(0.36)
I{Δ House Prices$_{t-2}$ <0} X Log(House Prices$_{t-2}$)	0.00	-0.02	-0.04
	(0.00)	(0.02)	(0.03)
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1673	1673	1673

Notes: : Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, and Saiz Land Supply data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (3) by instrumental variables for the dependent variable indicated. The excluded instruments are *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)* and *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)X I{Δ House Prices$_{t-2}$ <0}*. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 6: The Effect of House Prices on Tax Revenue and Spending: IV Estimates, Home Rule Interactions

Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	IV	IV	IV
	(1)	(2)	(3)
$Log(House Prices_{t-2})$	0.74**	-0.56***	-0.08
	(0.29)	(0.27)	(0.24)
Home Rule X $Log(House Prices_{t-2})$	0.04	-0.27***	-0.20***
	(0.07)	(0.06)	(0.06)
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1673	1673	1673

Notes: : Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, and Saiz Land Supply data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (3) by instrumental variables for the dependent variable indicated. The excluded instruments are *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)* and *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)X Home Rule*. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 7: The Effect of House Prices on Tax Revenue and Spending: IV Estimates, Number of Elected Officials Interactions

Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	IV	IV	IV
	(1)	(2)	(3)
$\text{Log(House Prices}_{t-2})$	0.53*	-0.50*	0.03
	(0.30)	(0.26)	(0.24)
Number of Elected Officials X $\text{Log(House Prices}_{t-2})$	0.05	-0.01	-0.02
	(0.04)	(0.04)	(0.02)
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1673	1673	1673

Notes: : Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, and Saiz Land Supply data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (3) by instrumental variables for the dependent variable indicated. The excluded instruments are *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)* and *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)X Number of Elected Officials*. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

TABLE 8: The Effect of House Prices on Tax Revenue and Spending: IV Estimates, Manager form of Government Interactions

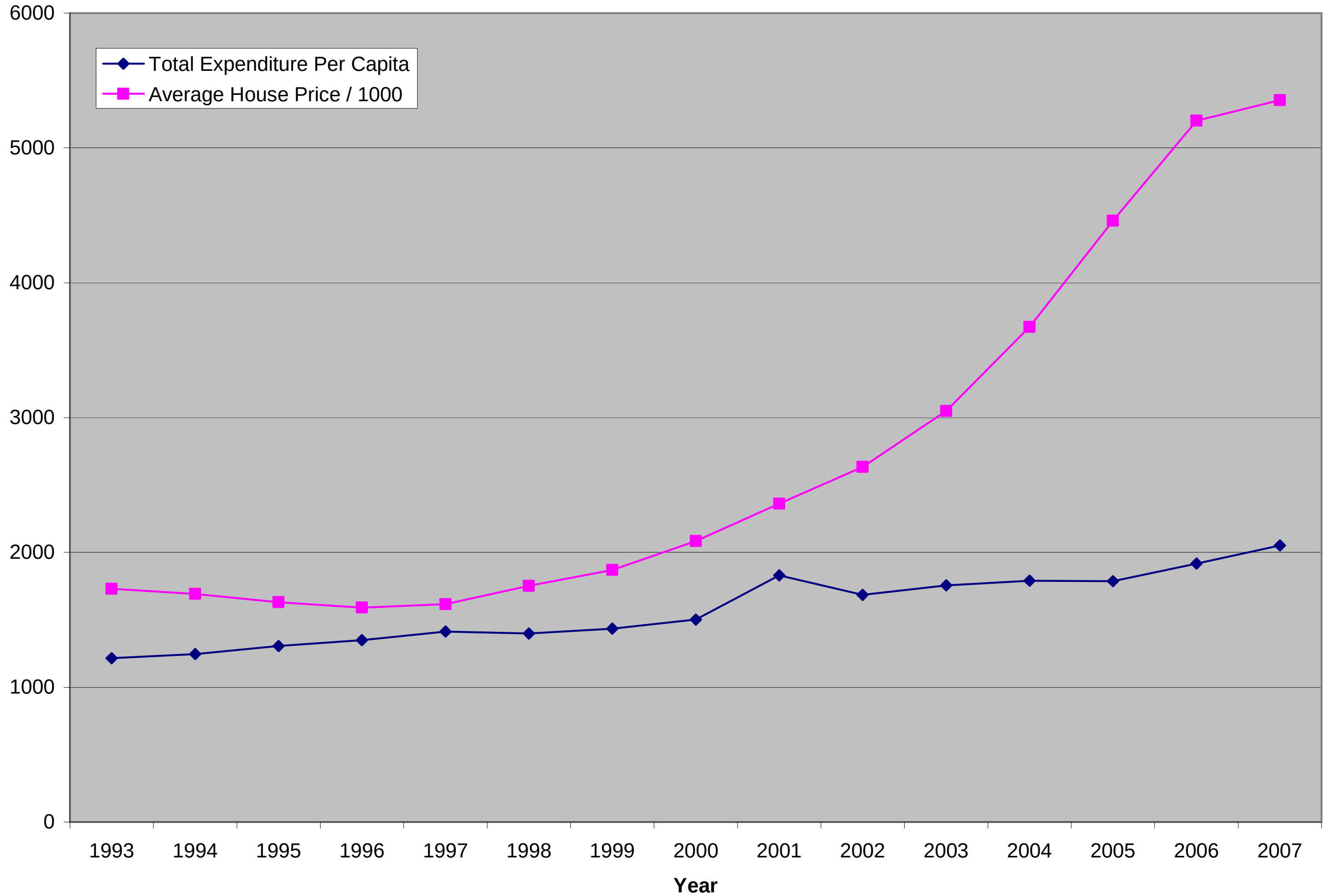
Dependent Variable:	Log(Property Tax Revenue Per Capita)	Log(Other Local Tax Revenue Per Capita)	Log(Total Spending Per Capita)
Model:	IV	IV	IV
	(1)	(2)	(3)
Log(House Prices$_{t-2}$)	0.70**	-0.43*	-0.02
	(0.27)	(0.24)	(0.23)
Manager X Log(House Prices$_{t-2}$)	0.05	-0.19**	-0.08
	(0.09)	(0.08)	(0.07)
City Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	1673	1673	1673

Notes: : Source: Authors' Calculations from CFTR, Census of Governments 1992, 1990 Decennial Census of Population, California Department of Finance, Dataquick, and Saiz Land Supply data. The unit of observation is city-year. The sample contains all 140 cities in California located in MSAs. Each column of the table reports the results from one regression fitting equation (3) by instrumental variables for the dependent variable indicated. The excluded instruments are *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)* and *Percentage of Land Supply Constrained X 10 Year Interest Rate (t-3)X Manager*. The main entries in the columns are the coefficient estimates with the standard errors clustered at the city level reported in parentheses. * indicates statistically significantly different from zero at the 10% level; ** indicates statistically significantly different from zero at the 5% level; *** indicates statistically significantly different from zero at the 1% level.

FIGURE 1: House Prices and City Government Revenue, 1993-2007

Notes: Source: Authors' Calculations from CFTR, 1990 Decennial Census of Population, California Department of Finance, and Dataquick data. Annual observations are the population weighted averages for the 140 cities in CA MSA sample.

FIGURE 2: House Prices and City Government Spending, 1993-2007



Notes: Source: Authors' Calculations from CFTR, 1990 Decennial Census of Population, California Department of Finance, and Dataquick data. Annual observations are the population weighted averages for the 140 cities in CA MSA sample.