Municipal Election Timing and Local Growth Controls

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Abstract

In this paper, I argue that the timing of city council elections plays an important role in shaping municipal land use policy. When elections are held off-cycle (i.e. on a date separate from high-profile national elections) interest groups that oppose new growth are more likely to turn out and vote. This stems from an asymmetry over the costs and benefits of growth: new housing development yields concentrated costs and diffuse benefits, so opposition groups are more likely to mobilize when doing so is costly. Using an extensive dataset on municipal election timing, I demonstrate that cities with off-cycle elections have higher home prices and issue fewer building permits than comparable cities with on-cycle elections. This finding holds both in a cross-sectional matching analysis and a difference-in-difference analysis of cities that shifted their election timing.

The most recent version of this paper is available at:

https://sites.lsa.umich.edu/ornstein/election-timing/

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1 Introduction

Why do governments enact policies that restrict growth? Though this question has long preoccupied political scientists at the national level, it has generated comparatively little study at the level of cities. Molotch (1976) famously describes the city as a “growth machine”, a political entity whose principal aim is to promote business interests through population growth. Given this theoretical frame, it is puzzling that municipal governments expend substantial effort doing precisely the opposite. Particularly in the United States, where municipalities possess broad powers to regulate land use (through zoning plans, permit limits, open space requirements, minimum lot sizes, building height restrictions, etc.), it is common to observe growth control policies that restrict the supply of housing in a jurisdiction below what markets demand (Quigley & Raphael 2005).

The ill effects of such regulations are, at this point, well-documented. Because home prices must rise when increasing demand for housing is not met by increasing supply, the most regulated US cities tend to have higher rents than we would expect from construction costs and wages alone (Glaeser & Gyourko 2003, Quigley & Raphael 2005). In turn, these excess housing costs can have profound effects on the broader economy. For one, they slow economic growth by pricing workers out of cities where they would be most productive. One estimate suggests that easing housing restrictions in the three most productive US cities alone would increase GDP by roughly 9.5% (Hsieh & Moretti 2015). Second, by pricing poorer households out of more affluent areas, growth control policies exacerbate residential segregation, both by race (Rothwell & Massey 2009) and by income (Rothwell & Massey 2010). Such segregation has been shown to affect civic participation (Oliver 1999), public goods provision (Alesina et al. 1999, Trounstine 2015), and even life expectancy (Chetty et al. 2016). Finally, density restrictions in central cities promote suburban sprawl, which increases both commuting costs and carbon emissions (Glaeser & Kahn 2010).

Given these tremendous costs, it is worth investigating the political and institutional factors that lead municipal governments to enact such policies. In this paper, I argue that
the timing of municipal elections is one such factor. When elections are held off-cycle, groups with the greatest stake in municipal politics are more likely to turn out and vote. Because new housing developments impose concentrated costs on some residents, anti-growth groups are more likely to mobilize when the cost of doing so is high. Thus, politicians in cities with off-cycle elections are more likely to cater to these interests, restricting the growth of new housing.

To test this theory empirically, I employ an extensive dataset on municipal elections from California during the past twenty years. In both OLS and matching analysis, I show that cities where elections are held off-cycle have significantly higher home prices and issue fewer new building permits than comparable cities with on-cycle elections. Because this cross-sectional analysis may not eliminate all city-specific unobserved confounders, I also conduct a difference-in-difference analysis. The pattern holds across time as well; cities that switched to on-cycle elections had slower home price growth and issued more building permits between 2002 and 2016 than comparable cities that kept their elections off-cycle.

The paper proceeds as follows. In the next section, I review the literature on election timing, and discuss why off-cycle elections are likely to benefit organized interest groups with a larger stake in municipal politics. In section three, I extend this theory to the context of local growth control policies. I argue that new housing development yields concentrated costs and diffuse benefits, an asymmetry that is likely to mobilize anti-growth coalitions when voting is costly. Section four introduces a brief case study on how election timing influenced politics of land use in Palo Alto, California. Section five describes my dataset and section six introduces the results of my empirical analysis. Section seven concludes.

2 Off-Cycle Elections Empower Special Interests

Although “Election Day” in the United States is officially the Tuesday following the first Monday in November, most elections are not held on that day (Berry & Gersen 2010).
The United States contains tens of thousands of local governments, including roughly 3,000 counties, 19,000 municipalities, 14,000 school districts, and 35,000 special districts (Berry 2009). At this lower level, elections are commonly held off-cycle, a term I will use for any election not held on November of even-numbered years, concurrently with US presidential or Congressional races.

The historical roots of this practice are deep. As Anzia (2012a) documents, several city governments experimented with election timing in the late 19th century as a play for partisan political advantage. In the decades that followed, Progressive reformers adopted off-cycle elections as part of a package of institutions designed to weaken urban political machines. The institution has proven remarkably sticky. Today, roughly 80% of municipalities continue to hold their elections off-cycle.

The most prominent consequence of holding elections off-cycle is lower voter turnout. Because there is some marginal cost to turning out, citizens are more likely to vote when there are multiple concurrent elections on the ballot, particularly high-profile national elections like the presidency. Berry & Gersen (2010) document a 20 percentage point decrease in turnout when municipal elections are held off-cycle. This finding is replicated in quasi-experimental studies as well; local governments that are forced to shift the timing of their elections saw large subsequent changes in voter turnout (Anzia 2012b).

But this decrease in turnout is not uniform. Citizens with a larger stake in an election’s outcome are more likely to show up when doing so is costly. As a result, organized interest groups have an advantage in securing favorable representation during these off-cycle elections. For example, when school district elections are held off-cycle, members of teachers unions are more likely to turn out to vote. In such districts, there is a significant increase in the average teacher’s salary (Anzia 2011, Berry & Gersen 2010). Similarly, because most special districts hold their elections off-cycle, groups that support additional spending are more likely to mobilize. As a result, places with more special districts tend to tax and spend more than those without them (Berry 2008).
In the two examples above, there is a classic collective action problem at work. Some groups receive concentrated benefits from additional government spending (e.g. teachers receive higher salaries; library patrons get better libraries). But the bulk of the population bears very small per capita costs from the necessary increase in taxes or debt. This produces an enthusiasm gap when it comes to turning out supporters (Anzia 2012b). The beneficiaries of additional spending are much more likely to organize and turn out their supporters than those that oppose it.

But how does all this relate to the politics of local land use? To complete my argument, I must demonstrate that housing development produces a similar collective action problem. I turn to this task in the next section.

3 The Costs and Benefits of Urban Growth

New housing development imposes concentrated costs on nearby residents. A larger population can increase traffic congestion and compete for scarce parking spaces. New residents crowd local public amenities like libraries, parks, or beaches. Tall apartment buildings block their neighbors’ sunlight and impede their views. Homeowners, rightly concerned for the value of their largest and most highly-leveraged asset, tend to organize against significant increases in the housing stock (Fischel 2001).

By comparison, the benefits that come from new housing are diffuse and uncertain. Building additional housing stock may put downward pressure on rents, but the marginal benefit that any individual voter reaps from a new housing development is minuscule. In fact, in some cases renters in the immediate vicinity of new development may see an increase in rent (Hankinson 2017), owing to induced demand from gentrification. In principle, new development brings concentrated benefits to one group of people: the new residents. But by definition, these people are not current residents of the municipality, and therefore they are not voters!
Citizens wishing to influence local land use policy are likely to do so through city council elections. Municipal governments in the United States have broad discretion to regulate the land use of private property within their borders, and in practice, exercising that power lies overwhelmingly with elected city councilors. Of 2,729 municipalities surveyed by the Wharton Residential Land Use Regulation Survey (Gyourko et al. 2008), 94% reported that approval for any project that required a rezoning decision required a majority (or supermajority) vote in city council. In addition, 70% of municipalities surveyed require planning commission approval for new building. These committees tend to be appointed rather than elected (there are no instances in my dataset of an elected zoning board or planning commission member), so any group looking to influence those committees through elections would have to do so through mayoral or city council elections.

4 Case Study: Palo Alto’s Measure S

In November 2010, the residents of Palo Alto, California passed Measure S, a referendum shifting its municipal elections on-cycle. Prior to 2010, Palo Alto city council members were elected during odd-numbered years. But following the referendum’s passage, city council elections were moved to coincide with national elections on even-numbered years. Proponents of the change argued that it would boost voter turnout and decrease the cost of administering municipal elections.

The first claim was certainly proven true. As Figure 1 (panel A) illustrates, on average 47% of registered voters turned out to vote for city council in the three elections prior to Measure S. Afterwards, turnout increased dramatically. About 85% of registered voters turned out in 2012 and 2016, and 60% turned out during the congressional midterm in 2014.

But did the composition of the electorate change, to the advantage of pro-development candidates? To explore this question, I turn to the local newspaper (The Palo Alto Observer), which has kept archives of its interviews with each candidate for city council going
Figure 1: Following the switch to on-cycle elections, Palo Alto city council elections saw much higher turnout (A), and more pro-development city councilmembers elected (B). Solid lines denote averages before and after the passage of Measure S (dotted line).

back to 2005. Because housing policy is such a salient issue in the Bay Area, the candidates have typically been asked to state their opinion on local zoning and housing development policies. For every candidate between 2005 and 2016, I manually code whether each candidate’s platform is pro-development (i.e. the candidate expresses willingness to relax height restrictions, fewer regulations on accessory dwelling units, lower density requirements, build housing near transit corridors, etc.) or not. For candidates with no clear position, I assign a 0.5.

How well did pro-development candidates perform in Palo Alto city council elections before and after the shift in election timing? Figure 1 (panel B) provides some suggestive evidence. Prior to Measure S, roughly 25% of the candidates elected to city council were pro-development. That fraction increased to 50% after the city shifted to on-cycle elections. The
most dramatic result was in 2016, when three out of four elected councilmembers expressed pro-development opinions.

Of course, this single case is far from conclusive. There are a number of reasons why more city councilmembers would have expressed pro-development sentiments toward the end of this period (the housing market collapse and its aftermath spring to mind). But it seems likely that the shifting election timing played some role in the election of these pro-development candidates. In the 2016 election, the Democratic party establishment endorsed a slate of pro-development candidates, two of whom were elected. Although the election was nominally nonpartisan, I have no doubt that Democratic party support for some candidates played a role in voters’ decisions, especially given the context of the 2016 presidential election, which mobilized plenty of Democratic partisans in the Bay Area.

5 The Data

5.1 The Election Timing Variable

To generate my measure of municipal election timing, I refer to the California Election Data Archive, an extensive archive of every election held in the state of California since 1996. Subsetting the data so that I only consider elections for mayor and city council (or the equivalent legislative body, like County Supervisor in San Francisco), I then determine whether each election was held on November during an even-numbered year: if yes, I code it on-cycle, if no, of-cycle.

Once that step is complete, I compute for each municipality the fraction of elections between 1996 and 2016 that were held off-cycle. This measure, \( \text{pct.off.cycle} \), is my primary independent variable. In 73% of cases, this variable equals either 0 or 1. It is a rare city that holds some of its city council elections on-cycle and some off-cycle (e.g. San

\[ ^{1}\]I include mayoral elections in the measure as well, because mayors typically vote on the city council and appoint members to municipal zoning and land use committees.
Francisco). Roughly 25% of cities switched the timing of their elections during the survey period. These will prove useful for the difference-in-difference analysis (Section 6.3).

5.2 Other Variables

In the following empirical analysis, I employ three outcome variables. The first is median home price, taken from the American Community Survey (2014, five year estimates). The second is a time series of median sale price per square foot from the real-estate website Zillow, and my final outcome variable is the number of building permits issued each year by each municipality. These data come from the Census Bureau’s Building Permits Survey, conducted annually since 1980.

From the American Community Survey I collect covariate data on population, median income, and demographic makeup for every city in California with a population greater than 20,000. I also employ the measure of city-level developed by Tausanovitch & Warshaw (2014) using multilevel regression and poststratification.

6 Empirical Analysis

My empirical analysis proceeds in three parts. First, I estimate the relationship between off-cycle elections, home prices, and building permits using cross-sectional OLS. As predicted, off-cycle elections are associated with higher home values and fewer new building permits. Second, I perform a matching analysis, comparing cities with off-cycle elections against a matched set of cities that hold their elections on-cycle. This analysis yields a similar result. Finally, to hold unobserved city effects constant, I restrict my focus to those cities that switched their election timing between 1996 and 2016. This difference-in-difference analysis is consistent with the cross-sectional results: cities that switched to on-cycle elections had slower growth in home prices and issued roughly three times as many building permits as those that did not.
Table 1: Estimated OLS coefficients and standard errors, regressing median home prices and building permits on percent off-cycle elections and covariates in a sample of California cities.

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Median Home Value</th>
<th>Log New Building Permits</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Pct. Off-Cycle</td>
<td>107,676***</td>
<td>73,324***</td>
</tr>
<tr>
<td></td>
<td>(22,457)</td>
<td>(13,620)</td>
</tr>
<tr>
<td>Log Population</td>
<td>−24.91</td>
<td>6,846</td>
</tr>
<tr>
<td></td>
<td>(2,998)</td>
<td>(5,811)</td>
</tr>
<tr>
<td>Median Income</td>
<td>6.99***</td>
<td>7.08***</td>
</tr>
<tr>
<td></td>
<td>(0.26)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>Ideology</td>
<td>−297,501***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(31,396)</td>
<td></td>
</tr>
<tr>
<td>Pct. Black</td>
<td>−498,463***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(98,805)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>320,864***</td>
<td>−91,134**</td>
</tr>
<tr>
<td></td>
<td>(12,167)</td>
<td>(41,773)</td>
</tr>
<tr>
<td>Observations</td>
<td>424</td>
<td>424</td>
</tr>
<tr>
<td>R²</td>
<td>0.05</td>
<td>0.67</td>
</tr>
</tbody>
</table>

Note: *p<0.1; **p<0.05; ***p<0.01

6.1 Cross-Sectional Correlations: OLS

To begin, I estimate the a series of linear regression models of the following form:

\[ Y_i = \beta_1 T_i + \beta_2 X_i + \varepsilon_i \]

where \( Y_i \) is either a measure of median home prices or the logarithm of new units permitted by city \( i \) between 2010 and 2016. The variable \( T_i \) is the percentage of elections in city \( i \) held off-cycle, \( X_i \) is a matrix of city-level covariates, and \( \varepsilon_i \) is an iid error term.

As reported in Table 1 (Columns 1-3), the estimated relationship between election timing
Table 2: Matching Analysis: Effect of off-cycle elections and balance statistics.

<table>
<thead>
<tr>
<th>Outcome Variables</th>
<th>Mean, Treatment</th>
<th>Mean, Control</th>
<th>Difference in Means</th>
<th>T-Test p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Median Home Value</td>
<td>471,140</td>
<td>396,774</td>
<td>74,366</td>
<td>0.006</td>
</tr>
<tr>
<td>Log Building Permits</td>
<td>7.29</td>
<td>8.28</td>
<td>-0.99</td>
<td>0.009</td>
</tr>
<tr>
<td>Number of Cities</td>
<td>71</td>
<td>71</td>
<td>-0.99</td>
<td>0.009</td>
</tr>
</tbody>
</table>

Balance Statistics

<table>
<thead>
<tr>
<th>Balance Statistics</th>
<th>Mean, Treatment</th>
<th>Mean, Control</th>
<th>K-S Statistic</th>
<th>K-S Bootstrap p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Median Income</td>
<td>66,184</td>
<td>64,803</td>
<td>0.127</td>
<td>0.586</td>
</tr>
<tr>
<td>Log Population</td>
<td>15.6</td>
<td>15.641</td>
<td>0.098</td>
<td>0.864</td>
</tr>
<tr>
<td>Ideology</td>
<td>-0.172</td>
<td>-0.148</td>
<td>0.141</td>
<td>0.432</td>
</tr>
<tr>
<td>% Black</td>
<td>0.043</td>
<td>0.041</td>
<td>0.155</td>
<td>0.29</td>
</tr>
</tbody>
</table>

and home prices is positive across all specifications of the model. The magnitude of the effect is striking: the estimate reported in Column (3) suggests that home prices are roughly $40,000 higher on average in cities with off-cycle elections. A similar pattern shows up in the building permit regressions (Columns 4-6). Off-cycle cities cities issued roughly half as many building permits between 2010 and 2016 as comparable cities with on-cycle elections.

6.2 Matching Analysis

To complement the OLS estimation above, I also conduct a matching analysis (Rubin 1973). This estimation strategy compares treated observations (cities with off-cycle elections) to a matched sample of control observations (cities that hold elections on-cycle). The objective of the matching algorithm is to ensure that both samples, while differing on treatment condition, are on average balanced across potential confounding variables. As before, I include median income, log population, mean ideology, and percent black as covariates.\(^2\)

\(^2\)In all specifications, I identify the matched control group using Mahalonobis distance, courtesy of the Matching package in R (Sekhon 2011).
The two groups are well-balanced on the matching covariates, as indicated by the Kolmogorov-Smirnoff statistics in the second half of Table 2. They also seem to be geographically well-balanced, as illustrated in Figure 2. Both groups are concentrated primarily in the Los Angeles or Bay Area metropolitan areas. For each outcome variable, I compute the average treatment effect on the treated units (ATT), reported in Table 2. These estimates are similar to those from the OLS: the median home value in treated cities is roughly $74,000 higher than in control cities, and they issued half as many building permits.

### 6.3 Difference-in-Difference

Matching ensures that the treatment and control groups are balanced on observed covariates, but there may yet be unobserved city-level characteristics affecting housing policy. To adjust
for these unobserved covariates, we will now investigate within-city variation through a difference-in-difference analysis.

To do so, I compare the growth in home prices (using a time series from Zillow) between cities that shifted their election timing from off-cycle to on-cycle, and those cities where elections remained off-cycle the entire period. As before, I create a matched control group, balancing on median income, population, ideology, and percent black.\(^3\) I perform a similar analysis for the growth of newly permitted housing stock.

Figures 3 and 4 illustrate the results. Both groups begin with roughly the same average sale price per square foot (only a $24 difference). But home prices grow much more slowly in

\(^3\)This matching is not strictly necessary for a difference-in-difference analysis as long as one assumes that the outcomes in both groups follow “parallel trends”. However, I find the parallel trends assumption more plausible after matching on observed covariates, so one could consider this test even more conservative than a standard difference-in-difference.
Figure 4: Compared to cities that kept their elections off-cycle, cities that shifted to on-cycle elections issued building permits for roughly four times as many new housing units between 1996 and 2015.

The treatment group, and by 2015, the difference is nearly $100. This coincides with a large difference in the number of new building permits issued between the treatment and control group. Collectively, the control group permitted roughly 50,000 new housing units between 1996 and 2016, while the treatment group issued nearly 200,000 during that same period.

In Table 3, I report the estimates, balance statistics, and measures of uncertainty. Median home value per square foot grew, on average, by $50 less in the cities that moved their elections on-cycle. And those treated cities issued roughly two-and-a-half times as many permits as the control group between 2002 and 2016 (about 2,500 new units per city on average).
Table 3: Difference-in-difference, comparing cities that switched to on-cycle elections (treatment) and those that remained off-cycle (control).

<table>
<thead>
<tr>
<th>Outcome Variables</th>
<th>Mean, Treatment</th>
<th>Mean, Control</th>
<th>Difference in Means</th>
<th>T-Test p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>∆ Median Value per Sq. Ft. (2002-2016)</td>
<td>99.3</td>
<td>150.6</td>
<td>-51.3</td>
<td>0.023</td>
</tr>
<tr>
<td>New Units Permitted (2002-2016)</td>
<td>4467</td>
<td>1919</td>
<td>2548</td>
<td>0.015</td>
</tr>
<tr>
<td>Number of Cities</td>
<td>31</td>
<td>31</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Balance Statistics</th>
<th>Mean, Treatment</th>
<th>Mean, Control</th>
<th>K-S Statistic</th>
<th>K-S Bootstrap p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Median Income</td>
<td>70,564</td>
<td>68,884</td>
<td>0.193</td>
<td>0.576</td>
</tr>
<tr>
<td>Population (2000)</td>
<td>58,195</td>
<td>62,449</td>
<td>0.193</td>
<td>0.568</td>
</tr>
<tr>
<td>Ideology</td>
<td>-0.052</td>
<td>-0.048</td>
<td>0.129</td>
<td>0.936</td>
</tr>
<tr>
<td>% Black</td>
<td>0.055</td>
<td>0.054</td>
<td>0.096</td>
<td>0.99</td>
</tr>
</tbody>
</table>

7 Conclusion

This study has several limitations that could be improved with future work. First, the empirical analysis is restricted to California, due to the lack of a comprehensive dataset on municipal election timing in other states. There is reason to suspect that California is not a representative case of what we would find in other contexts: housing policy is a highly salient political issue on the west coast, owing to the rapid increase in home prices in recent years. More data from outside California would help increase confidence in the findings presented here.

Secondly, I cannot definitively show that the observed increased in median home values is due exclusively to regulation. It is possible that the relationship can be explained by some other policy enacted by municipal officials (say, an increased investment in public goods, which is capitalized into home values). The analysis of building permits helps alleviate these issues.

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4 I am in the process of compiling a larger dataset from other states to correct this.
concerns, it remains difficult to determine what fraction of the increase in home prices is attributable to supply restrictions.

Finally, because election timing is not randomly assigned, it remains inconclusive whether off-cycle elections cause increased home prices. It is possible that some cities have stronger homeowner lobbies, which affects both land use policy and the choice of election timing. Of course, as Anzia (2011) notes, the fact that interest groups would expend limited lobbying resources to influence the timing of elections is itself evidence that off-cycle elections are favorable to those interests. (Or, at least, citizens believe they are.) Nevertheless, it would be nice to have more conclusive evidence of a causal link. An interesting avenue for future research would be to identify cities where election timing is somehow randomly or quasi-randomly assigned (e.g. by state-level mandate).

Despite these limitations, the evidence presented here provides an intriguing glimpse at yet another consequence of election timing. If restrictive land use policy is partly the product of organized interests mobilizing during low-turnout elections, then it raises fundamental questions about the nature of representation in municipal government. And it suggests that a relatively simple institutional reform could yield very large welfare gains.

References


