Election Timing and the Politics of Urban Growth*

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August 14, 2019

Abstract

In this paper, I show that the timing of municipal elections plays an important role in shaping urban land use policy. Combining a national voter file with property tax records, I find that the share of homeowners voting in off-cycle municipal elections is significantly higher than in presidential elections. Because homeowners are more likely to support restrictive zoning, this gap in political participation has downstream effects on policy as well. Using an extensive dataset from California cities, I demonstrate that referenda permitting new housing are more likely to fail when held off-cycle, and that cities with off-cycle elections issue fewer new residential building permits and have higher median home prices 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://joeornstein.github.io/ElectionTiming.html`

*Many thanks to Liz Gerber, Rob Franzese, Clayton Nall, Brian Min, and seminar participants at the University of Michigan and APSA Pre-Conference on Local Political Economy for their helpful comments and fixes. Particular thanks to Jeethi Nair for her insightful and diligent research assistance.

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1 Introduction

In May 2013, the city council of Ann Arbor, Michigan met to discuss the construction of a new high-rise apartment building in the downtown core. Residents packed the council chamber for two hours of debate, voicing concerns that the 150-foot tall building would overshadow the neighborhood's nearby historic homes. At the end of deliberations, the council narrowly approved the construction, by a 6-5 margin.

"Audience members jeered and literally hissed at council members." reported the *Ann Arbor News*, storming out to shouts of "Shame on you!" and "Disgusting!" (Stanton 2013).

Land use policy is among the most contentious issues in local politics, and municipal governments wield considerable power in determining the rate of population growth within their jurisdictions. But I cite this particular episode to highlight a curious pattern that emerged from the city council vote. At the time, Ann Arbor held its city council elections every year, electing half of the council in odd-numbered years, and half in even-numbered years. The vote on the new apartment building split the city council nearly perfectly by election timing. Of the councilmembers elected in even years, all but one voted to approve the construction. Of those elected in odd years, all but one voted to reject it.[1]

In this paper, I argue that the pattern we observe here is not mere coincidence, and that the timing of municipal elections has significant, observable consequences for land use policy and the growth of cities. When elections are held off-cycle (i.e. on a date separate from high profile elections like presidential or congressional races), citizens that oppose new housing development are more likely to turn out to vote than supporters. These citizens, in turn, elect councilmembers that are more willing to use municipal zoning authority to limit urban growth.

Although it may seem like a parochial issue, municipal land use policy has an profound

[1]Several months later, the lone odd-year city councilmember who voted to approve construction was up for re-election. She was soundly defeated, by nearly 30 percentage points.

impact on the broader economy. The most tightly 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 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 aggregate GDP by roughly 9.5% (Hsieh & Moretti 2015).

In addition, by pricing poorer households out of more affluent areas, restrictive land use policies exacerbate residential segregation, both by race (Trounstine 2018) and by income (Rothwell & Massey 2010). Such segregation has been shown to affect civic participation (Oliver 1999), public goods provision (Trounstine 2015), and even life expectancy (Chetty et al. 2016). Restrictions on new residential construction are also largely responsible for the recent decline in regional income convergence (Ganong & Shoag 2017), as Americans from poor regions are less able to move to opportunity in growing metropolitan areas. Finally, density restrictions in central cities promote suburban sprawl, which increases both commuting costs and carbon emissions (Glaeser & Kahn 2010).

Given these costs, why do citizens that oppose growth so often get their way in municipal politics, at the expense of citizens that would benefit from new housing construction? This fact is particularly puzzling in light of much of the foundational scholarship in American urban politics. Molotch (1976) famously describes the city as a "growth machine", a political entity whose principal aim is to promote business interests through population growth. Peterson (1981) makes a similar argument: because labor and capital are mobile across municipal boundaries, city governments are poorly suited to enact redistributive policy, and are instead most likely to pursue developmental policies that grow their property tax base. And yet, in the late 20th and early 21st centuries, many city governments have abandoned this growth machine model, and have instead severely curtailed new housing development through stringent zoning regulations.

In this paper, I argue that election timing provides one explanation for the stringency of municipal zoning regulation. Because off-cycle municipal elections do not coincide with federal or state elections, fewer residents turn out to vote, and those that do are peculiar in many ways. By combining a national voter file with tens of millions of property tax records, I find that voters in off-cycle municipal elections are significantly more likely to be homeowners than voters in presidential elections, and are on average 8 years older. Because these groups of voters tend to view new residential development more skeptically, this gap in political participation is likely to affect the stringency of zoning policy as well.

To test the effect of election timing on policy, I compile an extensive dataset on municipal elections, land use referenda, home prices, and building permits from California cities over the past twenty years. In both OLS and matching analysis, I show that when elections are held off-cycle, citizens are less likely to pass referenda permitting dense housing development, city governments issue fewer new building permits, and median home vales are significantly higher. 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 subsequently issued more new building permits and saw slower home price growth between 1996 and 2018 than comparable cities that kept their elections off-cycle.

The paper proceeds as follows. In the next section, I briefly sketch the history of municipal zoning in the United States, and discuss the role that city councils play in its implementation. In section three, I review the literature on election timing, and discuss why groups that oppose new residential development are likely to be overrepresented in off-cycle elections. Section four introduces my dataset, and describes the procedure I implemented to merge voter file data with tax records. I discuss the results of my empirical analyses in section five. Section six concludes.

2 Background: Municipal Zoning

New York City adopted the first comprehensive zoning code in 1916. Responding to fears that skyscrapers would shroud the island of Manhattan in perpetual shadow – and diminish the value of property on Fifth Avenue – city planners drew up a map of the city divided into zones. Within each zone, the city designated maximum building heights and permitted land uses (Fischel 2015). Despite early objections that municipal zoning violated the Fifth Amendment's prohibition on seizure of private property without due process, the Supreme Court ultimately upheld the constitutionality of these ordinances in 1926's *Ambler Realty v. Village of Euclid* (Wolf 2008). Since that time, municipal governments have been granted broad discretion to regulate land use within their borders. Today, urban land use policy is determined by a patchwork of over 19,000 municipalities, comprising tens of thousands of local legislators, zoning board members, and city planners.

These regulations take many forms. The most common is to specify permitted land use for each parcel (e.g. residential, commercial, industrial). This type of zoning ("Euclidean") is intended to separate some activities from others – e.g. keeping industrial pollutants away from shopping areas, or prohibiting commercial uses from sprouting up in quiet residential neighborhoods.

In addition to regulating the type of land use, zoning also typically regulates the *intensity* of land use. For example, zoning ordinances will often specify a maximum residential density that is allowed within each zone. Other ordinances might mandate a percentage of every lot area that must be dedicated to open space, or a minimum distance that buildings must be set back from the street. Another popular restriction is the maximum floor area ratio (FAR), which limits the total floor area of buildings relative to the size of the lot on which they sit. In practice, these regulations all but ensure that large swaths of US cities are set aside for single-family homes, even when a more intensive land use (townhouses, apartment buildings) would be more appropriate given demand.

Other land use ordinances that are seemingly unrelated to housing can nevertheless limit the number of housing units built in a city. Take, for instance, the near-ubiquitous requirement that developers set aside parking for each new building they construct. Even in cities without formal zoning codes, these requirements can be onerous. For example, the city of Houston mandates that for each studio apartment, developers must set aside 1.25 parking spaces (Lewyn 2005). Not only does all that mandated parking take up land that could be used for housing, but abundant, inexpensive parking further incentivizes urban sprawl by reducing the cost of automobile commutes (Shoup 1999).

Over time these regulations have accumulated in such a way that building new, affordable housing has become prohibitive in many metropolitan areas. In the century since New York City's zoning code was first implemented, the length of the text has ballooned from 14 pages to 4,126 pages. It has been estimated that roughly 40% of Manhattan's housing stock would be illegal to build today (Bui et al. 2016).[2]

How is municipal land use policy determined? In practice, much of the regulatory authority lies with the elected city council. In nearly every US municipality, the city council is responsible for adopting and amending the city's comprehensive plan. Of 2,729 municipalities surveyed by the Wharton Residential Land Use Regulation Survey (Gyourko et al. 2008), 94% reported that rezoning decisions require a majority (or supermajority) vote in city council. In addition, 70% of municipalities surveyed require planning commission approval for any 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 the composition of those committees would have to do so through mayoral or city council elections.

Who shows up to city council elections? That depends, in part, on when they are held.

[2] Although New York City as a whole is twice as populous today as it was in 1910, the population of Manhattan itself peaked in the 1910 Census, just before the introduction of zoning.

3 Off-Cycle Elections Empower Special Interests

Although "Election Day" in the United States is officially the Tuesday following the first Monday in November, most US elections are not held on that day (Berry & Gersen 2010). The United States comprises 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*, on a date separate from presidential, congressional, or gubernatorial elections.

The historical roots of this practice are deep. As Anzia (2012*a*) 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, the Progressive movement advocated off-cycle elections as part of a package of reforms designed to weaken urban political machines. The institution has proven remarkably sticky. Today, roughly 80% of US municipalities continue to hold their elections off-cycle (Anzia 2013).

The most prominent consequence of holding elections off-cycle is lower voter turnout. Because voting entails a non-trivial time cost, 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 California municipal elections are held off-cycle. This finding is replicated in quasi-experimental studies as well; local governments that were compelled to shift the timing of their elections saw large subsequent changes in voter turnout (Anzia 2012*b*, Garmann 2016).

But this decrease in turnout is not uniform. Citizens that have a larger stake in local politics are more likely to show up to local-specific elections. For example, when school district elections are held off-cycle, members of teachers unions are more likely to turn out to vote than those with smaller stakes in school district policymaking. In such districts, there is a significant increase in the average teacher's salary (Anzia 2011, Berry & Gersen 2010). Similarly, because most special districts (e.g. water districts, library districts) hold their elections off-cycle, groups that benefit from the district's services are more likely to show up to vote than those that do not, resulting in higher levels of taxes and spending (Berry 2008).

In the two examples above, we see the classic logic of collective action at work (Olson 1965). One group receives concentrated benefits from additional government spending (e.g. teachers receive higher salaries; library patrons get better libraries) while the costs are diffuse; the population at large 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 2012*b*). The beneficiaries of additional spending are much more likely to organize and turn out their supporters than those that oppose it.

For whom does restrictive municipal zoning policy yield concentrated benefits? The most prominent such group is homeowners. In his influential book, *The Homevoter Hypothesis*, Fischel (2001) describes how resident homeowners came to dominate American municipal politics during the late 20th century. Because their financial portfolio largely consists of a single, highly-leveraged, undiversified, immobile asset, homeowners develop a (wholly justified) concern for maintaining home values in their community. And municipal government policy is an important determinant of home values. Studies have repeatedly demonstrated that home prices respond to factors like local tax policy (Hamilton 1976), public school quality (Black 1999), transportation infrastructure (Hess & Almeida 2007), placement of public parks (Troy & Grove 2008), and crime risk (Linden & Rockoff 2008, Pope & Pope 2012).

But arguably it is zoning policy, by regulating the overall supply of housing, that exerts the most direct influence on home values. Homeowners tend to support greater restrictions on new construction than renters. Marble & Nall (2017) conduct a series of survey experiments to assess urban residents' views towards new housing development. In these surveys, homeowners consistently report stronger opposition to new housing construction than renters. This effect is stronger than that of any other demographic variable or experimental manipulation. Hankinson (2017) finds a similar result. Although there is some support for building restrictions among renters in gentrifying neighborhoods, homeowners consistently support these policies more strongly than renters.

All of this suggests that homeowners will be relatively more likely than renters to turnout to municipal-specific elections, and vote for candidates that share their concern for maintaining home values and limiting new construction. Existing research supports this hypothesis. Dipasquale & Glaeser (1999), for example, find that homeowners are 25 percentage points more likely to report voting in local elections. Einstein et al. (2017) find that homeowners are more than twice as likely to speak at local zoning board meetings than renters. In municipalities with such a large gap in political participation, municipal governments are likely to be more responsive to homeowners' concerns. But when municipal elections are held on-cycle, this turnout discrepancy may shrink, as renters turn out for the more high-profile elections.[3]

Homeownership is not the only characteristic that influences residents to vote in municipal elections. For instance, Kogan et al. (2017) finds that off-cycle voters are much older on average (roughly 10-20 percentage points more senior citizens than the presidential electorate). If older residents prefer slow growth, then this could be another channel through which election timing affects the incentives of city councilmembers. Ortalo-Magne & Prat (2014) provide a mechanism for this preference in their overlapping-generations model on the political economy of urban growth. Older agents are more likely to oppose new construction because they have made greater investments in real estate over the course of their lives, and are less able to recoup a loss in the value of that capital.

Taken together, these mechanisms suggest that off-cycle electorates will be, on average, more opposed to new housing development, and this is likely to affect the choices of their elected city officials.

[3]De Benedictis-Kessner (2017) documents an increase in mayoral incumbency advantage when municipal elections are held on-cycle, suggesting that on-cycle voters – drawn to the polls for other reasons – are less informed on average about municipal politics.

4 Data

The data I employ in my empirical analyses are drawn from a number of sources. In this section, I describe these data and the procedures I used to link them.

4.1 Voter Files and Tax Records

My data on voter demographics and election turnout are drawn from a nationwide voter file from L2. These data contain over 190 million unique voter records, compiled from every state and county-level voter registry in the country. They include names, addresses, dates of birth, and each registered voter's historical turnout data for presidential, midterm, primary, municipal, and special elections. By identifying each voter in the L2 dataset that resides within a city, I can determine the composition of the electorate for each on-cycle and off-cycle election held within that jurisdiction over the past several decades.

Because the voter file only includes each state's currently registered voters, I am more likely to be missing turnout data from voters in earlier elections. If renters are more likely than homeowners to move between states each year and be scrubbed from the voter rolls, then this will bias my estimates of homeowner over-representation upwards. To compensate for this problem, I only analyze recent off-cycle elections, and I drop any cities that have not held off-cycle elections since 2015. In what follows, I restrict my attention to comparing the most recent presidential election (November 2016) and the most recent off-cycle election held in each municipality.

I link these voting records with a second dataset of parcel-level deed and tax records from CoreLogic, a real estate analytics firm. These data are collected from over 3,100 county tax assessor's offices, and include information on property characteristics, assessed value, owners' names, and geolocation. Because each voter in the L2 dataset has an associated residential address, I am able to link the two datasets by street address, zip code, and unit number.

To determine whether each housing unit is owner-occupied, I compare each property address with the owner's listed mailing address. If they match, then I classify the property as owner-occupied. This is a more reliable method than attempting to match the names of owners and voters, because it correctly classifies homeownership in cases where names are misspelled, or only one member of the household is listed as the homeowner, or the home is owned through an LLC. To the extent that this procedure biases my empirical analysis, it is likely to *overestimate* the number of renters that participate in municipal politics, because it will code as renters some homeowners that own more than one home, or those who list their mailing address as a P.O. Box.

For voters where I cannot find a matching property record – or cannot classify the owner-occupied status of the property – I must impute whether they are a renter or homeowner. To do so, I fit a logistic regression for each county, predicting homeownership based on Census Tract, age, and whether the voter's address includes an apartment number. For my empirical analyses estimating the percentage of homeowners who vote in municipal elections, I weight by these predicted probabilities. In the Supplementary Materials, I show that the results are robust to alternative, more conservative coding choices, including classifying all voters that failed to match as renters.

4.2 The Election Timing Variable

Owing to its extensive records on municipal election timing going back two decades, my empirical analyses on the policy effects of election timing focus entirely from the state of California. So it is worth noting the ways in which California cities differ from their counterparts in the rest of the United States. First, California has experienced consistent, rapid population growth throughout its history as a state. Since 1840, there has not been a single decade during which its population grew by less than 10%.[4] This is significant, because it has

[4]https://www.census.gov/population/www/censusdata/files/table-16.pdf

required a continual expansion of the housing supply to accommodate new migrants. This trend has largely been reflected at the city level as well. Unlike other areas of the country, where cities have experienced protracted population decline, 78% of California's cities are currently at their population peak, and only six cities are below 90% of their population peak (author's calculations). As a result, there is no overhang of housing supply in shrinking cities to drive down home prices (Glaeser & Gyourko 2005). In nearly every city, new construction is required to keep up with expanding demand.

Second, California cities face unique situation regarding local public finance, owing to a 1976 measure called Proposition 13. Passed by referendum as part of the broader "tax revolt", Prop 13 places strict limits on municipal governments' ability to raise property taxes. All property tax rates are statutorally capped at 1% of assessed property value, and assessments can only increase at a maximum of 2% per year. As a result, the effective tax rate paid in high-demand real estate markets is substantially below 1% (Ferreira 2010). The effect that Proposition 13 has on homeowner behavior is well-researched: people are simply less likely to move. Because purchasing a new home results in a reassessment by the local government, many residents are "locked-in" to their homes, paying favorable property tax rates (Ferreira 2010). There is less scholarly agreement, however, on how Prop 13 affects municipal land use policies. Some scholars suggest that Prop 13 makes new residential development less attractive, because their property taxes will be insufficient to pay the cost of new public services (Quigley & Rosenthal 2005). However, because new housing is assessed at market value rather the statutorially constrained assessments of older housing stock, this could *increase* the incentive to build new housing, particularly in areas that have undergone rapid home price growth.

Finally, California consists of two very distinct regions. The coastal cities are land-constrained, wealthy, liberal, and most have recovered easily from the housing price collapse in 2007. The inland cities are more land-abundant, conservative, and have had greater difficulty recovering from the Great Recession. In the empirical analysis, I conduct a matching

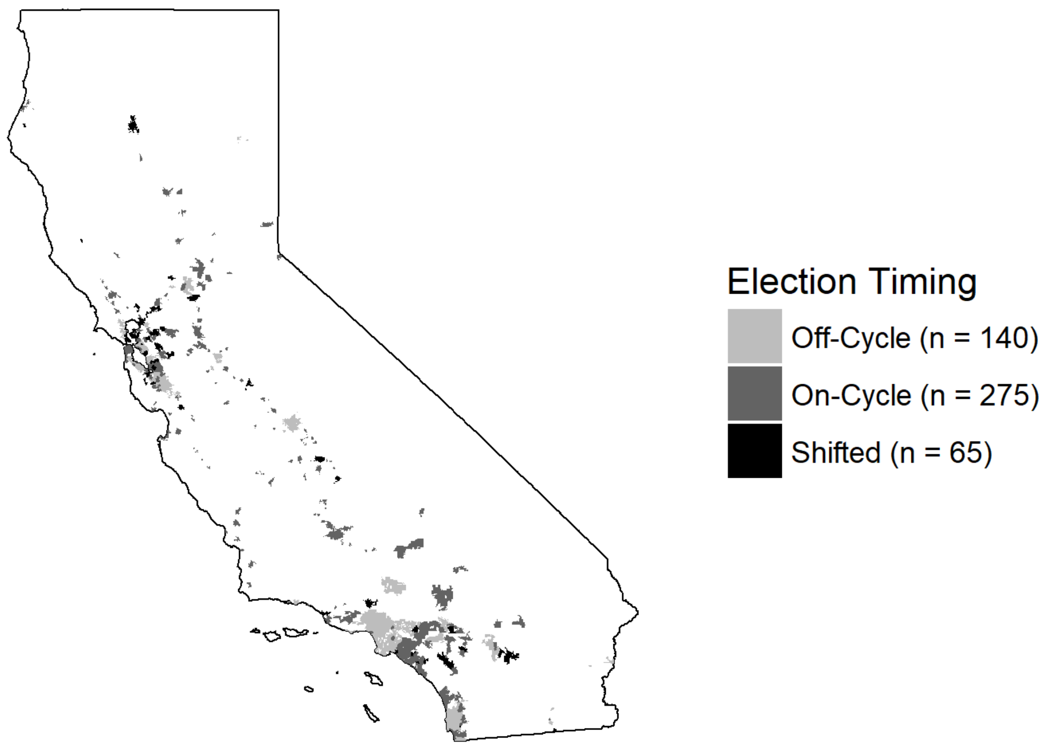

Figure 1: Map of municipalities in the dataset. Shading denotes whether municipal elections (1996-2016) were held off-cycle, on-cycle, or whether their timing shifted over the past two decades.

analysis to ensure that we are comparing cities within, rather than across, these regions.

To determine the exact timing of California municipal elections and ballot initiatives, I reference the California Election Data Archive (CEDA), an extensive database of every election held in the state of California since 1996.[5] 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, off-cycle.

Once that step is complete, I compute for each municipality the fraction of elections between 1996 and 2016 that were held off-cycle. The measure reveals a substantial amount

[5]Available at http://www.csus.edu/isr/projects/ceda.html.

of heterogeneity in election timing. 25% of the cities in my sample held all of their elections off-cycle during this period, while 41% held their elections on-cycle. Roughly 13% of cities switched the timing of their elections during the survey period, a fact that will prove useful for the difference-in-difference analysis (Section 5.5). Figure 1 maps the cities in my dataset, shaded by election timing.

4.3 Ballot Initiatives

Over the past two decades, California has stood out among US states for its unique reliance on the ballot initiative to shape land use policy. Slow-growth citizen groups frequently resort to direct democracy to constrain the ability of city councils to permit new development (Gerber & Phillips 2004). There are several popular tools in this arsenal. One is the Urban Growth Boundary (UGB), a requirement that all new residential development take place within a specified boundary, beyond which the municipality will not extend city services (Gerber 2005). As of writing, at least 85 municipalities in California have adopted some form of UGB via ballot measure. Another tool is the initiative requirement, a rule that prohibits certain types of development (particularly multifamily housing) unless expressly approved by ballot initiative. Finally, California voters will often use ballot measures to directly shape the city's zoning code: imposing restrictions on building heights, setbacks, parking requirements, environmental review, traffic impacts, etc. As a result, there is now a large set of data on how voters react when asked to weigh in on municipal land use decisions.

For each ballot measure, the CEDA database includes the municipality, election date, ballot question, and number of voters that voted for and against the measure. Using the text of the ballot question, I manually code whether the measure restricted or approved new residential development, removing initiatives that did not pertain to land use or only applied to nonresidential development. I categorize each measure based on the type of housing development (infill or greenfield), and the type of restriction (UGB, initiative requirement, height restriction, etc.). Using this coding scheme, I identify 59 initiatives that were placed on the ballot between 1996 and 2016 to approve or prohibit new infill housing development.

One important caveat when interpreting these data is that the existence of popular initiatives on land use is *itself* a development control. Municipalities that require new development to face the voters before it can go forward are placing an additional veto player into the permitting process. As such, the types of housing development that are proposed tend to be significantly watered down, and often come paired with developer-funded public goods (Gerber 2005). For example, many of the ballot initiatives in the CEDA dataset allow new housing, but on the condition that a portion of the land area be preserved as permanent open space. Nevertheless, I code these initiatives as "pro-housing" because they expand the housing stock relative to current law.

4.4 Building Permits and Home Prices

To assess the effects of election timing on zoning policy, I employ two outcome variables. The first is a direct measure of regulatory stringency, the number of new building permits issued each year by the municipal government. These data come from the Census Bureau's Building Permits Survey, a count of all new housing units approved by each permit-approving jurisdiction in the United States, conducted annually since 1980. The other outcome is a measure of median home prices. Although not a direct measure of land use regulation, home prices provide a useful proxy for the elasticity of housing supply in an area, after accounting for demand-side factors like median income and urban amenities.[6] In all of the following analyses, I use a measure of median sale price per square foot from the real-estate website Zillow.[7]

[6] See Saiz (2010) for a more thorough explanation on how supply elasticity affects home price levels, and Glaeser et al. (2005) for an example of an empirical analysis using home prices relative to construction costs to infer the stringency of land use regulation.

[7] https://www.zillow.com/research/data/

4.5 Developable Land

Municipalities with an abundance of nearby developable land are likely to have an easier time expanding their housing supply than land-constrained cities, because it merely requires building out, rather than building up (Saiz 2010). To account for this potential confounder, I generate a measure of nearby developable land for each municipality in my dataset. This entails a three-step process. First, I use the National Land Cover Dataset (NLCD) to identify the parcels of land within a 20km radius of the city center that are undeveloped. I then identify which of those parcels are *developable*, following criteria from Saiz (2010). I exclude any land that is classified as wetlands in the NLCD, as well as any terrain that is too steep to build on (grade greater than 15 percent), which I compute from USGS Digital Elevation Model (90 sq. meter grid cells).[8] Finally, I compute the fraction of land within 20km of the city center that matches these criteria (undeveloped, not-too-steep, and not wetlands). The result is my `percent.developable` variable.

4.6 Other Covariates

Because many municipalities cite cost savings as a motivation for changing their election timing, omitting data on local fiscal conditions may bias my estimates. Cities with large per capita debt burdens may be more likely to switch to on-cycle elections, and also to pursue tax-base enhancing real estate developments. To account for this possibility, I collect data on outstanding debt per capita, expenditures per capita, and taxes per capita from the US Census of Governments.[9] From the American Community Survey I collect covariate data on population, median income, educational attainment, and demographic composition for every city in California with a population greater than 10,000.

[8] Data available from the US Geological Survey, accessed through the FedData package in `R` (Bocinsky 2017).

[9] Available at http://www2.census.gov/pub/outgoing/govs/special60/. The filename is "IndFin1967-2012.zip".

Hedonic models of urban quality of life (Roback 1982) suggest that amenities like pleasant climate are likely to affect median home values. So I also compute average January and July temperatures for each municipality from the high-resolution WorldClim dataset (Hijmans et al. 2005). These climate variables are unlikely to vary much within metropolitan areas, but they do predict cross-regional differences in new building and home prices.

I also employ a measure of city-level ideology developed by Tausanovitch & Warshaw (2014) using multilevel regression and poststratification. If liberal cities – in an effort to turn out Democratic voters – are more likely to hold their elections on-cycle, and liberal cities also have more restrictive zoning policies – as Kahn (2011) documents in California – then omitting local-level ideology could bias my estimates. Note that this estimate is only available for cities with population greater than 20,000.

Home prices are also sensitive to the quality of local public goods. In particular, the performance of nearby public schools is strongly capitalized into property values, as border discontinuity studies reveal (Black 1999). A review of the literature suggests that one standard deviation increase in test scores is associated with home prices that are four percent higher (Nguyen-Hoang & Yinger 2011). To account for this effect, I include school district-level data on the Academic Performance Index, a measure computed annually by the California Department of Education to track school district performance and hold local officials accountable. Payson (2017) documents the importance of this measure in local school board elections; see that paper for a more detailed description of the measure. For each city in my dataset, I assign an API score based on the school district with the most territorial overlap.[10]

[10] Where multiple school districts overlap with a municipality, I assign the API scores for the unified school district, and use scores from secondary or elementary districts only if there is no unified school district. Data files available at `https://www.cde.ca.gov/ta/ac/ap/apidatafiles.asp`.

5 Results

My empirical analysis proceeds in four parts. First, I compare the characteristics of voters in off-cycle elections against those in presidential elections. As expected, off-cycle electorates are more likely to be older and own homes, but there is little evidence for systematic differences in either property value or race. Next, I analyze the results from several California land use referenda, and find that citizens are more likely to vote against new infill housing development when ballot initiatives are held off-cycle. Next, 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. I also estimate this cross-sectional relationship through matching, 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 2018. 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.

5.1 Off-Cycle Electorate Characteristics

By combining the voter file data from L2 with the deed-level property data from CoreLogic, I am able to identify the share of voters own homes in both on-cycle and off-cycle municipal elections. Comparing each city's most recent off-cycle election with the most recent presidential election, I then estimate the effect of off-cycle election timing on the electorate's homeownership rate. These results are illustrated in Figure 2.

In presidential elections (top-left panel), homeowners make up a disproportionate share of each city's electorate. Despite the fact that several cities are majority-renter, there is only one city where more renters turned out to vote than homeowners, and the average homeowner

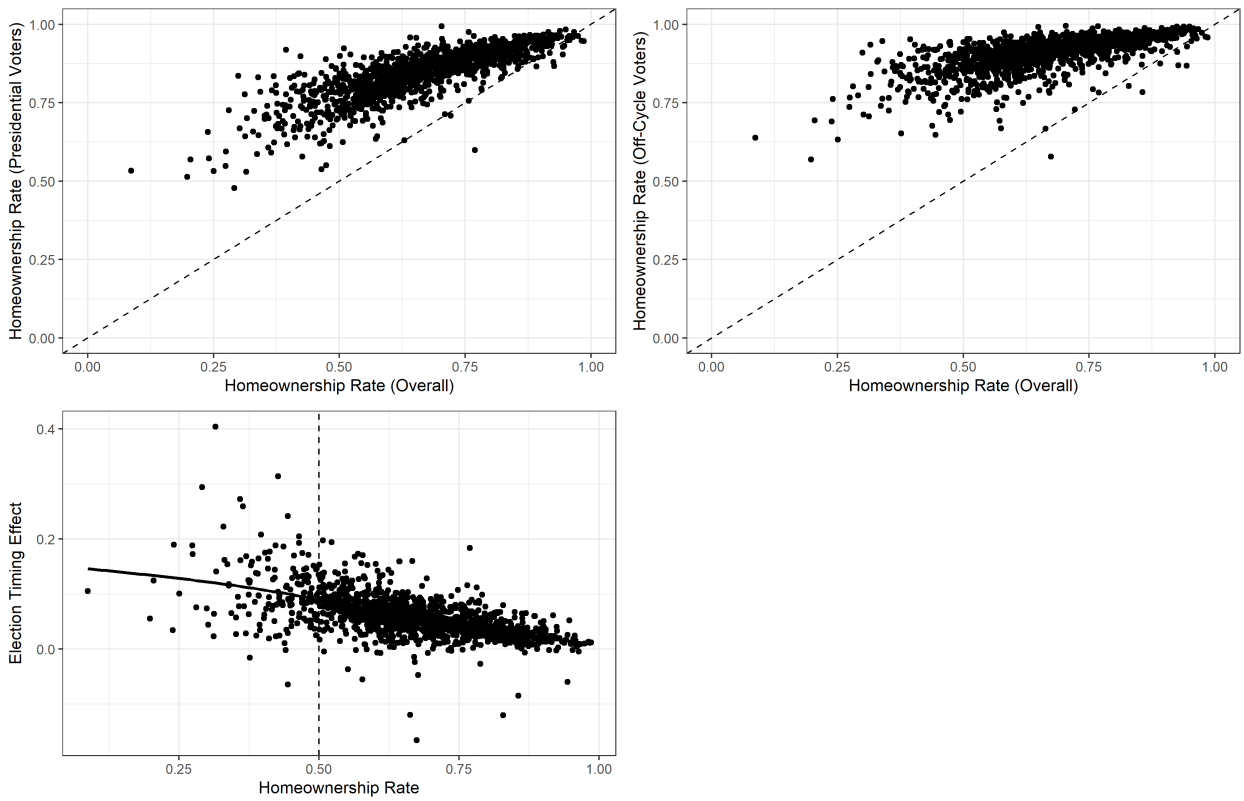

Figure 2: Comparing the share of homeowners in the population, the presidential electorate, and off-cycle municipal electorate. Each point is a city, and the top two panels plot a city's overall homeownership rate against the share of voters who own homes in the most recent presidential election (2016) and most recent off-cycle election.

share of voters is 85%. This is to be expected, since homeowners tend to be older, wealthier, and whiter, all characteristics correlated with voter turnout, and there is some evidence that homeownership has a causal effect on political participation (Hall & Yoder 2018).

In off-cycle elections (top-right panel), the over-representation of homeowners is particularly striking. On average, the share of homeowners increases by 6% between on-cycle and off-cycle elections. In nearly every city that I study, homeowners form a three-quarters supermajority of the off-cycle electorate, but in only 25% of those cities do homeowners form a three-quarters supermajority of the population as a whole.

As the bottom-left panel of Figure 2 illustrates, the election timing effect is not homogeneous. The effect is smallest for cities made predominantly of homeowners, and largest

Table 1: Characteristics of electorate averaged across cities in my dataset, varying election timing.

	Presidential	Off-Cycle	Election Timing Effect (95% CI)
% Renters	0.157	0.097	[-0.065, -0.055]
Median Age	55	63	[7.8, 8.5]
% Black	0.054	0.047	[-0.016, 0.003]
Median Property Value	$192,464	$189,015	[-7776, 14672]

(10-15%) in cities that are majority renter. This result is largely intuitive; in places with very few renters, election timing cannot cause a large increase in the share of renters who turn out to vote. In the Supplementary Materials, I develop a model to formalize this intuition. In particular, the model predicts an asymmetric treatment effect curve, which explains why the effect is largest in cities that are slightly majority-renter.

In addition to an increased share of homeowners, the off-cycle electorate is roughly 8 years older on average, replicating the results from Kogan et al. (2017). I find no evidence that the off-cycle electorate is wealthier (in terms of property value) and find no systematic differences in racial composition.[11] Table 1 reports these results.

5.2 Ballot Initiatives

Turning now to the ballot initiatives data, I find some evidence that voters are more likely to reject proposed housing development when the referendum is held off-cycle. Table 2 reports the coefficients from an OLS regression, controlling for city-level characteristics, metropolitan area fixed effects, and interacting the type of initiative with the election timing. The results suggest that initiatives to block urban sprawl are highly popular in California. Of the ballot measures analyzed, the share of residents voting to enact or renew Urban Growth Boundaries averaged 60%, regardless of election timing. Initiatives to permit new infill

[11] There are a few notable exceptions, including Ferguson, Missouri, where there was a 14% shift in the share of black voters between the most recent presidential and city council elections (the former was 45% white, the latter 60% white). Perhaps this reflects the city's recent demographic shift from majority-white to majority-black.

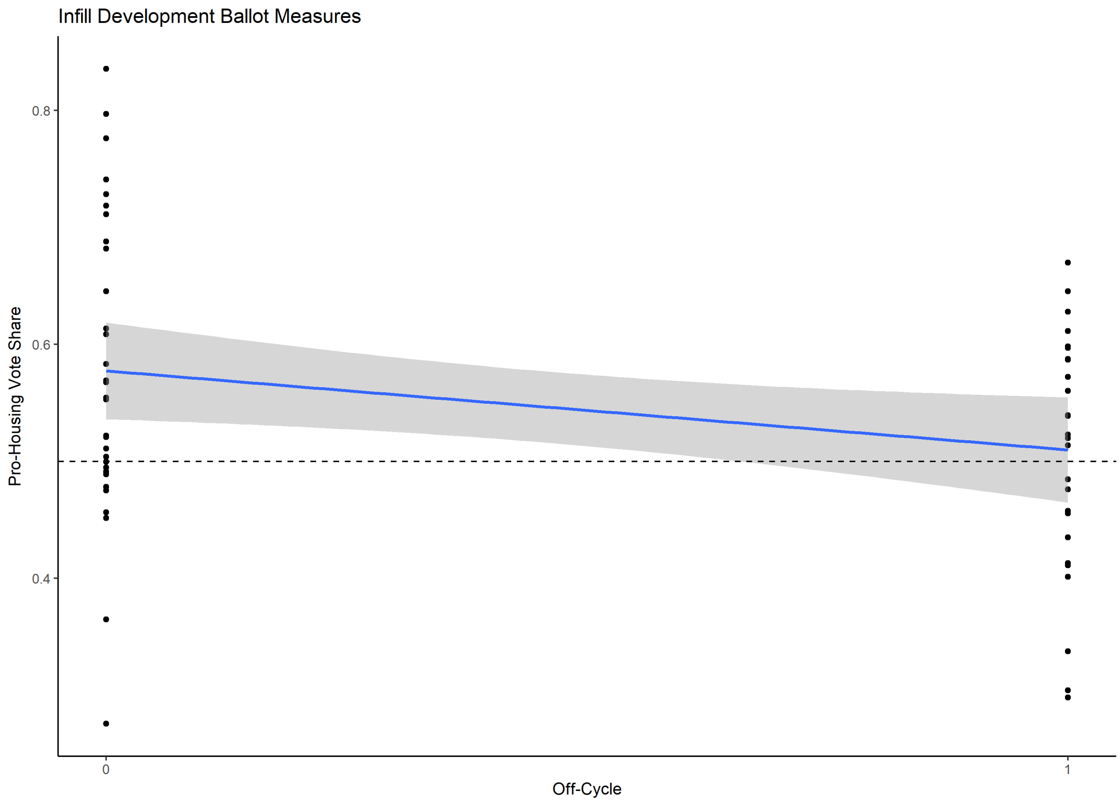

Figure 3: New infill development attracts roughly 7-8 percentage points less support when the ballot initiative is held off-cycle.

development were significantly less popular, and their success depended on election timing. Figure 3 illustrates the vote share garnered by the pro-housing side of these initiatives, broken down by election timing. Among infill development initiatives, the pro-housing side received roughly 7 percentage points more support when the election was held on-cycle.

All of this tentatively suggests that off-cycle voters are less likely to support new development that intensifies land use within existing neighborhoods. In the land-constrained cities on the California coast, where any new housing development is necessarily infill development, this eliminates any avenues for new housing construction entirely.

Table 2: Relationship between election timing and success of pro-housing ballot initiatives, by type of development. City-level controls include mean temperature, log population (2000), median income, pct. white, pct. over 65, pct. college graduates, pct. nearby developable land area (2001), school district Academic Performance Index (2003), and debt per capita (2002).

	Dependent variable:		
	Percent Pro-Housing		
	(1)	(2)	(3)
Off-Cycle	0.02	0.02	0.04
	(0.03)	(0.03)	(0.03)
Infill	0.19^{***}	0.20^{***}	0.15^{***}
	(0.04)	(0.03)	(0.04)
Off-Cycle * Infill	-0.09	-0.10	-0.12^{*}
	(0.05)	(0.05)	(0.05)
CBSA Fixed Effects	No	Yes	Yes
City-Level Controls	No	No	Yes
Observations	216	200	194
R^2	0.17	0.27	0.36
Note:	*p<0.05; **p<0.01; ***p<0.005		

5.3 Cross-Sectional Correlations: OLS

Next, I examine the relationship between election timing, new residential building permits, and median home prices. 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 in 2014 or the logarithm of new units permitted by city i between 2000 and 2016. The variable T_i is the percentage of elections in city i held off-cycle between 1996 and 2016, X_i is a matrix of city-level covariates, and ε_i is an iid error term.

As reported in Tables 3 and 4, the estimated relationship between off-cycle election timing and building permits is negative across all specifications of the model. The magnitude of the effect is striking: the estimate reported in Column (4) suggests that off-cycle cities issued between half and two-thirds as many building permits between 2000 and 2016 as comparable cities with on-cycle elections. A similar pattern shows up in the median home price regressions (Table 5). Median home prices are roughly $61 higher per square foot in cities with off-cycle elections.

5.4 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. I define the "treatment" group as those cities with a majority of city council elections between 1996 and 2016 held off-cycle, and all other cities as the control group. Dichotomizing the treatment in this manner is not terribly problematic, since most cities in my sample hold either 100% or 0% of their elections off-cycle. As before, I include as covariates each city's median income, population, nearby developable land, per capita debt burden, and the percentage of residents that are white, college-educated, and over 65 years of age as covariates. I also perform an exact match on metropolitan statistical area, so that each treated city is compared to a matched control city within the same CBSA.[12]

The two groups are well-balanced on the matching covariates, as indicated by the Kolmogorov-Smirnoff statistics in the second half of Tables 6 through 8. For each outcome variable, I

[12]In all specifications, I identify the matched control group using Diamond & Sekhon's Genetic Matching algorithm (Diamond & Sekhon 2012), courtesy of the `Matching` package in `R` (Sekhon 2011). Owing to the heavily right-skewed city size distribution, I drop three cities with population greater than 500,000.

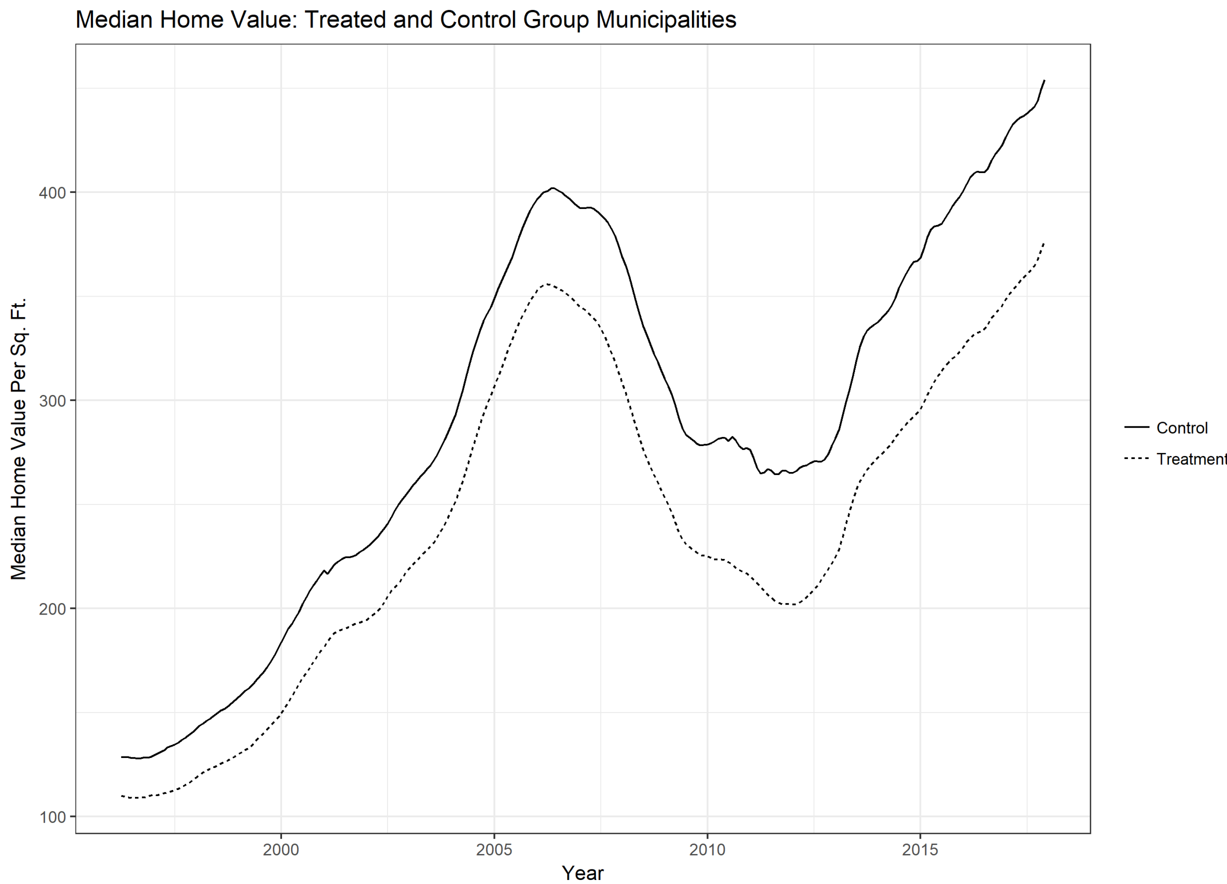

Figure 4: Median real home prices grew more slowly in cities that moved their city council elections on-cycle than in comparable cities that did not.

compute the average treatment effect on the treated units (ATT). These estimates are similar to those from the OLS: the median home value in treated cities is roughly $75 higher per square foot than in control cities, and they issued half as many building permits.

5.5 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 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, demographics, developable land, and per capita debt burden.[13] I perform a similar analysis for the growth of newly permitted housing stock.

In total, I identify 27 cities that shifted their election timing from off-cycle to on-cycle during the period of study. As illustrated in Figure 1, these cities are located throughout the state, although a plurality are within or around the San Francisco metropolitan area. Their mean population is roughly 55,000, median income is on average $55,000, and roughly 30% of their population is college educated. These and other covariate balance statistics are listed in Table 9.

The cities that shifted their election timing are broadly similar to the cities that did not, with three notable exceptions. First, they tend to have a greater share of nearby developable land (26% compared to 9%). Second, they tend to have a larger percentage of white residents (54% and 44%, respectively). And finally, they hold more municipal debt per capita ($2000 compared to $1400). Because each of these characteristics may affect the price and growth of the housing stock, I opt for the more conservative approach of creating a matched control group prior to estimating the difference-in-difference. Post-match, there are no significant differences between the groups, as measured by a Kolmogorov-Smirnoff statistic.

Figures 4 and 5 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 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

[13]This matching is not strictly necessary for a difference-in-difference analysis as long as one assumes that the potential outcomes in both groups follow "parallel trends". However, I doubt the parallel trends assumption holds in this case. The late-2000s housing bubble and collapse affected different parts of California more severely than others, so if the treatment and control groups are not balanced on regional characteristics, it is unlikely that their home values and new building would have followed similar trajectories. The parallel trends assumption is more plausible after matching on these observed covariates, so one could consider this test even more conservative than a standard difference-in-difference. See Abadie (2005) for a detailed discussion of semi-parametric difference-in-difference estimators.

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.

As Figure 5 reveals, the largest gap in new homebuilding occurred in the period prior to the Great Recession (2000-2007). This accords with intuition, but it is striking how much steeper that line during this period is for the cities that switched to on-cycle elections. Homebuilding in the control municipalities increases only slightly during this period, while in the treatment group, the housing stock expands nearly 5% each year, before converging with the control group by 2009. Nearly all of the difference in new housing stock between the two groups came about during that period.[14] In Table 9, I report the estimates, balance statistics, and measures of uncertainty. Median home value per square foot grew, on average, by $17 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 2000 and 2016 (about 4,300 new units per city on average).

6 Conclusion

The debate over urban land use policy is often framed as a choice between local autonomy and broader economic efficiency. Should cities be compelled to permit more housing in order to benefit people that do not currently live there, but would like to? Or do the current residents have a right to determine for themselves the density and character of their own community?

The results I present in this paper suggest that neither normative benchmark is currently being met. Because most cities hold their elections off-cycle, the selection of officials who determine land use policy is largely dominated by a small, unrepresentative electorate with preferences for slow growth. As a result, the equilibrium housing policy reflects neither the

[14]Nine of the cities in the treatment group switched their election timing on or after 2010, too late to have explained this pattern. However, the difference-in-difference estimate is robust to dropping those observations.

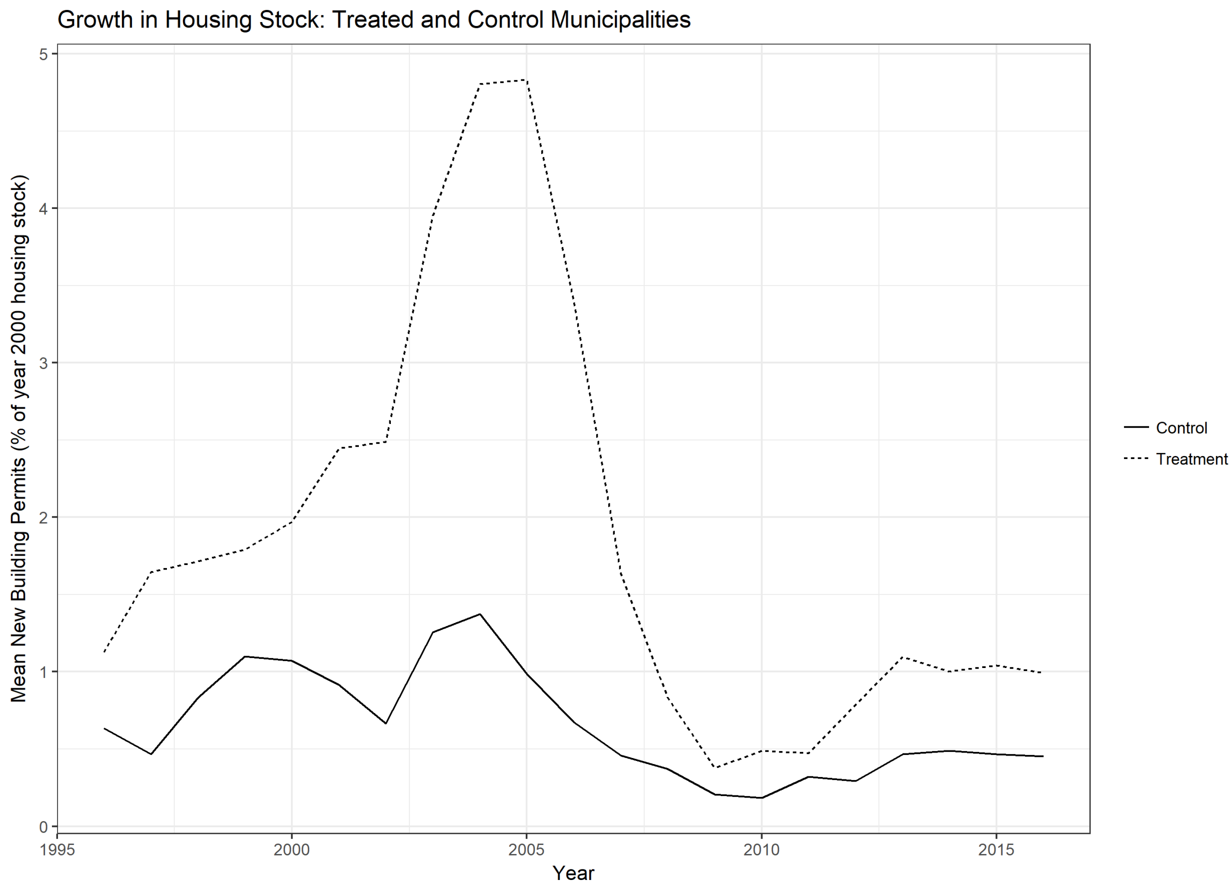

Figure 5: Compared to cities that kept their elections off-cycle, cities that shifted to on-cycle elections issued permits for roughly four times as many new housing units between 1996 and 2015.

desires of each city's median voter, nor the optimal growth of the housing supply that a benevolent urban planner would pursue. The broader trends we have observed in the US housing market – limited supply elasticity and rapidly rising home costs – are partly a result of this institutional constraint.

There are a number of directions in which I hope to expand this study in future work. Although I have done what I could to alleviate endogeneity concerns, the fact remains that my sample consists of cities that self-selected into their institutional rules. An interesting avenue for future research would be to identify cities where election timing is assigned exogenously (e.g. by state-level mandate). Fortunately, we've recently observed such an exogenous treatment assignment. In September 2015, California passed SB 415, a law requiring that lower-level governments hold their elections currently with statewide elections by November 2022 – if off-cycle elections attract 25% lower voter turnout than the average on-cycle election. Over the next several years, we should begin to see how this discontinuous shock to election timing affects municipal-level public policy. Readers are encouraged to remind me to write a follow-up paper in 2028.

Despite such limitations, the evidence presented here provides a compelling glimpse at yet another significant 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 broad welfare gains.

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Appendix 1: A Formal Model

Suppose there are two groups of voters in a jurisdiction: Group A (homeowners) and Group B (renters). When elections are held on-cycle, members of Group A will turn out to vote with probability α, and members of Group B will turn out to vote with probability β. When held off-cycle, these probabilities are α' and β', respectively. Let θ denote the fraction of the population that belongs to Group A. Given this setup, we can derive three empirical implications.

Proposition 1 *If members of Group A are proportionately more likely to vote in off-cycle elections than members of Group B (e.g.* $\frac{\alpha'}{\alpha} > \frac{\beta'}{\beta}$*), then the share of the electorate from Group A will be larger in off-cycle elections.*

This first proposition is rather intuitive, but note that the necessary assumptions are actually quite weak. We do not need to assume that Group A is strictly more likely to turnout than Group B ($\alpha' > \beta'$), nor do we need to assume that on-cycle elections yield higher turnout ($\beta > \beta'$, $\alpha > \alpha'$).

Proof: When elections are held on-cycle, the share of the electorate from Group A will be:

$$\frac{\theta\alpha}{\theta\alpha + (1-\theta)\beta}$$

And when elections are held off-cycle, the share from Group A is:

$$\frac{\theta\alpha'}{\theta\alpha' + (1-\theta)\beta'}$$

So the effect of holding elections off-cycle is the difference between those two expressions.

$$E = \frac{\theta\alpha'}{\theta\alpha' + (1-\theta)\beta'} - \frac{\theta\alpha}{\theta\alpha + (1-\theta)\beta} \tag{1}$$

Setting $E > 0$ and solving yields the necessary condition.

$$\theta^2\alpha'\alpha + \theta(1-\theta)\alpha'\beta > \theta^2\alpha'\alpha + \theta(1-\theta)\alpha\beta'$$

$$\alpha'\beta > \alpha\beta'$$

$$\frac{\alpha'}{\alpha} > \frac{\beta'}{\beta} \tag{2}$$

Proposition 2 *The magnitude of the election timing effect is nonlinear in θ. It is smallest when θ equals 0 or 1, and largest for intermediate values.*

Intuitively, if a jurisdiction consists either of 0% or 100% homeowners, then election timing will never affect the share of homeowners in the electorate (it will always be 0% or 100%). It is only at intermediate values of θ that election timing will have any effect on the composition of the electorate.

Proof: Taking the first order condition of (1) with respect to θ yields

$$\frac{\partial E}{\partial \theta} = \frac{\alpha'\beta'}{[\theta\alpha' + (1-\theta)\beta']^2} - \frac{\alpha\beta}{[\theta\alpha + (1-\theta)\beta]^2} = 0 \tag{3}$$

Solving yields

$$\frac{\theta^2}{(1-\theta)^2} = \frac{\beta\beta'}{\alpha\alpha'}$$

$$\theta^* = \frac{\sqrt{\beta\beta'}}{\sqrt{\beta\beta'} + \sqrt{\alpha\alpha'}} \tag{4}$$

If condition (2) holds, then this value is a maximum. This follows because when $\theta = \{0, 1\}$, $E = 0$. For all other values of θ, $E > 0$.

Proposition 3 *If* $\alpha' > \beta'$, *then* $\theta^* < 0.5$.

Proof: Equation (4) implies that $\theta^* < 0.5$ when $\beta\beta' < \alpha\alpha'$. Combining this condition with equation (2) yields $\alpha' > \beta'$, completing the proof.

This final proposition suggests that the election timing effect will be largest when Group A (e.g. homeowners) are in the minority, which is precisely the result I find in the empirical analysis.

Appendix 2: Alternative Record Linkage Procedure

To generate the results presented in the text, I performed an imputation to predict the homeownership status of voters who could not be linked with property data from CoreLogic. However, the results are robust to a more conservative coding strategy, where I code each voter that failed to merge as a renter. The results from this analysis are presented in Figure 6, analogous to Figure 2 in the text.

As before, homeowners are significantly over-represented in both presidential and off-cycle elections, although the magnitude of the effect is smaller. Compared to the overall share of renters (34%), the median share of renters in presidential elections is 22%, and in off-cycle elections is 15%. Also as before, the effect of off-cycle election timing on homeowner turnout is largest for cities with a mix of homeowners and renters, and is as large as 15% for cities with majority renters.

Appendix 3: Spatial Econometric Tests

In the foregoing analysis, I have assumed that median home prices in one city are statistically independent of home prices in neighboring jurisdictions. This is a heroic assumption. Because homebuyers are not constrained by buy homes within a single municipality, factors

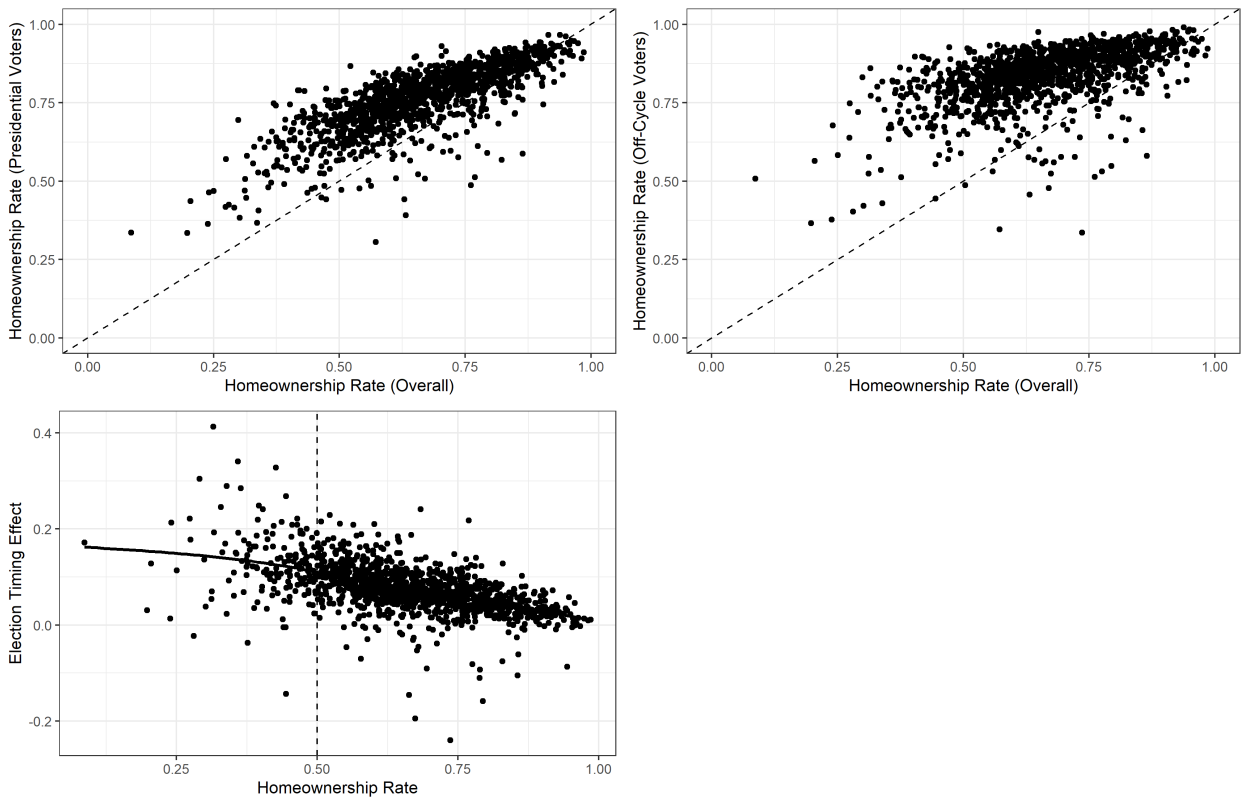

Figure 6: Homeowner turnout in presidential and off-cycle elections (conservative record linkage procedure).

that affect the price of housing in one city are likely to affect nearby municipalities as well. As a result, land use policies are likely to exhibit spillover effects. A supply restriction in one city can increase home prices throughout the metropolitan area.

Fortunately, these spillover effects are likely to bias *against* my hypothesis. If off-cycle elections cause City A to enact restrictive zoning, which increases home prices in both City A and neighboring City B, then I should be more likely to observe a null result when comparing home prices within a metro area. Nevertheless, it is a useful robustness test to explicitly model the spillover effect between jurisdictions, and see if it alters my substantive conclusion. To do so, I model home prices with a spatial autoregressive lag model, as follows:

$$Y_i = \rho WY + \beta X_i + \varepsilon_i$$

where Y is a vector of median home values and W is a spatial weights matrix, with each W_{ij} containing a measure of "closeness" between city i and j. In the following analysis, I populate the W matrix using the inverse distance between the centroids of each pair of municipalities (Column 1) and a 50km threshold (Column 2).[15] A positive ρ implies that median home values are positively correlated across space, holding X_i constant. In the presence of such autocorrelation, omitting the ρWY term would bias the estimates of β. Table 10 reports the coefficient estimates from this model; despite the addition of the spatial lag term, the estimated coefficient on Off-Cycle elections remains significant. Bear in mind that the β coefficient reported here is not, as in an OLS, equivalent to the estimated effect size. Rather, one can think of it as the "pre-spatial feedback" impulse, analogous to a coefficient estimate in a lagged-dependent variable time series model (Franzese & Hays 2007).

Appendix 3: Heterogeneous Treatment Effects

The effect of off-cycle election timing may vary depending on context. For example, new single family developments may provoke less political opposition in off-year elections than multifamily housing. As the ballot initiative results suggest, urban sprawl restrictions are quite popular, but support for new infill developments varies with election timing. To test this hypothesis, I recompute the cross-sectional regression analysis separately for single family and multifamily housing. As Figure 7 shows, the estimated effect of election timing is slightly stronger for multifamily housing than for single-family housing, but this difference is not statistically significant. Note that 24% of the municipalities in my dataset permitted zero multifamily units between 2010-2016, so I drop those observations when multifamily permits are the dependent variable below.

[15] I have also estimated the model using a threshold distance matrix, spatial contiguity matrix, and a shared-CBSA matrix, without meaningfully altering the results.

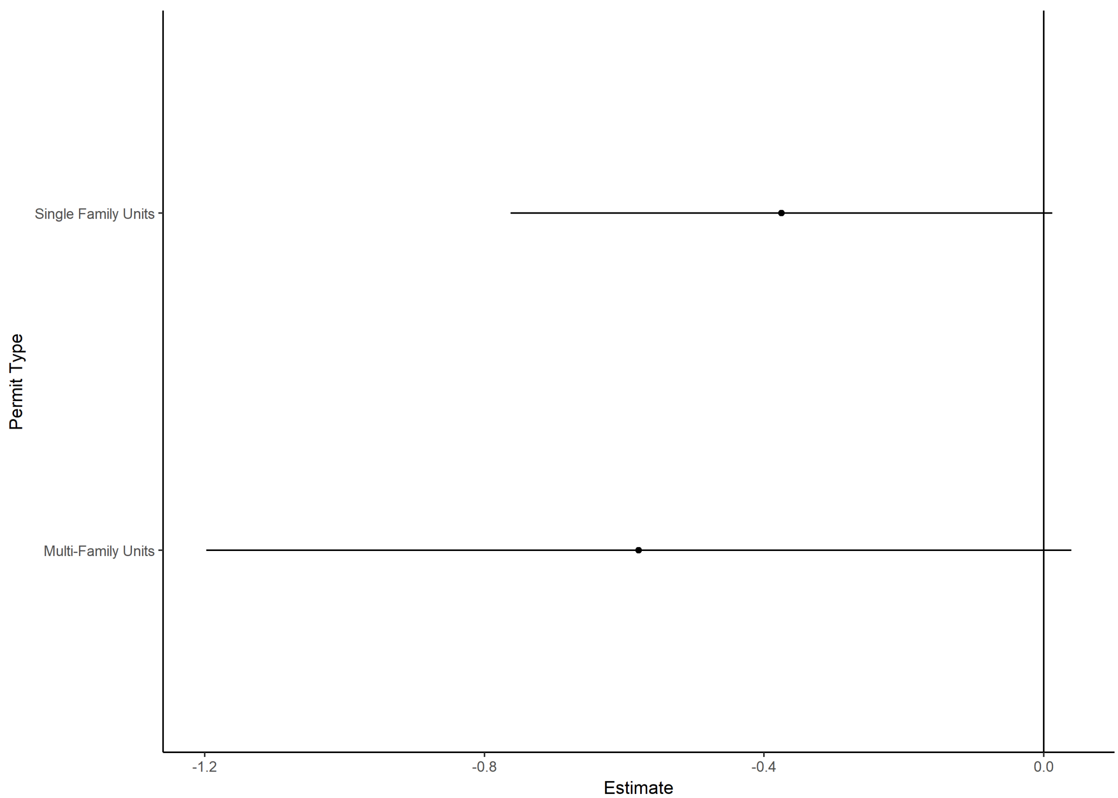

Figure 7: Estimated effect of off-cycle elections on log new building permits (2000-2016), by type of housing.

Table 3: Estimated OLS coefficients and standard errors, regressing log new building permits (2000-2016) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:		
	Log Permits (2000-2016)		
	(1)	(2)	(3)
Pct. Off-Cycle	-1.17^{***}	-0.55^{***}	-0.36^{*}
	(0.18)	(0.16)	(0.17)
Log Population	0.94^{***}	1.07^{***}	1.06^{***}
	(0.06)	(0.05)	(0.05)
Median Income		0.00	0.00
		(0.00)	(0.00)
January Median Temp.		-0.04	-0.06
		(0.02)	(0.04)
July Median Temp.		0.04^{***}	0.06^{**}
		(0.01)	(0.02)
Pct. White		1.20^{***}	1.17^{*}
		(0.42)	(0.48)
Pct. Over 65		-0.93	-2.44
		(1.64)	(1.67)
Pct. College Grad		-0.40	0.16
		(0.78)	(0.84)
Debt Per Capita (2002)		0.25^{***}	0.24^{***}
		(0.04)	(0.04)
Pct. Developable (2001)		2.25^{***}	2.36^{***}
		(0.43)	(0.57)
Academic Performance Index (2003)		0.001	0.001
		(0.001)	(0.001)
Constant	-3.29^{***}	-9.51^{***}	-9.97^{***}
	(0.95)	(1.48)	(2.76)
CBSA Fixed Effects	No	No	Yes
Observations	330	324	317
R^2	0.44	0.69	0.74
Note:	$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.005$		

Table 4: Estimated OLS coefficients and standard errors, regressing log new building permits (2010-2016) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:			
	Log Permits (2010-2016)			
	(1)	(2)	(3)	(4)
Pct. Off-Cycle	-1.02^{***}	-0.79^{***}	-0.71^{***}	-0.52^{*}
	(0.19)	(0.19)	(0.21)	(0.23)
Log Population	1.15^{***}	1.20^{***}	1.19^{***}	1.21^{***}
	(0.06)	(0.06)	(0.06)	(0.08)
Median Income		0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)
January Median Temp.		-0.01	-0.01	0.01
		(0.02)	(0.04)	(0.05)
July Median Temp.		0.02	0.07^{*}	0.08^{*}
		(0.02)	(0.03)	(0.03)
Pct. White		0.35	0.56	0.39
		(0.50)	(0.62)	(0.72)
Pct. Over 65		-4.27^{*}	-5.31^{*}	-8.97^{***}
		(2.06)	(2.13)	(2.62)
Pct. College Grad		3.01^{***}	2.53^{*}	4.71^{***}
		(1.00)	(1.08)	(1.45)
Debt Per Capita		0.14^{***}	0.14^{***}	0.17^{***}
		(0.03)	(0.03)	(0.05)
Pct. Developable		1.76^{***}	1.75^{*}	2.04^{*}
		(0.50)	(0.72)	(0.86)
Academic Performance Index		0.002	0.003	0.002
		(0.002)	(0.002)	(0.002)
Ideology Score				1.19
				(0.62)
Constant	-8.95^{***}	-13.84^{***}	-18.03^{***}	-19.03^{***}
	(0.97)	(1.89)	(3.55)	(3.97)
CBSA Fixed Effects	No	No	Yes	Yes
Observations	358	351	342	266
R^2	0.50	0.61	0.65	0.66
Note:		$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.005$		

Table 5: Estimated OLS coefficients and standard errors, regressing median home value per sqft (2017) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:			
	Median Home Value Per Sqft (2017)			
	(1)	(2)	(3)	(4)
Pct. Off-Cycle	150.45***	99.97***	69.35***	61.47**
	(30.09)	(19.84)	(19.19)	(21.75)
Log Population		−2.27	−13.21*	−16.73*
		(6.29)	(5.71)	(7.69)
Median Income		0.005***	−0.0002	0.001
		(0.0003)	(0.001)	(0.001)
January Median Temp.		7.40***	14.90***	11.10*
		(1.87)	(4.08)	(4.50)
July Median Temp.		−14.82***	−13.34***	−14.30***
		(1.35)	(2.65)	(3.07)
Pct. White			−24.49	−45.71
			(55.26)	(66.87)
Pct. Over 65			−85.06	272.87
			(144.84)	(243.07)
Pct. College Grad			664.51***	525.85***
			(97.42)	(134.87)
Debt Per Capita			−1.86	5.27
			(2.88)	(4.26)
Pct. Developable			−15.78	−25.97
			(65.87)	(80.04)
Academic Performance Index			0.24	0.23
			(0.17)	(0.20)
Ideology Score				−162.25**
				(57.92)
Constant	343.19***	780.31***	536.64	816.69*
	(17.40)	(162.42)	(323.94)	(369.62)
CBSA Fixed Effects	No	No	Yes	Yes
Observations	362	361	338	264
R^2	0.06	0.63	0.79	0.80
Note:				*p<0.05; **p<0.01; ***p<0.005

Table 6: Matching Analysis (Home Values): Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Median Home Value (per sqft)	499.6	423.8	75.8	0.0003
Number of Cities	126	67		
	Mean, Treatment	Mean, Control	K-S Statistic	K-S Bootstrap p-value
Balance Statistics				
Median Income	73,243	72,605	0.119	0.314
Population (2010)	71,608	70,842	0.142	0.118
Jan. Mean Temp	52.38	52.02	0.158	0.052
Jul. Mean Temp	72.71	72.39	0.134	0.126
Pct. White (2010)	0.38	0.39	0.174	0.046
Pct. College Grad	0.34	0.32	0.159	0.06
Pct. Over 65	0.124	0.122	0.087	0.666
Academic Performance Index	793.6	803.7	0.190	0.012
Pct. Developable (2011)	0.131	0.147	0.214	<2e-16
Debt Per Capita (2007)	2.23	1.84	0.134	0.148

Table 7: Matching Analysis: Building Permits (2010-2016). Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Log Permits (2010-2016)	7.91	8.56	−0.65	0.025
Number of Cities	127	67		
	Mean, Treatment	Mean, Control	K-S Statistic	K-S Bootstrap p-value
Balance Statistics				
Median Income	73,032	72,046	0.118	0.298
Population (2010)	71,208	71,531	0.150	0.1
Jan. Mean Temp	52.4	52.2	0.157	0.098
Jul. Mean Temp	72.9	72.6	0.126	0.226
Pct. White (2010)	0.38	0.39	0.173	0.026
Pct. College Grad	0.34	0.32	0.150	0.114
Pct. Over 65	0.124	0.122	0.087	0.656
Academic Performance Index	793	801	0.181	0.03
Pct. Developable (2011)	0.137	0.159	0.204	0.008
Debt Per Capita (2007)	2.23	1.91	0.118	0.292

Table 8: Matching Analysis: Building Permits (2000-2016). Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Log Permits (2000-2016)	10.11	10.67	−0.56	0.014
Number of Cities	124	69		
	Mean, Treatment	Mean, Control	K-S Statistic	K-S Bootstrap p-value
Balance Statistics				
Median Income	55,604	55,947	0.144	0.154
Population (2000)	68,082	64,230	0.096	0.566
Pct. White (2000)	0.44	0.45	0.12	0.292
Pct. College Grad	0.30	0.30	0.112	0.38
Pct. Over 65	0.111	0.111	0.088	0.638
Pct. Developable (2001)	0.148	0.165	0.272	<2e-16
Debt Per Capita (2002)	1.41	1.46	0.144	0.134

Table 9: Difference-in-difference estimates, comparing cities that switched to on-cycle elections (treatment) and those that remained off-cycle (control).

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Δ Median Value per Sq. Ft. (2002-2014)	78.2	95.8	-17.6	0.026
New Units Permitted (2000-2016)	4,300	2,545	1,755	0.024
Number of Cities	27	27		
	Mean, Treatment	**Mean, Control**	**K-S Statistic**	**K-S Bootstrap p-value**
Balance Statistics				
Median Income (2000)	56,392	54,395	0.222	0.484
Population (2000)	53,187	55,053	0.148	0.89
Mean Jan. Temp	50.6	50.9	0.185	0.658
Mean Jul. Temp	74.0	73.0	0.222	0.472
% White (2000)	50.0	56.2	0.222	0.482
% College Grad (2000)	28.7	32.1	0.222	0.48
% Over 65 (2000)	12.0	13.0	0.296	0.174
API (2003)	696	706	0.222	0.436
% Developable (2001)	22.4	18.4	0.259	0.282
Debt Per Capita (2002)	2,655	1,858	0.296	0.168

Table 10: Estimated coefficients estimates from the spatial autoregressive lag model.

	Dependent variable:	
	median.hv.sqft.2017	
	(1)	(2)
Pct. Off-Cycle	35.83^{**}	44.14^{***}
	(14.23)	(16.66)
Log Population	-13.68^{***}	-17.19^{***}
	(3.77)	(4.35)
Median Income	0.001^{***}	0.001^{**}
	(0.0003)	(0.0004)
January Median Temp.	7.85^{***}	4.55^{**}
	(1.36)	(1.99)
July Median Temp.	-4.32^{***}	-3.95^{***}
	(1.06)	(1.52)
Pct. White	-74.88^{**}	-95.90^{**}
	(36.00)	(42.24)
Pct. Over 65	86.16	104.99
	(103.89)	(119.92)
Pct. College Grad	605.91^{***}	738.92^{***}
	(69.27)	(78.63)
Debt Per Capita	-1.16^{**}	-1.11^{**}
	(0.46)	(0.51)
Pct. Developable Land	-1.69	80.15^{**}
	(16.96)	(40.48)
Test Scores	0.02	-0.10
	(0.13)	(0.14)
ρ	0.97^{***}	0.52^{***}
	(0.02)	(0.05)
Observations	412	405
LR Test (df = 1)	165.44^{***}	75.41^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01	